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'Current' Events: Galvanism and the World of Scientific Information, 1790-1830

Watts, Iain P.

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“CURRENT” EVENTS: GALVANISM AND THE WORLD OF SCIENTIFIC INFORMATION, 1790–1830.

IAIN P. WATTS

A DISSERTATION

PRESENTED TO THE FACULTY

OF PRINCETON UNIVERSITY

IN CANDIDACY FOR THE DEGREE

OF DOCTOR OF PHILOSOPHY

RECOMMENDED FOR ACCEPTANCE

BY THE DEPARTMENT OF HISTORY

Advisor: Michael D. Gordin

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Abstract

This dissertation uses a close study of the science of galvanism to reconstruct the movement of scientific information during and after the Napoleonic Wars. Galvanism—a science concerned with electricity, matter, and life—was one of the most dynamic and popular fields for both specialists and laypeople during 1790–1830. I argue that galvanism was a crucial sphere of activity driving a broader transformation in European scientific communications. Galvanism's practitioners self-consciously envisioned their science as a fast-paced race for new experimental discoveries. This went hand in hand with a new role for "information" (in its early-nineteenth-century sense of up-to-date intelligence or news) in the making of scientific knowledge. By following information about galvanism across wartime frontiers—in newspapers, journals, letters, and the records of conversations—I demonstrate how the struggle to maintain a cross-border flow of experimental results during the disruptions and blockades of the Napoleonic era forced scientific practitioners to innovate in how they kept in touch across distance. Ranging across Western Europe and North America, and with a particular focus on Britain, I combine macro-level analysis of communication systems with close studies of how information on the move was woven into the social dynamics of individual scientific careers, and the technical content of laboratory work.

The first chapter uses the spread of Alessandro Volta's galvanic battery in 1800 to introduce the fluid and hybrid media world of scientific information at the beginning of the nineteenth century, and its distinctive practice: reprinting. Chapters two, three, and four each continue the story of galvanism while focusing on individual forms of communication. Chapter two examines the increasing presence of science in daily newspapers. Chapter three looks at the new monthly scientific journals that appeared around 1800, showing how they brought periodical

print into the day-to-day practice of experimental investigation. Chapter four looks at letters, using the archive of Sir Charles Blagden at the Royal Society to reconstruct correspondence between London and Paris during Napoleon's Continental System blockade. Chapter five discusses the end of the wars and surveys the landscape of scientific communication that emerged after the coming of peace in 1815.

Acknowledgments

Communication between scholars is now rather easier than it was during the Napoleonic wars, and there is a lot more of it, but that activity is often invisible in the text and footnotes of finished work. Here I can recognize debts inexpressible in a mere citation. This dissertation is the product of countless interactions, discussions, and—yes—exchanges of information, sometimes face-to-face across a seminar table or in a bustling conference hotel, and sometimes via an email from thousands of miles away. I'm happy I can now offer the results back, in provisional form at least, to the scholarly community that has given me so much, and so express my deepest gratitude to everyone who took time to give me comments, advice, or a helping hand along the way.

My first thanks goes to the exceptional group of historians on my dissertation committee. All of them, through their feedback on my work, and by the example of their own scholarship, have taught me to think more broadly and deeply about history, and to make new kinds of connections between people, places, objects, and ideas across the landscape of the past.

Michael Gordin's superlative talents as a dissertation advisor are well known to the growing band of Ph.D.s he has steered through Princeton. I'll always count myself wonderfully fortunate to have benefited from his perceptive and rigorous questioning, and his fine sense of historical craftsmanship. He invariably read and returned drafts of my chapters with a speed and attention to detail that suggests immunity to the usual laws of space and time, and supplied a mass of astute comments pressing me on everything from the big picture stakes down to my choice of individual words. The dissertation is immensely better as a result. Michael: thank you.

Linda Colley guided me in my mission to train as a historian of modern Britain as well as a historian of science. She always pushed me to consider the wider social and geopolitical context of the story I tell in these pages, helped me to clarify and sharpen my prose, and gave me the confidence to put the connections—and disconnections—between Britain and Continental

Europe at the center of my analysis. Anthony Grafton introduced me to the world of early modern learned communication in his memorable seminar on the Republic of Letters, and so got me thinking seriously about a question I'm still wrestling with: how to relate the developments I trace here to the history of intellectual communities over the longue durée. I'm very grateful that he's offered the benefit of his unparalleled knowledge of the centuries of scholarly exchanges preceding the people I study. Jan Golinski is more familiar than most with the individual characters and scientific debates that populate the following pages, which made him an ideal choice for my external reader. I thank him for generously sharing his expertise on Humphry Davy, galvanism, and the history of chemistry during the last several years.

I've also been fortunate in the guidance of a remarkable group of mentors beyond my dissertation committee. Tom Broman has always instinctively grasped what I have been trying to do with this project, even in the early stages when I myself wasn't entirely sure. His enthusiasm during yearly freewheeling discussions at History of Science Society meetings always meant that I returned home with new energy and fresh ideas. Robert Fox has been a mentor and friend since we both held fellowships at the Chemical Heritage Foundation in Philadelphia, and he encouraged my ventures into the history of French science. Simon Schaffer gave me a dazzling one-on-one workshop on two of my chapters over a pint in Cambridge. As my M.Sc. supervisor, Hasok Chang guided my first research on early-nineteenth-century science. In the years since, I've been immensely fortunate to benefit from his philosopher's acumen and from our shared interest in the minute details of galvanic experiments. Alex Csiszar was an ideal interlocutor for numerous discussions about the history of scientific print. His work on the development of the scientific journal through the nineteenth century opened up many new questions for me about

how my own historical actors conceived of and used the periodical press, and dispatched some teleological specters that I would otherwise have expended much energy wrestling with myself.

Numerous other scholars generously gave me comments on drafts of chapters or welcomed me during my research trips abroad, including: Carin Berkowitz, Christine Blondel, Patricia Fara, Aileen Fyfe, Frank James, Elise Lipkowitz, Giuliano Pancaldi, Jim Secord, Alistair Sponsel, Emily Stanback, and Jonathan Topham. A collective thank you also to audiences at conferences, seminars, and workshops who provided a wealth of useful feedback after hearing talks or reading drafts of chapters: at the History of Science Society meetings, the International Congress of History of Science, Technology and Medicine, the Institute of Historical Research's British History in the Long Eighteenth Century Seminar and Director's Seminar, the Princeton-Harvard-MIT "Phunday" workshop in the history of the physical sciences, the Royal Society's "Publish or Perish" conference, the Society for the History of Authorship, Reading, and Publishing, and the writing workshop at the Chemical Heritage Foundation. I'm also happy to acknowledge that an article based on sections of Chapter 4 is forthcoming in *Isis*, and I thank the editor and anonymous referees for their useful comments on the manuscript.

But my most longstanding and valuable audience has been the community surrounding Princeton's Department of History, who collectively gave me an incomparable Ph.D. training in my discipline. A series of exceptional faculty members (in and out of the Department) taught me in seminars or gave me the benefit of incisive questions or comments when I presented my work: thanks especially to David Bell, David Cannadine, Angela Creager, Daniel Garber, Katja Guenther, Kevin Kruse, Erika Milam, Yair Mintzker, Susan Naquin, Philip Nord, Jennifer Rampling, Eileen Reeves, and Keith Wailoo. Past and present graduate students in the History of Science Program sharpened my thinking with questions simultaneously generous and rigorous,

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Archivists, librarians, and scholars across Europe and America all helped me track down the sources behind this dissertation. I'm particularly grateful to Keith Moore and all the staff of the Royal Society Library, who dealt swiftly and tolerantly with my endless requests for the Blagden Papers, and to Jane Harrison at the Royal Institution, where I spent many enjoyable hours immersed in Humphry Davy's manuscripts. Special thanks also to Stéphanie Deprouw-Augustin, who went above and beyond the call of scholarly generosity by mailing me, across the Atlantic, her only copy of her valuable unpublished master's thesis on the *prix du galvanisme*. The (ongoing) Davy Letters Project was launched just as I was beginning my dissertation, and I benefited greatly from its transcriptions of isolated letters in various far-flung archives: thank

you to Sharon Ruston and the other project team members. I was supported during my year in the archives by the Institute of Historical Research, London, which granted me a Mellon Fellowship for 2012-13; I am also grateful for the financial support of the Princeton Institute for International and Regional Studies, and Princeton's Graduate School and Department of History. Much of the writing was done while I was a Dissertation Fellow at the Chemical Heritage Foundation, Philadelphia, where Carin Berkowitz, Ronald Brashear, Rebecca Ortenberg, and my fellow Beckman Center Fellows made 2013-14 a wonderfully productive and collegial year.

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My parents and family have always encouraged my love of learning and offered support during the various twists and turns of my lengthy academic path. My sister Sarah and her partner Stephen, fellow enthusiasts for science both, always provide a warm welcome on visits home to Scotland. Their outdoor exploits are a source of inspiration and have persuaded me that I really should learn to use an ice axe one of these days. I definitely owe much of my interest in science, history, and electrical tinkering to my father Doyle, "man of science" himself; thanks to him and my uncle Gary Watts for helping with my recreation of the galvanic battery. My mother Jane gave me the incomparable gift of growing up in a house full of books in which there was always another classic novel ready for my bedside table. Her great love of reading meant that I was immersed in a world of stories from the beginning—surely this played no small part in my decision to read, write, and teach the stories drawn from the past's inexhaustible well.

Finally, Cate Reilly has shared in the making of these pages every step of the way. She knows exactly when to offer encouragement and—more important still—when to bring to bear her razor-sharp critical tools. Her careful and incisive readings have saved me from many a dodgy phrase or vague verbiage, and helped me approach a little closer yet to the elusive unobtainable *Ultima Thule* where the words really say exactly what the writer means. Coming from a neighboring humanities discipline, she has always seen things from a different perspective, with a literary subtlety that pushes me to question the assumptions and philosophy behind my work—even if I do usually plough ahead with my more rough-and-ready historian's approach to the interpretation of texts. In and out of Princeton, and on our travels and adventures from London to Berlin to Bologna's Piazza Galvani, she has been a constant source of love, support, and scholarly comradeship in our shared voyage through the sometimes turbulent waters of academic life. This dissertation is dedicated to her.

Abbreviations

Archives (for a full list of archives, see the Bibliography):

BL	British Library Manuscripts, London.
Blagden Diary	Charles Blagden's diary, CB/3/1-7, archives of the Royal Society, London.
RI	Archives of the Royal Institution, London.
RS	Archives of the Royal Society, London.
RS CB	Sir Charles Blagden Papers, archives of the Royal Society, London.

Journals:

AC	*Annales de Chimie.*
BB	*Bibliothèque Britannique (Sciences et Arts).*
BDS	*Bulletin des sciences, par la Société Philomathique de Paris.*
JP	*Journal de Physique.*
NJ	*A Journal of Natural Philosophy, Chemistry, and the Arts* ("*Nicholson's Journal*").
PM	*Philosophical Magazine.*

Published Collections of Primary Sources; Reference Works:

Banks Corr.	*The Scientific Correspondence of Sir Joseph Banks,* ed. Neil Chambers, 6 vols. (London: Pickering and Chatto, 2007).
Davy Works	*The Collected Works of Sir Humphry Davy,* ed. John Davy, 9 vols. (London: Smith, Elder, 1839-40).
DNB	*Oxford Dictionary of National Biography*, online version (http://www.oxforddnb.com/).
Faraday Corr.	*The Correspondence of Michael Faraday*, ed. Frank A. J. L. James, 6 vols. (London: IEE, 1991-2012).
Pictet Corr.	*Marc-Auguste Pictet, Correspondance, Science et Techniques,* ed. Jean Cassaigneau, Jean-Michel Pictet, Jean-Claude Pont, 4 vols. (Geneva: Slatkine, 1996-2004).
PVSA	*Procès-verbaux des séances de l'Academie tenues depuis la fondation de l'Institut jusqu'au mois d'août 1835*, 10 vols. (Hendaye Basses-Pyrénées: Imprimerie de l'Observatoire d'Abbadia, 1910-22).

"Yet something dead may get to live again,
Something with too much life or not enough,
Which, either way imperfect, ended once:
An end whereat man's impulse intervenes,
Makes new beginning, starts the dead alive,
Completes the incomplete and saves the thing.
…
For such man's feat is, in the due degree,
— Mimic creation, galvanism for life,
But still a glory portioned in the scale.
…
I can detach from me, commission forth
Half of my soul; which in its pilgrimage
O'er old unwandered waste ways of the world,
May chance upon some fragment of a whole,
Rag of flesh, scrap of bone in dim disuse,
Smoking flax that fed fire once: prompt therein
I enter, spark-like, put old powers to play…"

— Robert Browning, *The Ring and the Book* (1868).

"There's nothing so dead as yesterday's news."

— Old journalists' saying.

Introduction: Information and the Frontiers of Science

Just after midnight on 20 October 1813; the dark mouth of a river on the Breton coast. The English chemist Humphry Davy steps onto French soil for the first time, losing touch with one world and entering another. He, his wife, and his young assistant Michael Faraday have just endured a rough crossing of the English Channel to arrive at the sole point of official contact between Britain and France, the remote prisoner-exchange port of Morlaix. Travelling with Napoleon's special permission, they are commencing a scientific Grand Tour, through France and over the Alps into Italy. The first destination is Paris. "Tis indeed a strange venture at this time to trust ourselves in a foreign and hostile country," the twenty-two-year-old Faraday confided to his journal before leaving. "The slightest suspicion," he continued rather melodramatically, "would be sufficient to separate us for ever from England and perhaps from life."[1] In fact, the party enjoyed an excellent reception. At dinners and soirées, Davy finally met the luminaries of French science, and attended meetings of their official academy, the Institut National. Some were major rivals, all were subjects of an enemy power, yet, he was happy to record, they showed him "every kindness and civility—and really seem to consider Science as a kind of family link."[2] Europe had been at war for almost the entirety of Davy's adult life. Now, at last, he was standing at the center of the scientific world, able to talk directly with people he had known only through words carried on paper across the frontiers of war. But at a price: it would be over half a year before he heard again from friends in England.[3]

[1] *Curiosity Perfectly Satisfied: Faraday's Travels in Europe 1813-1815*, ed. Brian Bowers and Lenore Symons (London: Peregrine, 1991), 1. Until the journey to France, Faraday related, he had never been more than 12 miles from London.

[2] Davy to Joseph Banks, 10 December 1813, using transcription from Davy Letters Project (http://www.davy-letters.org.uk/); original in Yale University Medical Library, MS Coll. 49.

[3] By coincidence Davy's crossing of the English Channel overlapped in time with the Sixth Coalition's decisive defeat of Napoleon at the Battle of Leipzig (16-19 October), which destroyed French power in central Europe and

What Davy missed most on his European tour was information—reliable, frequent scientific news. He was one of the few people able to view both sides of the communication gap that separated Britain from the Continent during the Napoleonic wars, and he found it a rather trying experience. In London he had been ensconced at the center of British scientific life; now his connection with that world had been virtually severed, and what little information he did have about goings-on in British science was sparse and stale, coming in the occasional journal obtained, months late, by his French hosts. During his stay in Paris he wrote letters home to British men of science and to his family, hoping the French government would forward them, but had no idea if they were received because he did not get any in return. Even general news of home was faint: English newspapers were banned in France, and he had to rely on the heavily-censored French press. The sense of dislocation became even more acute when Davy's party left Paris at the end of December and headed south. As winter turned towards spring and Napoleon's Empire crumbled into dust, they travelled through the Alps and down into Italy. Davy was now cut off from both Paris and London, starved of fresh information. He had been hard at work on chemical experiments all through his trip, but was still utterly ignorant of what British colleagues thought of what he had sent home months before, or even whether it had reached them.[4] At Rome in March he at last had cause to hope for improvement: France was losing its grip on the Italian peninsula, and there was now "full communication" with England. "Write to me," he

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began the slow collapse that ended with Napoleon's abdication six months later. When Davy left London Napoleonic hegemony in Europe still seemed a viable (if increasingly unlikely) possibility; when he reached Paris, even Napoleon's hold on the throne of France was under threat.

[4] As late as June Davy was still complaining of having "seen no journals and heard no scientific news" since leaving Paris over six months before: Davy to Marc-Auguste Pictet, 17 June 1814, *Pictet Corr.* 3:169-70. Davy laments being "ignorant of every thing going on in the world of Science in England" in Davy to Henry Warburton, 4 July 1814, RS MM/6/72. His report that "notwithstanding our travels I have pursued an unbroken train of chemical researches" is in Davy to William Sotheby, 12 April 1814, using transcription from Davy Letters Project (http://www.davy-letters.org.uk/); original in Huntington Library, San Marino CA, Sotheby Collection, SY 41.

exhorted his brother in London, "tell me all the news."[5] Three weeks later Napoleon abdicated the thrones of France and Italy. At Milan, in June, Davy had the satisfaction of finally meeting Alessandro Volta, whose epoch-making invention of the galvanic battery fourteen years earlier had been the gateway to his own major discoveries, and fame.[6] It was a moment for Davy to reflect on his legacy, his place in scientific history. Afterwards, he toured in Switzerland and Bavaria and then journeyed south again, not neglecting to stop at Bologna, the birthplace of the science of galvanism a quarter century before. He and his party stayed on in Italy until March 1815, when they hastened back to England because news arrived that Napoleon had escaped from exile in Elba and landed on the south coast of France.

* * *

The subject of this dissertation is communication. It looks at how people doing science at the beginning of the nineteenth century wrought solutions to an enduring problem of collective human life: the problem of how to move information across distance. This problem is fundamental to all kinds of large-scale social organization, but in the pursuit of the natural sciences it occupies a special place. It was of vital concern to Humphry Davy and his contemporaries; it remains vital today.[7] Consider: science in the twenty-first century is a vast cooperative enterprise pursued by widely-scattered groups of individuals living all over the world. It is idealized as one of the most purely international human endeavors there is. The collective pursuit is structured and held together by the information that flows across the distances separating its practitioners. Nor are the effects of these specialist communications

[5] Davy to John Davy, 18 March 1813, in John Davy, *Memoirs of the Life of Sir Humphry Davy* (London: Longman, 1836), 1:481-83 (no extant MS).
[6] Davy to Pictet, 17 June 1814, *Pictet Corr.* 3:169-70.
[7] Notably, communication is at the heart of recent prophecies of what the future of science might look like: e.g. Michael Nielsen, *Reinventing Discovery: The New Era of Networked Science* (Princeton: Princeton University Press, 2011).

restricted to the participants alone. On the contrary; the drive to make science more connected has transformed our world—the Internet, hypertext, and the web browser were all originally developed to link and share ongoing research.[8] So here is the double premise from which I begin: the history of science has a lot to teach us about the history of human communication more generally, and a careful look at how communication worked in the past will illuminate new features of the historical development of modern science.

My principal characters are the group to which Humphry Davy belonged, European "men of science" in the decades around 1800.[9] Unlike their modern counterparts, who can draw on a sophisticated social and technological infrastructure for sharing information, for scientific practitioners during this "Age of Revolutions" the problem of communication was conspicuous, raw, and relentless. The troubles they faced in organizing long-distance communications before the modern industrial age were compounded by the disruptions of the Revolutionary and Napoleonic wars, especially the epic conflict between Britain and France that stretched for over twenty years (with one short break), from 1793-1814.[10] Certainly not all scientific practitioners had to suffer the extraordinary sense of disconnection that affected Davy during his continental journey, but each confronted the problem of communication—and thus the problem of *community*—directly and self-consciously. My dissertation is a study of their resourceful and

[8] The web, of course, was a direct product of a search for a better system for sharing scientific information. As CERN's website puts it, "the web was originally conceived and developed to meet the demand for automatic information-sharing between scientists in universities and institutes around the world." (http://home.web.cern.ch/topics/birth-web). On the early ARPANET/Internet—simultaneously means of communication between research sites and research project in itself—see Katie Hafner and Matthew Lyon, *Where Wizards Stay Up Late: The Origins of the Internet* (New York: Touchstone, 1996) and Janet Abbate, *Inventing the Internet* (Cambridge, MA: MIT Press, 1999).

[9] I use the actors' term "men of science" in part to emphasise that the *idea* of scientific community was in fact strongly gendered male in this period—though this certainly did not prevent women from participating in science in numerous ways, as creators, mediators and consumers of knowledge. I will also speak of the "scientific world," another contemporary term denoting the international collective of those pursuing the natural sciences. Notably, both of these phrases came into use only at the end of the eighteenth century—pointing towards the emergence of ideas and practices of community defined by a specifically *scientific* vocation rather than broad learned scholarship. For variety, I use the term "scientific community" as well, although this phrase did not appear until the 1830s.

[10] With a short resumption during Napoleon's "hundred days" of power after his return from Elba in 1815.

inventive struggles to keep in touch in a world out of joint—and of how these struggles shaped the emergence of modern science. The turn of the nineteenth century was an epoch of profound transformation in the modes of scientific enquiry, in science's relationship with other areas of intellectual life, and in its place within wider society. This moment was also, I show, characterized by a ferment of creative experimentation in the techniques of scientific communication, in everything from specialist publishing to the ways ordinary laypeople encountered scientific discoveries. My aim is to trace how systems of scientific communication co-evolved with changes in the practices, social organization, and public image of science as it entered the modern age.

I pursue this project through a focus on a particular scientific field with a very high profile around 1800: galvanism, the science of electricity, matter, and life. Carried on by a multitude of individuals across Europe, galvanism fostered a thriving culture of fast-paced experimental work based around a self-conscious race for discoveries. Novel, spectacular, and sometimes controversial, for many people it represented the cutting edge of progress in natural knowledge. In 1800 the appearance of Alessandro Volta's battery transformed and accelerated the field, exposing a connection between electricity and chemistry that had been absent from Bologna anatomy professor Luigi Galvani's original experiments with frogs announced in the early 1790s. The result was a proliferation of information about galvanism in both specialist and general media. Because it rose to prominence precisely when the European intellectual world was riven and fractured by war and national rivalry, galvanism gave focus to the problem of scientific communication. Demand for information about galvanic experiments and discoveries fuelled creativity in the strategies for moving scientific information around. In this dissertation I follow galvanism across a wide range of different communications media, looking in

newspapers, journals, letters, and the records of conversations to track how it propagated through the world. I show how practices of scientific communication emerged out of the day-to-day activity of individuals, as rough-and-ready tools forged on the job in pursuit of practical goals.

My argument runs as follows. Beginning towards the end of the eighteenth century, there was a reorientation in the relationship between the practice of science and the media in which science was communicated, which turned on a particular entity: *scientific information*. Also called "intelligence," or "scientific news," scientific information was a phenomenon with very old roots stretching back over centuries of scholarly exchanges. But around 1800 there was not just (by any measure) much more of it; it was also performing new functions. Among far-flung communities of inquiry—especially those pursuing fast-moving experimental sciences like galvanism and chemistry—information emerged as a time-critical regulator of scientific activity. From the pages of new monthly journals to the columns of the daily newspapers, scientific information was now more than epiphenomenal news of scholarship or books. It took on a crucial role in the process of arriving at new scientific knowledge, defining a new pace, a new rhythm, and a new preoccupation with time that did not apply in other areas of intellectual life. Day by day, month by month, appearing in print in ever greater quantities, this stream of news and information serialized the progress of science itself. Information bound specialist communities together and dealing in information was a necessary part of the daily life of someone who aspired to a scientific vocation. That is why Davy so missed it on his European tour.

Galvanism was distinctive because it was an *information-based* science. Unlike many other scientific fields around 1800, from mathematical astronomy to natural history, galvanism was not rooted in books. Rather, it was propelled forwards by short articles or announcements in

journals, scraps of intelligence in newspapers, and private exchanges in letters—all of which combined to form a rolling bulletin of experimental progress. Crucially, this stream of information looked different depending on your location, position in the scientific community, and connections to the many channels on which pieces of information moved. Fast-moving, experimentally-based, and strongly associated with a race for *discoveries*, galvanism had many practitioners scattered across war-torn Europe eager to find out what their colleagues and rivals were doing. Because of this, distance and time mattered more in the practice of galvanism than in other sciences, so that the problem of *communication*, the problem of getting information across distance from person to person and from place to place, was particularly pressing. Galvanism is the field to look at to understand how the scientific community conceptualized and (always imperfectly) solved that problem at the beginning of the nineteenth century. I seek to use its story to illuminate a transitional moment in science's history, when the natural sciences were dissociating from other kinds of intellectual activity and emerging as their own information world.

I argue for three interrelated trends in the surge of scientific information around 1800. First: galvanism buoyed the rise of new fast-paced monthly journals, specifically devoted to collecting and printing up-to-date information for the use of scientific practitioners. These journals elbowed aside older eighteenth-century modes of communicating science, bringing periodical print right into the *ongoing* day-to-day pursuit of experimental discovery. Galvanism had a particularly high profile in their pages, and the intense interest and activity it generated was simultaneously a cause and a consequence of the journals' success. With the journals' rise, rapid intelligence acquired a new valence in the actual hands-on practice of science—a trend I relate to

the decline of a broad-based "natural philosophy" as a way of knowing nature, and the ascent of the laboratory sciences, particular galvanism and chemistry.

Second: scientific information had a rising profile in *general* culture; far from being a self-contained system, it was very much part of the broader news landscape of Europe and beyond. This new visibility for science evolved in tandem with a general boom in news and periodical print associated with the French Revolution and the wars which followed. One key manifestation of this was the elevation of science's presence in the preeminent vehicles of public information transmission: the daily newspapers. Both the newspapers and the new scientific journals participated in what I call the "periodical commons"—a landscape of interconnected print media which spread and shared information by reprinting from one another. The increased presence of science—and galvanism especially—*as news* within this periodical commons was bound up with changes in the relationship of science to broader public audiences. It was associated in Britain with the advent of prestige projects for a new kind of "public science," epitomized by the foundation of the Royal Institution, and in France with Napoleonic state support for scientific endeavors in the name of progress and national glory.

Third: the wartime environment brought about an acute and *self-conscious* concern among men of science—especially in Britain and France—with the problem of communicating with each other across national frontiers. This was manifested especially in letters exchanged among elite figures, but it also shaped the operation of the new journals, which rather unexpectedly emerged as important technologies for moving information across geographical and geopolitical divides. War forced a rethink of the nature of scientific community and of the means to hold it together in the post-Revolutionary world. Precisely because it so disrupted scientific communications, war pushed men of science to innovate as communicators, indeed

forced them to think of themselves much more consciously *as* communicators, with long-term consequences.

The ruptures and disruptions of the Napoleonic wars, together with the transition to European peace after 1815, form the background to my narrative—just as these events formed the ever-present background to daily life for the people I study. But war and geopolitics also shaped communication in galvanism in quite specific ways. A broad conclusion of my research is that information about galvanism was very often enmeshed and tangled up with other kinds of intelligence that had nothing to do with science. Thus the wider history of news and communication in this period is woven throughout my story. For example, when London men of science like Davy eagerly perused rare issues of French newspapers, they were scanning not just for news of military developments on the Continent but also for the latest reports on Parisian science. Similarly, American diplomats taking secret dispatches between the US Ambassadors in Britain and France (amidst growing tensions with both nations) also carried letters reporting new galvanic discoveries—which were in turn treated in the newspapers as prizes gained for national glory. Science in the Napoleonic era was no longer the unworldly scholarship of the early modern Republic of Letters, but certainly not yet a jealously-guarded cornerstone of wartime economies. Rather, it inhabited a tense space in between, uncomfortably balanced between ideals of international community and rising nationalist political cultures. Following scientific information opens up a way to explore how the pursuit of natural knowledge intersected with national identity, state power, and shifting geopolitical fault lines at the beginning of the nineteenth century.

Information as a Historical Object

In the last fifty years, information has emerged as a way of seeing the world, a master-concept for ordering reality.[11] Stemming from the mathematical theory of communication ("information theory") created in the 1940s, and the simultaneous development of the electronic computer, this informational worldview reached its mature form with the rise of the Internet, the core of our (ongoing) "information revolution." But because the historical origins of the informational worldview are recent, and because it has become so pervasive, we might want to think carefully about what precisely we mean when we deploy the word "information" to speak about the past. What exactly is talk of information before the advent of modernity *about*? To what extent does our use of the term map onto structures, categories and practices that would be recognizable to someone living two centuries ago? How can we balance our *analytic* use of information, as a conceptual tool, with the project of recovering how historical actors viewed their world?

The study of information has launched a whole field of inquiry at the intersection of philosophy, computing, mathematics, and the natural sciences.[12] But such work is concerned with information as a universal object, outside history (and perhaps even space and time), and is silent on how we might use it to put a question to the past.[13] Fortunately, historians have not been

[11] Some physicists and philosophers have even suggested that information *is* the fundamental constituent of reality, a view summed up in John Archibald Wheeler's well-known slogan: "it from bit." John Wheeler, "Information, physics, quantum: The search for links," in *Complexity, Entropy, and the Physics of Information*, ed. Wojciech Zurek (Redwood City, CA: Addison-Wesley, 1990), 5. Debate continues on how exactly one might specify an ontology of information: Luciano Floridi, "Against Digital Ontology," *Synthese* 168 (2009): 151-178. See also: http://xkcd.com/505/.

[12] The noted philosopher of information Luciano Floridi has proposed that the question "what is information?" should be regarded as a "Socratic question" in the tradition of "what is being?," "what is truth?," and "what is knowledge?"—i.e. not a question which can be completed with a concise answer or definition, but rather one whose elaboration forces open a whole vein of philosophical inquiry. Luciano Floridi, "Information," in *The Blackwell Guide to the Philosophy of Computing and Information* (Oxford: Blackwell, 2004), 40.

[13] Of course, it is certainly possible to assert the plausible but basically *non-historical* position that information, in our modern universalist bits-and-bytes sense of the term, has always existed (even long before the formulation of the concept)—and then to go on to analyze past communications or media using modern information theory as a trans-historical guiderail. This may still be the basis for illuminating stories; it is the kind of viewpoint taken in science writer James Gleick's bestseller *The Information: A History, a Theory, a Flood* (New York: Vintage, 2012). Such an

slow to respond to this challenge, with a number of scholars studying the genealogy of current preoccupations with the handling of information or knowledge. Ann Blair has vividly documented responses to "information overload" among early modern scholars, examining the use of tools such as note-taking, indexing, reference books, and encyclopedic compilations. Jacob Soll has brilliantly shown how Jean-Baptiste Colbert's concern with the mastery of information was integral to the formation of the French absolutist state under Louis XIV. Most recently, Jeremy Black has even sought to answer the large-scale question of the "rise of the West" in informational terms.[14]

I have drawn inspiration from these histories, but my perspective here is different. I seek to recover a *historical* meaning of information that conforms more closely to how people around 1800 saw their world and how they interacted with each other. One reason for taking a careful and critical approach with this concept is that "information" was in fact a word in wide use at the beginning of the nineteenth century, but it was used in a different and rather more specific sense that it now carries. The meaning historians and other present-day writers generally have in mind when they deploy the word is, broadly, information as "organized data."[15] Daniel Headrick articulates it thus: "Information implies an assemblage of data, such as a telephone book, a map,

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approach is analogous to, for example, using the modern science of genetics to talk about animal husbandry in the Middle Ages; it is not *wrong*, and indeed quite defensible in the terms of modern science, but historians tend to be justifiably wary of this kind of approach to the past because it departs from a narrative framed around the meanings historical actors actually ascribed to what they were doing.

[14] Ann Blair, *Too Much to Know: Managing Scholarly Information before the Modern Age* (New Haven: Yale University Press, 2011); Jacob Soll, *The Information Master: Jean-Baptiste Colbert's Secret State Intelligence System* (Ann Arbor: University of Michigan Press, 2009); Jeremy Black, *The Power of Knowledge: How Information and Technology Made the Modern World* (New Haven: Yale University Press, 2014). For an informational perspective on the Victorian period see Aileen Fyfe, "The information revolution," in *The Cambridge History of the Book in Britain, vol. 6: 1830-1914*, ed. David McKitterick (Cambridge: Cambridge University Press, 2009), 567-94.

[15] The sense now commonly used, for example, in the field of library and information science. More rigorously, it also conforms to what philosopher Luciano Floridi terms the "General Definition of Information," according to which information is well-formed and meaningful data. Luciano Floridi, *The Philosophy of Information* (Oxford: Oxford University Press, 2011), 83-84.

a dictionary, or a database—not random data, however, but data organized in a systematic fashion."[16]

But in 1800 "information" was not associated with data at all. Rather, to speak of information necessarily implied an act of communication.[17] More specifically, the term grouped together a set of features that might describe a particular communicative act and its content: novelty, freshness, unexpectedness, and (often but not always) *transmission across distance*. Information was a dynamic entity, always in the process of moving towards human recipients.[18] Consequently, it was closely connected—and often in fact identified with—intelligence and news; as Dr Samuel Johnson's dictionary tersely put it, information was "intelligence given, instruction."[19] The implication was something unknown and interesting (i.e. "informative") to a

[16] Daniel Headrick, *When Information Came of Age: Technologies of Knowledge in the Age of Reason and Revolution, 1700-1850* (Oxford: Oxford University Press, 2002), 4. Headrick's survey demonstrates how applying our broad notion of information to the past can yield problematic results; it offers a compendium of interesting but disparate sketches of what Headrick calls "information systems"—maps, dictionaries, encyclopaedias, statistical tables, postal services, population censuses, semaphore telegraphy, and even schemes of botanical and chemical nomenclature—which would not all have looked particularly connected to the inhabitants of the eighteenth and early nineteenth centuries. Analogous problems can confront information history articulated from the information and library science point of view; see Toni Weller, *Information History—An Introduction: Exploring an Emergent Field* (Oxford: Oxford University Press, 2008). For a broad treatment of similar topics which, more successfully, prefers an analytic of "knowledge" rather than "information," see Peter Burke, *A Social History of Knowledge II: From the Encyclopaedia to Wikipedia* (Cambridge: Polity, 2012).

[17] Interestingly, this notion is also implicit in the idea of information in what has been hailed as the founding document of information science, Claude Shannon's "Mathematical Theory of Communication," *Bell System Technical Journal* 27 (1948): 379-423, 623-56, which in turn derived inspiration from Harry Nyquist's 1924 mathematical investigation of "the maximum speed of transmission of intelligence by telegraph." In Shannon's paper information is always something which is *in the process of being transmitted* on some channel.

[18] One illuminating historical work that does treat of information more in this fashion, and which makes a clean distinction between information and knowledge, is C.A. Bayly's *Empire and Information: Intelligence Gathering and Social Communication in India, 1780-1870* (Cambridge: Cambridge University Press, 2000).

[19] With wonderful circularity, eighteen pages later Johnson went on to define "intelligence" in the following manner: "commerce of information; notice; mutual communication; account of things distant or secret." Samuel Johnson, *A Dictionary of the English Language* (London: 1755), via http://johnsonsdictionaryonline.com/. The distinction I draw here is also apparent from the entry for "information" in the Oxford English Dictionary. Dr Johnson's definition more or less conforms to sense 2.a of "information" given by the *OED*, which first begins to emerge as far back as the late Middle Ages: "Knowledge communicated concerning some particular fact, subject, or event; that of which one is apprised or told; intelligence, news." The modern use of the word—e.g. in the phrase "information management"—is usually sense 2.d in the *OED*, which dates from the 1930s: "Separated from, or without the implication of, reference to a person informed: that which inheres in one of two or more alternative sequences, arrangements, etc., that produce different responses in something, and which is capable of being stored in, transmitted by, and communicated to inanimate things."

particular individual or group. Information was a *relative* quantity: what's informative to me might be yesterday's news to you.

Putting the emphasis on the act of *transmission* inherent in information around 1800 is more than a piece of historical semantics; it aligns the concept with a deeper truth about how people experienced exchanges with one another before the modern industrial age. Simply put: at the beginning of the nineteenth century, communication between people who were far apart was slow, unreliable, and expensive to a degree that is now utterly alien.[20] That is an obvious point, but one which I believe has been consistently undervalued in historical writing about intellectual communications.[21] An overwhelming tyranny of distance was imposed by two hard limits constraining the pace of travel and information: the speed of the horse, and the speed of the sailing ship. These meant that it was not generally possible to travel over long distances at an average speed of more than about 10-12 miles an hour.[22] Yet the situation was in fact much worse: communication was not just slow but unpredictable. Despite much improvement during

[20] "No aspect of everyday life," writes the historian Tim Blanning, is "more alien to the modern experience." The entire first chapter of Blanning's general survey of European history in the long eighteenth century is devoted to an extended discussion of communication that carefully balances a vivid portrait of the difficulties of traversing distances in the pre-modern world with an account of the very significant if uneven improvements during the period (especially in Britain). "It is communication between people and people, or between people and places," he writes, "that weaves the social fabric. That is why communication provides the best point of entry to the past." Tim Blanning, *The Pursuit of Glory: The Five Revolutions that Made Modern Europe, 1648-1815* (London: Penguin, 2007), 3.

[21] But see Aileen Fyfe, *Steam-Powered Knowledge: William Chambers and the Business of Publishing, 1820-1860* (Chicago: University of Chicago Press, 2012), which pays particular attention to the effects of changing technologies of transportation and communication in the mid-nineteenth century on the logistics of running a national and international "useful knowledge" publishing business.

[22] Yet it was also precisely in this period that human beings first began to break the ancient shackles of distance, albeit in a very limited way, with two iconic new technologies: balloon flight, and the optical (semaphore) telegraph. The first of these carried a few daring aeronauts along at speeds of up to seventy miles an hour (though unfortunately, it was lamented, without any real control over direction). The optical telegraph was an invention of Revolutionary France during the wartime emergencies of 1792-1794, and was used extensively by Napoleon. Using lines of signal towers, it enabled rapid transmission of small amounts of military or state intelligence along a few special routes; the longest line ran from Paris to Venice. The Revolutionary and Napoleonic age was therefore unique in being able to witness the operation of high-speed travel and communication without any real expectation that they could become a part of ordinary life. In tandem with the wartime news boom (and wartime disruptions), this perhaps gave a new meaning to the idea of fast "intelligence," making the speed of communications seem less an unalterable background to life and more like a historically-variable quantity.

the course of the eighteenth century, roads varied enormously, ships might have to wait days or weeks in port for a favorable wind, and good luck trying to send anything bigger than a letter across international borders (and often within them) without specialist help. The disruptions of war provided an additional obstacle, especially because science during the Napoleonic age was always a comparatively weak enterprise that did not command the resources of other international communicators, such as merchants or diplomats. A more *historical* understanding of information that takes account of the importance of transmission therefore hones the concept's edge as a tool for understanding the distinctive features of scientific organization around 1800. Only in a world where signals can be sent around the planet in the blink of an eye does it really make sense to use the same word for the content of communications and locally-stored data.

Nonetheless, while divesting information of its link to data—a link only forged by modern information theory—I do not want to flatten the concept into a pure "actors' category," with no burden of meaning outside that found within historical sources. In fact, I will be concerned with mapping information systems—such as the widespread transmission of texts by reprinting—whose scale and effects were only partially evident to contemporary observers, and which are only now becoming more visible thanks to the assemblage of a large range of sources to which techniques of digital search can be applied. The aim is thus twofold: to reconstruct how scientific communication looked to those actually engaged in it—and *also* to follow information beyond their necessarily limited and partial perspectives. The transformations wrought by the Internet provide the motive and the tools to look back and see the history of science afresh in the Napoleonic age—a moment when communication was likewise fluid and hybrid, in ways very particular to the period but which can also have a strange sort of familiarity. In the pages that

follow information functions both as actors' category and analytical tool for the historian; it is a way of calling into being a dialog between the present and the past.

With this readjustment towards seeing information-as-communication, another question comes into view: the relationship between information and knowledge. I seek to join my work on communication to a broad spectrum of recent scholarship in the history of science identified by James Secord as the study of "knowledge in transit," in which science is approached primarily "as a form of communicative action," where statements are "vectors with a direction and a medium and the possibility of response."[23] But in contrast to most work aimed at understanding "how knowledge travels" (as the question is often phrased), I prefer to see knowledge not as the substance, but the outcome, of acts of communication. Knowledge about galvanism was always unstable, the subject of ongoing discussion and argument in which people repeatedly disputed each other's experiments, quibbled over interpretations, haggled over the truth and how to find it. An answer to the question of what, or where, knowledge was at any time is elusive because it so often depends on the historical voices to which we choose to listen. In contrast, information—and especially news—is concrete. It can be tracked precisely, on a day-to-day basis, and we can frequently say with confidence who did or did not have it, where it was, and what it was being used to do. Following it leads us straight to the laboratory, the experimental table, the journal editor's study, the academy meeting, the public lecture theatre, the newspaper office. Information was the tool men of science *used* to forge consensus over what was true knowledge about galvanic electricity. Furthermore, this was an active and competitive process. In the race for discovery, and scientific fame, access to good intelligence was a way to stay ahead.

Nonetheless, getting the information was only half the problem. The recipient also had to make sense of it, to fit it into an ongoing project of enquiry or view of the world—and here we

[23] James Secord, "Knowledge in Transit," *Isis* 95 (2004): 654-72.

reconnect with some familiar themes in the history of science: authority and trust. Making sense of new texts, whether a short item of "philosophical intelligence" or perhaps a longer experimental report, required the employment of hermeneutic processes which overlapped with those employed by people judging the reliability and correctness of other kinds of information, from government spymasters to readers of the newspapers. In carrying on the business of science—and particularly a fast-moving experimental science like galvanism—intelligence might be drawn from all kinds of sources, and brought to bear on the problem at hand. But different forms of communication framed information in different ways, in terms of the level of trust usually accorded to their contents, and also in terms of audience, mobility, and the level of privacy or publicity particular to each. Every individual overlaid their own web of trust on the pathways along which galvanism moved, and trust was often employed as a guiding principle in constructing mechanisms of communication, from private letters carried across the frontiers of war by reliable neutral parties, to the emerging system of specialist journals that was grounded in the reputations of particular editors. A paragraph in a scientific periodical controlled by a known and reputable figure therefore carried a different valence from an identical copy appearing in a swifter but anonymously-edited newspaper. Yet, as we shall see, webs of scientific trust were by no means secure against error, bad transmissions, or even the occasional imposter. Distance, national rivalry, and the shifting frontiers of war further disrupted the formation of trust through the direct knowledge of individuals.[24] Before his journey to France in 1813, Davy had never met

[24] A wealth of sociological and historical studies exist on the forging of agreement over scientific knowledge in local settings, but discussion of the role of various kinds of *distance* in this process remains undeveloped. See however Mario Biagoli, "Financing the Aura: Distance and the Construction of Scientific Authority," Chapter 1 in *Galileo's Instruments of Credit* (Chicago: University of Chicago Press, 2006), 21-75; also David Kaiser, *Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics* (Chicago: University of Chicago Press, 2009). Contrast with the picture of localized face-to-face trust in knowledge in Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England* (Chicago: University of Chicago Press, 1994); for a different view which emphasizes the importance of "weak ties" in early modern science see David Lux and Harold Cook,

the elite Parisian savants he was on his way to see, though he had been closely tied into exchanges of information with them for over a decade. The written word proved a powerful tool for bridging such distances, yet it was frequently hard to assess and sometimes impossible to trace back to its human source. Men of science in London, Paris, Berlin, and Bologna may all have seen themselves as participating in the same science of galvanism, but they viewed their collective effort from very different perspectives indeed.

In sum: for men of science in 1800 the pressing problem was not ordering *knowledge* they already had, but getting and making sense of new *information* about what other individuals in their community were up to. Identifying information with data misses the crucial fact that texts communicating new intelligence—perhaps unreliable, unverifiable, or from far away—were treated very differently from volumes of agreed-upon knowledge which could be pulled off a library shelf and examined at any time. The pressure was greatest in galvanism and other rapidly-developing fields where practitioners craved up-to-date information about one another's work, and in which new discoveries proliferated according to an uneven war-torn geography of communication. Staying current on what colleagues or rivals were doing was crucial, but no stable or reliable recipe for doing so existed. Consequently, members of the international scientific community habitually grumbled that information was not moving as quickly, reliably, or widely as it ought to be. Seen from the perspective of its inhabitants, the condition of the scientific world at the beginning of the nineteenth century was not defined by "information overload," but rather by a chronic state of *information starvation*. And this when the amount of text about science being printed each month was greater than at any previous time in the history of the world.

"Closed circles or open networks?: Communicating at a distance during the scientific revolution," *History of Science* 36 (1998): 179-211.

Following the News: An Elaboration of the Approach

In framing my research I have been able to mine a rich vein of prior scholarship at the intersection of two fields: the history of science and the history of the book.[25] I see this dissertation as a contribution to that lively meeting of historiographies, but the approach I deploy here is a little unusual. Much path-breaking work in this area and in book history more broadly has traced the histories of particular *forms* of print (such as early modern volumes of natural philosophy, or Victorian periodicals), or even of individual publications—most notably in Robert Darnton's publishing history of the *Encyclopédie,* and James Secord's magisterial study of the "readings" of the 1844 *Vestiges of the Natural History of Creation*.[26] This continues to prove an extraordinarily fruitful and powerful approach. Here, however, I adopt a different line of inquiry. I follow not a particular textual form, or important publication; rather, the method is to track intelligence or news about the *scientific field* of galvanism through the broadest possible range of sources, and see where it takes us. Sometimes we will find ourselves in print media—journals, newspapers, general periodicals—but on other occasions we will be looking over the shoulders of men of science penning manuscript letters to colleagues, or listening in lecture halls, or

[25] "History of the book" here understood in its broad sense as the history of print rather than specifically the codex. In fact, the codex is the form of communication must conspicuous by its absence in my story. This is because books were simply not particularly important in the pursuit of galvanism: of those few written on the subject during the Napoleonic period, none acquired very much importance. Clearly, if this were a dissertation about communication in natural history, for example, books would play a more central role, as they continued to do in that field for a long time (despite massive expansion in scientific journal publication during the decades after 1800, the most important single text in the life sciences in the nineteenth century was still a book, Darwin's *Origin of Species*).

[26] Robert Darnton, *The Business of Enlightenment: A Publishing History of the Encyclopédie, 1775-1800* (Cambridge MA: Harvard University Press, 1979); James Secord, *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of* Vestiges of the Natural History of Creation (Chicago: University of Chicago Press, 2003). Key studies within the history of science focusing on the early modern codex and the Victorian periodical respectively are: Adrian Johns, *The Nature of the Book: Print and Knowledge in the Making* (Chicago: University of Chicago Press, 1998); Geoffrey Cantor et al., *Science in the Nineteenth-Century Periodical: Reading the Magazine of Nature* (Cambridge: Cambridge University Press, 2004). A classic definition of the field of book history is Robert Darnton, "What Is the History of Books?," in *The Kiss of Lamourette: Reflections in Cultural History* (New York: Norton, 1990), 107-35; see also Robert Darnton, "'What Is the History of Books?' Revisited," *Modern Intellectual History* 4 (2007): 495–508. The intersection with the history of science is explored in Nicholas Jardine and Marina Frasca-Spada, eds., *Books and the Sciences in History* (Cambridge: Cambridge University Press, 2000).

overhearing private conversations about new discoveries (later jotted down in a diary). Therefore this dissertation is not quite book history, nor even a history of written media, but rather a history of news, communication, and information.[27]

This approach reveals that scientific communication in this period was composed of a fluid and hybridized matrix of different techniques and media, variously employed according to circumstances and individual choice. First, individual forms which later developed in very different directions overlapped or shared conventions: cross-fertilization between "journals" and "newspapers" (in French, and sometimes English, denoted by the same word) is one important example. Second, information migrated very freely both between different kinds of text, and also to and from non-textual venues of communication, like lectures or conversations. Private discussions fed into manuscript letters to distant colleagues, letters found their way (sometimes unexpectedly or without permission) into print in specialist journals, whole paragraphs from the journals turned up in newspapers and magazines, imported newspapers provided vital foreign scientific intelligence for the journals, and the appearance of the latest issues of periodicals started new conversations. A central feature of this matrix of communication was the pervasive phenomenon of *reprinting*—explored in detail in my first chapter—with which publications repackaged information from a range of different sources, bringing it to new audiences.[28]

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[27] Some historians of the book have already produced illuminating studies along similar lines: Jeffrey Freedman, *A Poisoned Chalice* (Princeton: Princeton University Press, 2002); Robert Darnton, *Poetry and the Police: Communication Networks in Eighteenth-Century Paris* (Cambridge MA: Belknap, 2010); Robert Darnton, *The Devil in the Holy Water, or the Art of Slander from Louis XIV to Napoleon* (Philadelphia: University of Pennsylvania Press, 2009); Robert Darnton, "An Early Information Society: News and the Media in Eighteenth-Century Paris," *American Historical Review* 105 (2000): 1-35. A recent use of this kind of approach is Andrew Pettegree's impressive history of "the news" in early modern Europe, which historicizes the newspaper as an unstable and mutable format shaped by constant interaction with others forms of news, from manuscript newssheets, to merchants' private letters, to popular ballads and broadsides. Andrew Pettegree, *The Invention of News: How the World Came to Know About Itself* (New Haven: Yale University Press, 2014).

[28] There exists a mythic narrative, now cast aside by historians of science but still endemic in popular accounts and the media, in which it is claimed or implied that the techniques of "modern" scientific communication and community were largely invented in the seventeenth century—in the extreme, even twentieth-century practices like the peer review system have been said to be essentially seventeenth century ideas. Such claims not only

Because boundaries between different communicative forms were so permeable around 1800, it will not do to study scientific communication by focusing exclusively on single formats like letters, or journals, or newspapers: it is the information itself that is the quarry. Accordingly, I have sought to move along with information in my historical story, following it through space and time to watch how it was created, propagated, transformed, and consumed by people for particular ends. Only by reconstructing how information actually moved, where and when it turned up, and especially what people *did* with it in their ordinary day-to-day scientific labor, can we be sure of capturing not just the diversity of information media in science but also why people cared about the contents and what they used it for.[29]

Information and scientific practice created one another. In galvanism, the way particular episodes or controversies played out was molded by the media that carried information between the participants. And these media—particularly the new scientific journals—were *themselves*

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mischaracterize scientific practices in the early modern world, but also implicitly deny the fluid and hybrid nature of scientific communications media at the beginning of the nineteenth century. One example: the origin of modern scientific journals is simply identified with Henry Oldenburg's foundation of the *Philosophical Transactions* in 1665 (for a recent example of that myth voiced by a very eminent scientist see "Royal Society celebrates 350 years of scientific publishing," *Guardian*, 6 March 2015, http://www.theguardian.com/science/grrlscientist/2015/mar/06/royal-society-celebrates-350-years-of-scientific-publishing). In part because of such myths, there remains a danger of applying now-freighted labels like "journal" or "newspaper" to sources from around 1800 confident in knowing what we mean when, in fact, we are talking about forms which were neither unitary nor stable. One purpose of this dissertation is to look behind such words to find out what the things to which we apply these terms were actually like during this period, and how they were changing. The aim is not to seek a single "origin" of modern scientific publication, but rather to uncover one of several pivotal moments within a textured, connected, and contingent history of scientific print stretching from the fifteenth century to the present.

[29] To draw a metaphor from the legal scholar Lawrence Lessig's work on information systems, what I am offering here is a history of the changing *information architecture* of science around 1800—the interlocking system of media, institutions, and individuals that enabled scientific exchanges. (See Lawrence Lessig, *Code: And Other Laws of Cyberspace, Version 2.0* (2006), available at http://codev2.cc/.) Like the physical architecture of a building, this information architecture constrained and shaped (but did not determine) the interactions among the people who chose to live inside. It encoded and enabled certain ways of making scientific knowledge as a dispersed community, and excluded others. The ability of scientific practitioners to shape different parts of this architecture varied. Some constraints were inescapable: war or no war, it was impossible to get a message from London to Paris in a day. Others were malleable but outside the control of the scientific community: the speed which French newspapers containing scientific reports arrived in London, for example. Other features were in a sense governed by men of science themselves, but also bound by broader norms—the editor of a journal might in principle be free to run his publication how he wanted, but in practice a loose set of book-trade practices and community standards governed how he did so. Still other components were more matters of individual choice: who to write letters to, and what to write in them.

shaped by the distinctive tenor of work in galvanism (and other sciences): the day-to-day activity of reading or hearing about other people's experiments, repeating them, varying them, trying new ones, working out interpretations, and writing or talking about the process and the results. The prize up for grabs was "discovery," equated with a contribution to scientific progress. Naturally, to be part of the community doing galvanism—to participate in the game of discovery—it was essential to stay connected and share information with other people working in the field, whether in the same town or situated hundreds of miles away. Much hinged on the transmission and interpretation of information about particular experimental details. Accordingly, at various points during my story, I take a high-resolution look at how communication factored into particular technical questions and controversies. Problems like building a battery, understanding the electrolysis of water, or experimenting with new chemical elements—all these were situations where intelligence mediated interactions-at-a-distance among large groups of individuals, creating winners and losers in the race for discovery, and creating scientific knowledge. Information moving across wartime frontiers created and sustained distinctive broad-based communities of experimental practice, facilitating emulation, rivalry, disagreement, and the forging of uneven degrees of consensus.[30]

Following the news of particular galvanic experiments will also take us beyond specialist science, to the question of the communicative relationship between scientific activity and the wider society which sustains it—the history of "popular" or "public" science. Important studies have exposed the degree to which the phenomenon of "popular science" was in fact the *outcome*

[30] Such practice-based links were far from the only way of creating or maintaining scientific community, but they had real resilience because they were sustained by a mutual hunger for relevant and useful information. In this view, scholarly politeness, gentlemanly civility, internationalism, and other displays of selfless devotion to the collective were certainly not absent as ways of building community and personal credit, but the desire for information that could be profitably put to use in the pursuit of experimental discovery emerges as a powerful motivating factor too. For civility and politeness in early modern science and scholarship see Shapin, *A Social History of Truth*; Goldgar, *Impolite Learning*.

of mid- to late-nineteenth-century debates over scientific authorship and professional status, accompanying the rise of a new kind of writer, the scientific popularizer.[31] Such work makes it quite clear that applying the "popular science" label to relations between scientific specialists and laypeople before the mid-nineteenth century is no longer tenable.[32] This invites historians to deepen our understanding of the dynamics of these interactions around 1800, when the boundary between "science" and "the public" was far more fluid.[33]

Studying information about galvanism provides a new perspective on this question: by allowing us to see where scientific intelligence was actually going, it opens up a view in which different texts and communicative forms, rather than being intrinsically "specialist" or "popular," could in fact serve multiple purposes in different contexts. It was common, for example, for certain newspapers to bring crucial pieces of scientific information to informed specialists as well as curious laypeople. Often the same words would be reprinted across separate types of media, as when paragraphs from journals with a more restricted audience provided ready-made

[31] See in particular Bernard Lightman, *Victorian Popularizers of Science: Designing Nature for New Audiences* (Chicago: University of Chicago Press, 2010); also Secord, *Victorian Sensation*; Jonathan Topham, "Rethinking the History of Science Popularization/Popular Science," in *Popularizing Science and Technology in the European Periphery, 1800–2000*, ed. Faidra Papanelopoulou, Agustí Nieto-Galan, and Enrique Perdiguero (Farnham: Ashgate, 2009), 1–20. Work on the popularization of science in the nineteenth century has mainly focussed on Britain; for an important exception see Lynn Nyhart, *Modern Nature: The Rise of the Biological Perspective in Germany* (Chicago: University of Chicago Press, 2009).

[32] For an earlier perceptive critique see Roger Cooter and Stephen Pumfrey, "Separate spheres and public places: Reflections on the history of science popularization and science in popular culture," *History of Science* 32 (1994): 237-67.

[33] In framing my own approach I draw on a wealth of previous work on science and "the public" or "the public sphere" in the eighteenth and early-nineteenth centuries. Particularly relevant for this dissertation is Jan Golinski, *Science as Public Culture: Chemistry and Enlightenment in Britain, 1760-1820* (Cambridge: Cambridge University Press, 1992); see also Thomas Broman, "The Habermasian Public Sphere and 'Science *in* the Enlightenment,'" *History of Science* 36 (1998):123–50; Larry Stewart, *The Rise of Public Science: Rhetoric, Technology, and Natural Philosophy in Newtonian Britain, 1660-1750* (Cambridge: Cambridge University Press, 1992); Michael Lynn, *Popular Science and Public Opinion in Eighteenth-Century France* (Manchester: Manchester University Press, 2006); Larry Stewart, "Feedback Loop: A Review Essay on the Public Sphere, Pop Culture and the Early-Modern Sciences," *Canadian Journal of History* 42 (2007): 463–83. The seminal work of historical sociology informing these discussions is of course Jürgen Habermas, *The Structural Transformation of the Public Sphere: An Enquiry into a Category of Bourgeois Society*, trans. Thomas Burger (Cambridge, MA: MIT Press, 1989); for an important critique of historians' use of Habermas see Harold Mah, "Phantasies of the Public Sphere: Rethinking the Habermas of Historians," *Journal of Modern History* 72 (2000): 153–82.

copy for broader-readership magazines and newspapers. The uses people had for the same information might be quite different—an intriguing fact for the layperson might be a vital hint for the specialist in pursuit of new discoveries. Once we realize how fluid and unsystematic the movement of information between specialist and lay audiences really was around 1800, we can then begin to trace the rise of *new* models—pioneered in Britain by Humphry Davy at the Royal Institution—in which the "man of science" was indeed self-consciously imagined as distinct from a public cast in the role of passive audience rather than active participant in the creation of knowledge.[34]

Galvanism was bound up with this shifting relationship between science and society. Initially, at least, its development appeared to promise an expansion and even democratization of participation in scientific inquiry. Galvani's first experiments did excite broad interest, but the investigation of animal electricity he launched was rather recondite and technical, requiring specialist knowledge of anatomy as well as electricity, and the number of people contributing to galvanism during its first decade was fairly limited.[35] This all changed in 1800 with the appearance of Alessandro Volta's battery. Volta's iconic instrument placed a new electrical power at the full disposal of experimenters, opening up a whole unexplored field of practical investigation. Easy to build, and requiring little prior knowledge to use, the battery dramatically lowered the barrier to entry for investigation at this cutting edge of physical science. A large number of people all across Europe and beyond built batteries themselves and began to explore what the new device could do. Growing symbiotically with the new journals, and cross-fertilizing with broader news cultures, the pursuit of galvanism fostered an alternative economy of scientific information, no longer oriented around the polished long-form disquisitions of elite

[34] See Golinski, *Science as Public Culture*, 188-235.
[35] Citations to all relevant historiography on galvanism and electrochemistry appear in the chapters that follow.

academy memoirs or books, but around brevity, speed, dense interconnection, and broad distribution of information.

Furthermore, the advent of the battery also linked galvanism with chemistry, another science with a high profile and many practitioners. Soon after its appearance, Volta's battery was used to decompose water into hydrogen and oxygen, revealing a clear link between galvanism and the chemical forces binding matter together. Sitting at the intersection of the study of electricity, matter, and life, and variously linked to natural philosophy, chemistry, physiology, and medicine, galvanism now cut right across the boundaries of knowledge in the sciences. Its precise location within received orders of knowledge was unclear. This broad scope, encompassing phenomena ranging from technical investigations of electrolysis to unorthodox medical therapies, was one factor contributing to the high degree of interest galvanism attracted. It helped also that galvanism could be spectacular and at times controversial: dramatic electrical demonstrations made for exciting viewing, and grisly experiments on animal and human bodies raised troubling questions about the material basis of life and death. The patronage of Napoleon, who endowed a large prize for galvanic research in 1802—declaring the subject "the road to great discoveries"—further added to the excitement. For these and other reasons, to many people living during the Napoleonic period galvanism appeared as the single most exciting, novel and fast-moving part of the scientific enterprise. "Perhaps no equal period of Philosophical history," Humphry Davy proclaimed in a Royal Institution lecture in 1808, "exhibits a more brilliant picture of the activity and power of the human mind, seeking for a new empire over the natural powers of things, than may be contemplated in the annals of the last eight years."[36] Davy was a biased observer, but his view was widely shared; often galvanism seemed to stand in for the frontiers of progress in human knowledge of nature. For Davy, it proved the fast-track to

[36] RI MS HD/1/A/2. Davy was referring to the eight years that had elapsed since the advent of Volta's battery.

scientific fame, propelling him in ten years from obscure young provincial radical to one of the most famous men of science of the nineteenth century. In the process, however, he and his colleagues would leave behind the relatively open field of galvanism's early years. Instead, as we will see in Chapter 2, Davy found he needed to manage his fame and public image, especially in the pages of newspapers, which latched onto him as the voice of galvanism and chemistry in Britain but which did not always treat him as he would have liked. At the very moment when science was appealing to greater numbers of people than ever before, a new powerhouse of public information—still little-studied by historians of science—began to drive a mediating wedge between scientific practitioners and laypeople: the daily press.

Communication and the Boundaries of Science in the Age of Revolutions

A wealth of scholarship on intellectual life in the early modern world has painted a rich portrait of the imagined community of learned individuals known, then and now, as the "Republic of Letters."[37] Steadily, piece by piece, such work has pulled down old historiographically-constructed boundaries between what we now call "science" and other areas of early modern learning. This is apparent in the intellectual biographies of key individuals: we now have Galileo the Renaissance humanist, Leibniz the jurist, historian, genealogist, linguist, and theologian, and Newton the dedicated enquirer into biblical prophecy and ancient chronology.[38] In addition, this fertile admixture of what would later come to be quite separate areas of specialization structured

[37] The literature is vast. See especially Anne Goldgar, *Impolite Learning: Conduct and Community in the Republic of Letters, 1680-1750* (New Haven: Yale University Press, 1995); Anthony Grafton, "A Sketch Map of a Lost Continent: The Republic of Letters," in *Worlds Made by Words: Scholarship and Community in the Modern West* (Cambridge MA: Harvard University Press, 2009), 9-34; Lorraine Daston, "The Ideal and Reality of the Republic of Letters in the Enlightenment," *Science in Context* 4 (1991): 367–86; Peter Miller, *Peiresc's Europe: Learning and Virtue in the Seventeenth Century* (New Haven: Yale University Press, 2000).

[38] John Heilbron, *Galileo* (Oxford: Oxford University Press, 2010); Maria Rosa Antognazza, *Leibniz: An Intellectual Biography* (Cambridge: Cambridge University Press, 2011); Jed Buchwald and Mordechai Feingold, *Newton and the Origin of Civilisation* (Princeton: Princeton University Press, 2012).

group sociology and communication. Pushing back against the view that the investigation of nature had a distinctive sociological character during the early modern period, David Lux and Harold Cook have concluded that "the sociology of early modern science was therefore little different from the sociology of early modern knowledge more generally."[39] Scholars always had their enclaves and specializations—often reflected in formal subdivisions within academies, universities, and other institutions—yet right into the late Enlightenment the pursuit of natural knowledge was still by and large carried on within a broad-based Republic of Letters encompassing the grand sweep of learned production.[40]

During the period spanning roughly the French Revolution to the mid-nineteenth century, this situation changed utterly. Historians of this period now tend to agree that around 1800 the European approach to natural knowledge underwent a process of redefinition—a transformation that has been provocatively called "the invention of science."[41] A new figure, the "man of science," arriving on the scene around the end of the eighteenth century, came to represent in his person the idea of a distinctively rational, objective, technical, practically-grounded, and

[39] Lux and Cook, "Closed circles or open networks?," quote on 201. Lux and Cook also note (202) that practitioners of the new philosophy often "operated in a manner little different from other communities of people, whether scholars, diplomats, merchants, or any others who travelled long distances and followed up their meetings with correspondence."

[40] See for example Lawrence Brockliss, *Calvet's Web: Enlightenment and the Republic of Letters in Eighteenth-Century France* (Oxford: Oxford University Press, 2002).

[41] Andrew Cunningham and Perry Williams, "De-Centring the 'Big Picture': 'The Origins of Modern Science' and the Modern Origins of Science," *British Journal for the History of Science* 26 (1993): 407–32; Andrew Cunningham, "Getting the Game Right: Some Plain Words on the Identity and Invention of Science," *Studies in History and Philosophy of Science A* 19 (1988): 365–89. Cunningham's position is explicitly contrasted, of course, with the long tradition of locating the origins of "modern science" in an early modern scientific revolution: Cunningham asserts that, while there may well have been a revolutionary transformation in approaches to natural knowledge during the seventeenth century, it did not produce modern science (and *certainly* not the modern scientist). Of course, disagreements over broad claims like these usually boil down to rather futile disputes over choices of definitions and emphasis: depending on one's notion of what "modern science" is, and what it might mean for it to come into existence, plausible arguments can be no doubt be advanced for placing its "birth" in any period from the Renaissance until the Cold War. Following some of Cunningham's insights, I venture that we might get more genuinely historical traction by turning the question around, asking "what was science?" in the period 1789-1850 in which historians generally place the emergence of *political* and *industrial* modernity.

progress-oriented approach to knowledge.[42] This coincided with the eclipse of "natural philosophy" as a broad road to knowing nature (to be replaced by a more specialized "physics"), and the emergence around 1800 of chemistry and galvanism as the most active and fast-moving areas of scientific discovery.[43] This dissertation can therefore be read as one strand of a broader story of how the investigation of natural knowledge began to cleave off from other areas of intellectual life to become the thing we call modern science.[44]

To acknowledge these changes is to realize that in looking at galvanism and scientific communication around 1800 we are surveying intellectual ground which is shifting beneath our feet.[45] Across the chapters which follow we will see five forces at work in this transition. First, the rise to prominence of specialized experimental sciences like galvanism and chemistry, which required mastery of a wide range of hands-on skills, and in which abstract ideas were coupled to

[42] A good representative of this new vision is the Prospectus of the Royal Institution (1800): "Invention seems to be peculiarly the province of the man of science; his ardour in the pursuit of truth is unremitted; discovery is his harvest; utility his reward." *The Prospectus, Charter, Ordinances and Bye-laws of the Royal Institution of Great Britain* (London: Royal Institution, 1800), 6. Searches of digitized books and other sources reveal that the term "man of science" in English indeed dates from the end of the eighteenth century, as does the phrase "scientific discovery" (in this view, the coinage of the word "scientist" in 1833, and its very slow adoption, is more of an Anglophone linguistic peculiarity).

[43] The *term* "natural philosophy" did, however, survive for rather longer. On the shift see Simon Schaffer, "Scientific Discoveries and the End of Natural Philosophy," *Social Studies of Science* 16 (1986): 387-420.

[44] This shift can certainly be related to the rise of "mechanical objectivity" detailed in Lorraine Daston and Peter Galison, *Objectivity* (New York: Zone Books, 2007), although Daston and Galison offer a different chronology that dates the emergence of mechanical objectivity to around the mid-nineteenth century. In any case the redefinition of approaches to nature between the late-eighteenth and mid-nineteenth centuries was certainly a drawn-out and highly complex process that can be viewed from many historical angles; for another insightful discussion see James Secord, *Visions of Science: Books and Readers at the Dawn of the Victorian Age* (Chicago: University of Chicago Press, 2015).

[45] It is for this reason that I do not use the phrase "Republic of Letters" to refer to the communities of people I study; to do so would be to apply a term whose referent was at that very moment losing its coherence. With this choice of terminology I certainly do not want to downplay the continuing importance of traditions of communication with roots in the early modern world—especially exchanges of letters, which remained crucial even with the rise of new specialist print media. Nor do I want to deny that the *ideal* of a broad Republic of learned scholarship still had influence, especially for men of letters of the older generation such as Sir Joseph Banks, a dedicated antiquarian as well as President of the Royal Society. But even if the concept was sometimes retained as a rhetorical device, it is seems clear that the disruptions of 1789-1815 represented a decisive break. As Elise Lipkowitz has shown, the term "Republic of Letters," previously rooted in a broad cosmopolitan unity, increasingly became a rather thin rhetorical cover draped across more important national and political allegiances. Elise Lipkowitz, "'The Sciences Are Never at War?': The Scientific Republic of Letters in the Era of the French Revolution, 1789-1815" (PhD diss., Northwestern University, 2009); Elise Lipkowitz, "Seized Natural-History Collections and the Redefinition of Scientific Cosmopolitanism in the Era of the French Revolution," *British Journal for the History of Science* 47 (2014): 15–41.

practical laboratory technique rather than broad "philosophical" knowledge. Second, the emergence of a vision of science as a special route to intellectual and material *progress*, through a certain process: "discovery." Third, the increasing importance of science to the state. Fourth, the success of scientific practitioners in enlisting laypeople as public audiences and patrons for what they were doing. And fifth, the great sea-change in western culture known as Romanticism, which in some of its forms promised to fuse natural science with other modes of thought but which ultimately ended up driving a wedge between them.

Yet the transition from the Republic of Letters to "science" is also linked to quite specific shifts I trace in the practice and structure of communications. In newspapers and periodicals, an influx of information about "discovery" in galvanism and other fields helped cement the link between scientific knowledge and progress. Science's rising profile in news and periodical print marked it out as up-to-the-moment: a stream of individual day-to-day encounters with nature in the laboratory rather than reflective learned scholarship. At the international level, the disruptions of war and blockade played an important role in restructuring the intellectual landscape as well. In this process, I argue, the crucial factor was *not* the obvious fact that men of science experienced disruption in their communications. The real point of interest is the comparative *resilience* and *success* of international science in holding together amidst those disruptions—though certainly with many rifts and delays. Among the many agents that worked to make this possible were belligerent states themselves. As we will see, rather than preventing scientific discoveries from going abroad, both the British and French states quietly went to work enabling the flow of scientific intelligence across borders. Other kinds of scholarship—classical studies, moral philosophy, history, antiquarianism, theology, and so on—were left to fend for themselves, and seem to have had little success, or perhaps motive, in keeping up strong

international links across wartime frontiers. In contrast, scientific practitioners—especially specialist communicators like journal editors or letter-writing go-betweens—went to great lengths to keep in touch. They did this because of collegiality, cross-border friendships, and because scientific discovery was increasingly felt to represent a general benefit to humanity—but also for the less noble-minded but eminently practical reasons that keeping up-to-date on rivals' investigations, and distributing that information, helped science at home and furthered their own careers. All this marked out scientific information on the move as *special*: specially deserving of mobility. Precisely when the products of science were increasingly bound up with geopolitics and national rivalry, science emerged as the archetypal transnational intellectual endeavor.

This dissertation is explicitly framed as a transnational project, but one in which the object is not to deemphasize national borders in scientific exchanges but rather to make the role of those borders an *object* of historical inquiry. It is partly for this reason that, although the narrative shifts back and forth to France, and also at times to Germany, Italy, and the United States, it is primarily anchored in Britain.[46] The separation between Britain and the rest of Continental Europe created by the long conflict between Britain and France was the strongest and most persistent geopolitical fault line during these two decades of war.[47] If information crossed that fault line—or if a person did, like Humphry Davy when he was rowed from a British ship to a French shore on that October night in 1813—the position of that piece of information or

[46] An additional reason is the distinctive course galvanism took in Britain. It was in Britain that the link between galvanism and chemistry was most actively pursued, and by about 1808, in part thanks to Humphry Davy's discoveries and his creation of a new field, "electro-chemistry," Britain was seen across Europe as leading the way in galvanism in an age where Paris dominated other areas of science. Thus the trajectory of galvanism in Britain is central to its wider European story, although, as I emphasize here, it was fundamentally shaped by the movement of information back and forth between Britain and the Continent.

[47] Of course, the degree to which Britain was connected or cut off from different parts of the Continent—France, the Low Countries, northern Germany, Italy—varied across this period, in ways which mattered deeply for the movement of scientific information. And during the Peace of Amiens in 1802-03, of course, there was free flow of information and people even between Britain and France. In the chapters that follow I trace some of the consequences of this temporal and spatial variation.

that person within European science changed completely. Rooting the analysis in Britain—with frequent narrative excursions to France, the other country to which I pay particular attention—allows us to recover a historical picture of the divide and its role in shaping scientific communication across this period when London and Paris were the two great centers of the scientific world.[48]

Of course, my aim is to track *connections* between Britain and the European Continent, as well as disconnections.[49] Consequently I envision this dissertation as aligning with recent currents in British history that stress that Britain's connections with Continental Europe in the eighteenth and nineteenth centuries warrant attention as much as the connections between Britain and its overseas Empire. This is not to deny the importance of the Empire in British History, but rather to say that links with *both* Empire and Europe counted in forging the national identity of Britain and determining its place in the world.[50] Therefore, although Britain was *self-consciously*

[48] Another more practical reason for choosing Britain as the place to begin tracking these connections concerns the survival of archival sources: the preservation of manuscripts like Charles Blagden's letters and extraordinary diary at the Royal Society (sources absolutely unique in their usefulness for this project), and also Humphry Davy's lectures, laboratory notes and other papers at the Royal Institution, make it possible to reconstruct the movement and use of intelligence in galvanism from the British point of view on a level of detail simply not possible for other people and places. The survival of the papers of Continental men of science is generally much worse than for their British counterparts; one notable exception is the letters and papers of the Genevan savant Marc-Auguste Pictet, the published volumes of which I have also consulted extensively.

[49] It must be acknowledged that "Britain" and "France" here often mean simply London and Paris, the overwhelmingly dominant sites for scientific activity in the two countries. I do pay occasional attention to differences between London, northern England, and Scotland when these matter for the movement of scientific information—a topic which could benefit from much more sustained attention (as could the relationship between Paris and science in the French provinces during this period of centralization in the French state).

[50] Similarly, I certainly do not mean to downplay the important relationship between Empire and science in Britain around 1800. While intra-European links were clearly far more consequential than links with the rest of the world in shaping the intellectual content and practice of galvanism and chemistry during this period, in other areas of science—above all, natural history—the reverse was true. A thriving historiography exists on the ways botanical information and specimens circulated between Britain and its overseas Empire in the Caribbean, Australasia, Africa, and elsewhere around 1800, a story involving several of the historical actors who feature in my narrative—particularly Sir Joseph Banks. See especially Richard Drayton, *Nature's Government: Science, Imperial Britain, and the "Improvement" of the World* (New Haven: Yale University Press, 2000); John Gascoigne, *Science in the Service of Empire: Joseph Banks, the British State and the Uses of Science in the Age of Revolution* (Cambridge: Cambridge University Press, 1998). Astronomy was also a natural partner of British overseas expansion and maritime power in the early nineteenth century: John McAleer, "'Stargazers at the world's end': telescopes, observatories and 'views' of empire in the nineteenth-century British Empire," *British Journal for the History of Science* 46 (2013): 389-413. On the global connections of science in this period generally see the essays in Simon Schaffer et al., eds., *The*

different in the Napoleonic period—with Britons glorying in the idea of themselves as a free maritime nation standing against Napoleon's tyrannical land-based Empire—this difference was elaborated through connection as well as disconnection.[51] And, turning this point around, because science was one of the few cultural activities which *did* retain some coherence across the English Channel during the long wars (increasing scientific competitiveness between London and Paris being quite compatible with that coherence) we might expect to find science harnessed to emphasize national *difference* as well. And so it was, as we will see: men of science like Davy were perfectly capable of deploying British discoveries in galvanism as part of a rhetoric of jingoistic nationalism in public and then returning home and penning friendly letters to Paris. Britain and France were two nations bridged, as well as divided, by a common scientific endeavor.

Finally: although I believe there are good reasons for laying a particular emphasis on Britain here, this is in part simply a narrative choice, and the story of galvanism and scientific communication could certainly be told using another scientific center like Paris or Berlin or Milan as the point of departure. That would be a different story, but one whose conclusions would—I hope—be complementary to and mesh closely with mine. In this the historiographical problem of narrative choice and geographical focus mirrors the historical reality of doing science in the Napoleonic world. There was no neutral point from which historical actors could view and assess scientific information on the move—a statement more or less true of all places and times, but *especially* true for this turbulent period. The effects of war, blockades, distance, and a multitude of local and personal factors created a tangle of asymmetries of access to particular

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Brokered World: Go-Betweens and Global Intelligence, 1770–1820 (Sagamore Beach, MA: Science History Publications, 2009).

[51] On the importance of the contrast with France in shaping British identity see Linda Colley, *Britons: Forging the Nation, 1707-1837* (New Haven: Yale University Press, 1992).

pieces of information, and differences of timing in when they were received—in print, in letters, and through the spoken word—so that each place and even each person necessarily had a different experience of the stream of information about galvanism in the years between 1790 and 1830. Yet all these individual experiences were interconnected, and meshed together. In the pages that follow, I aim to provide a picture not only of how European scientific information worked at the macro-level, but also how it looked to people struggling in the thick of things to solve practical problems. By starting these dead alive and reconstructing their experiences—partial, imperfect, fragmented, real, and human—we can recapture something of the historical meaning of information before the Information Age.

* * *

The chapters follow a chronological arc, with each one built primarily around episodes in galvanism drawn from a particular slice of the dissertation's overall period. (Within each chapter, I do sometimes jump back and forth in time.) The first and last chapters look at different forms of communication as they functioned together in an interconnected system. The middle chapters (two, three, and four)—while still keeping these interconnections in view—zoom in on the workings of newspapers, journals, and manuscript letters, respectively.

Chapter 1 begins with a pivotal event in the history of science and technology, the announcement of Alessandro Volta's galvanic battery in 1800. I use the spread of the battery to introduce a crucial feature of communications at this time: the distinctive culture of reprinting that mediated science before conventions of publication hardened later in the nineteenth century. Beginning with the first printed description of the battery in a London newspaper, I trace information about the device backwards to Volta's first letters penned in northern Italy to President of the Royal Society Joseph Banks, and forward as it spread across Europe in a mixed

cascade of letters, journals, magazines, and newspapers. I argue that techniques of communication around 1800 were characterized by a ferment of creative experimentation, in everything from specialist scientific publishing to the ways ordinary laypeople encountered news. Tracking information across different media, I show how writers and editors copied and reused information from text to text, creating a "periodical commons" communication system, grounded in the process of taking information from one place and reprinting it in another.

Chapter 2 focuses particularly on newspapers, whose level of scientific content was increasing at this time. Using the story of the sensational and controversial experiments of Giovanni Aldini (a source of inspiration for Mary Shelley's *Frankenstein*), which he exhibited in London and Paris during the Peace of Amiens (1802-03), I show how information about science in the newspapers was shaped by broader cultures of news and political life. In Britain this was linked to the advent of a new role for science in the public arena, in venues like public lectures and elite social gatherings. In France, the trend was part of Napoleon's efforts to co-opt the sciences as politically-safe sources of "glory" for the French nation and Empire.

Chapter 3 takes a close look at the new fast-paced scientific journals that appeared around the end of the eighteenth century. Taking up the story of galvanism after the resumption of war in 1803, I recover a largely-forgotten controversy over the action of galvanism on water that took over the European journals during 1805-1806. I show how these still rather low-status publications, closely related to the newspapers, brought practices of periodical print into the day-to-day pursuit of experimental discovery. Galvanism and the new journals evolved together, with the demand for information about galvanic discoveries driving the rise of the journals and shaping how they worked, just as the journals influenced the direction of galvanism and

chemistry by connecting previously-isolating researchers and reconfiguring the relationship between print and experimental practice.

Chapter 4 discusses communication in letters, focusing on links between Britain and France during the period when Britiain was most isolated from Continental Europe, Napoleon's "Continental System" blockade (1806-1814). This was also precisely the time of the most intense Anglo-French rivalry in galvanism, in which Humphry Davy in London and the young French chemists Gay-Lussac and Thenard in Paris used the galvanic battery to forge a new science, electrochemistry. Using the exceptionally rich archive of a key mediator between Britain and France, Sir Charles Blagden, I reveal how men of science worked behind the scenes to keep information moving back and forth between the two sides: they harnessed connections with neutral American diplomats, merchants and bankers, and even government ministers. Nonetheless, I show, this private communication in letters was not an isolated "correspondence network," but interacted and overlapped with information in print in crucial ways.

Finally, Chapter 5 takes the story through the coming of European peace in 1815, focusing especially on flows of information during an episode that forms a point of inflection between the galvanism discussed in previous chapters and the modern science of electricity: the announcement in 1820 of Hans Christian Ørsted's discovery of electromagnetism. This final chapter demonstrates how, although the end of war removed many barriers to international exchanges in science, it did not bring an end to the members of the scientific community's concern with problems of information. During the two and a half war-torn decades between the French Revolution and Napoleon's fall, so many changes had occurred in the dynamics of scientific information exchanges, in the media in which they were conducted—and even in the identity of science itself within the wider European intellectual landscape—that the members of

the scientific community had no way back to the old Republic of Letters as it had been when the Estates-General gathered at Versailles in May 1789. Far from ushering in a return to a pre-war normality, the coming of peace posed new questions and problems concerning the organization and future of scientific communications in an increasingly interconnected European world.

Chapter 1: Dynamics of Scientific Information in the Napoleonic World

> "There are periods when the sciences are cultivated with more ardor and success than at other times; these are when, after the passing of many years without a remarkable discovery, all of a sudden there comes a dazzling breakthrough. Then all minds are stirred and, quickened by a need to find new truths, they search for them avidly, and almost always they find them. What a stir the discovery of the voltaic pile produced! What enthusiasm it awakened! Its appearance made the already illustrious name of Volta immortal. Everywhere, piles were constructed; in every form they multiplied. Savants studied their marvelous effects at every opportunity; societies were founded with the sole aim of observing them; the whole world desired to be acquainted with them. In this general contest, some perfected this admirable instrument; others inquired into the laws of the fluid to which it gave motion, or examined the action of this fluid on the elements of bodies. Some perceived its agency in the bosom of the earth, and thus explained several phenomena which occurred within; others bolder still compared it to the fluid which seems to animate all living beings…"
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> —— Joseph Louis Gay-Lussac and Louis Jacques Thenard, *Recherches physico-chimiques* (1811).[1]

Our journey begins in a newspaper. Imagine opening the London *Morning Chronicle* on 30 May 1800. Conspicuous at the top of the page inside is a three-paragraph block of scientific news. The first paragraph contains a set of instructions. All we need to begin are some silver half-crown coins and some discs of zinc and cardboard of the same size. The newspaper describes a recent demonstration: "a piece of zinc was then laid upon the table, and upon it a half-crown; upon this was placed a piece of card moistened with water; upon the card was laid another piece of zinc, upon that another half-crown, then a wet card, and so alternately till more than forty pieces of each had been placed upon each other."[2] This simple description is the first printed information given to the world of how to build a battery. Follow it and you will have a replica of the Napoleonic Age's iconic scientific instrument, the Voltaic pile.[3]

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[1] Joseph Louis Gay-Lussac and Louis Jacques Thenard, *Recherches physico-chimiques* (Paris: Deterville, 1811), 1:vii-viii.

[2] *Morning Chronicle*, 30 May 1800, p. 3. A half-crown (2 shillings and 6 pence) had a diameter of around 34mm (1.3 inches).

[3] Here and subsequently, I used the word "battery" in the modern sense of an electrochemical battery. This usage (the word "battery" as shorthand for "galvanic battery" or "Voltaic battery") entered the English language soon after the advent of Volta's pile. Previously, beginning with Benjamin Franklin in 1748, the term had been used in eighteenth-century electricity to denote a set of Leyden jars connected together; it was derived by analogy between the combined firing of artillery batteries and the combined discharge of the electric spark from a "battery" of jars. In French, Volta's battery was generally referred to as "la pile," or sometimes "l'Electromoteur"; in German "Säule" [column] was used, along with "die galvanische Batterie." In Italian Volta originally called the device an "organo

The *Chronicle* goes on to explain what this new object can do. It provides a strong and continuous source of galvanic current, as anyone brave enough to receive an electric shock by touching the top and bottom with wet hands will confirm. And if the current is applied to water with copper wires, then the water will decompose, with hydrogen gas given off at one wire and the other tarnishing as it combines with oxygen. This decomposition of water (only twenty years earlier still considered an element) suggests a connection between galvanic electricity and the chemical forces that bind matter together. But what is the nature of this connection, and what other wonders might this apparatus reveal? The newspaper does not tell us. Only more experiments will.

The block of text that appeared in the *Morning Chronicle* at the end of May 1800 was initially intended for a local London audience. But it would spread across Europe and even beyond: the same text reappeared in other publications (in French as well as English) in places like Paris and Brussels, Milan and Saxony, Switzerland and the United States. Some readers used it to build Voltaic piles and to begin their own experiments. Others had the first news in different ways, through letters, journals, announcements at scientific societies, public lectures or private conversations, so that the newspaper account was woven into a mass of criss-crossing and interconnected paths on which information about the battery travelled. For a few, the encounter was transformative. "I experimented with the battery from morning to evening," recalled Henrich Steffens, who learned about it while a student at Freiburg (he would end his career as a Professor at the University of Berlin). "There is always something poignant," he remarked, "in the early days of progress just after a great scientific discovery." His room was crowded with onlookers; the pile "was new and interesting to everyone," not just to those dedicated to science. It made a

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elettrico artificiale" and "L'apparato elettro-motore"; he also used the word "colonna" to refer specifically to the stacked pile configuration.

"deep impression" that remained with him for the rest of his life.[4] No less touched by the battery's magic was another young scientific disciple, Humphry Davy. Upon hearing the news in Bristol, he too immediately built a pile and began a course of experiments. And like Steffens, he hastened to send his new results to the editors of monthly journals, both to claim them as his own and so fellow experimenters could read and expand upon them. Many others did the same. The barrier to launching investigations was tantalizingly low; the pile was easy to assemble and its possibilities seemed vast. Never, it appeared, had a new scientific discovery been as quickly followed by such feverish activity. It was, Davy proclaimed, "an alarm-bell to experimenters in every part of Europe."[5]

In this chapter the spread of information about the battery provides our itinerary for a tour of the scientific world in 1800. What was scientific information? How and where did it travel? In what media? Who controlled it? How did it relate to print and publication? And how did people put it to use in their day-to-day work? All these questions come into sharp focus when we follow intelligence about the battery, not because the battery was typical science in 1800, but because it was exceptional. It spread widely and quickly, disrupting established procedures for announcing new discoveries. It was interesting to different constituencies: natural philosophers, chemists, medical men, and ordinary readers of newspapers and magazines. People immediately identified it as an important new advance as soon as they heard about it, placed a premium on getting more information, and felt impelled to pass on their information to others. Consequently it has left many traces in historical sources, and the movement of intelligence about it can be tracked with

[4] Henrich Steffens, *Was ich erlebte: aus der Erinnerung niedergeschrieben*, vol. 4 (Breslau: Josef Max, 1841), 271-73.

[5] Davy made this remark in a public lecture given during his first years at the Royal Institution, probably in 1801 or 1802. The quote is preserved in RI MS HD/1/A/2, which is a later lecture text assembled from sheets of earlier lectures; the likely date of the original can be deduced from deleted passages.

unusual precision. It will be our tool for plotting a working map of the European system of scientific communication, balanced on the cusp of the nineteenth century.

Using *information* to trace the lines on our map opens an alternative perspective on the nature and function of scientific texts around 1800. Despite the changes wrought in modern scientific communications by the advent of electronic media and the Internet, one constant has been the expectation that scientific practitioners will engage with claims or discoveries through copies of a single document, carefully prepared as an official statement of new findings—a scientific paper. The release of this text into the world constitutes a certain hallowed moment, which we call "publication."[6] Similarly, a thriving historiographical tradition has traced the spread of new scientific claims, ideas, or discoveries through the studies of the content and, more recently, the consumption of key original printed works, from Copernicus's *De revolutionibus* to the anonymous *Vestiges of the Natural History of Creation*.[7] Historians are now comfortable assuming that readers create their own meanings and uses for published texts, and the untidy printing-house processes by which people other than the author partook in their production have been opened up to view. [8] Even the ways print travelled to actually get to readers are beginning

[6] The online preprint server arXiv.org does make available multiple versions of articles, with each version date-stamped, so that the model of a single text has now been slightly eroded. But the *announcement* of the claim still happens through the appearance of a single, first version of the document, which usually only receives minor emendations in later versions. And there is a sense in which the new preprint server system actually strengthens the reigning model, by creating a very sharply-defined moment when a text becomes public and universally available to anyone with an Internet connection, anywhere in the world. By cutting out almost all the apparatus of journal editors, reviewers, printers, and postal services familiar from twentieth-century scientific journal publication, the preprint system allows authors to control extremely precisely the timing how their texts become public (the interval between a document being submitted by the authors and the moment when it is made publically available is ordinarily a matter of hours).

[7] Owen Gingerich, *An Annotated Census of Copernicus' De Revolutionibus* (Leiden: Brill, 2002). James Secord, *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of* Vestiges of the Natural History of Creation (Chicago: University of Chicago Press, 2003).

[8] Classic approaches to the history of reading are Robert Darnton, "Readers Respond to Rousseau," in *The Great Cat Massacre and Other Episodes in French Cultural History* (New York: Basic Books, 1984), 214-56, and Lisa Jardine and Anthony Grafton, "'Studied for Action': How Gabriel Harvey Read His Livy," *Past & Present* 129 (1990), 30-78. On the world of the early modern print house see Adrian Johns, *The Nature of the Book: Print and Knowledge in the Making* (Chicago: University of Chicago Press, 1998).

to receive attention.[9] But there usually remains a lingering assumption. When studying how print mediated the movement of new science (and other knowledge) in the past, we tend to take it for granted that the moment when copies of a certain carefully-prepared text are printed—a moment of "publication"—really does constitute a well-defined point on which to pivot our discussion. We take this moment to be a fulcrum between writers and readers, between production and reception. Even the powerful argument made by Adrian Johns that the trustworthiness or fixity of printed productions was contingent and required local work to establish is built around the idea of particular important texts as the bearers of knowledge, because it is precisely the potential for those objects to be *counterfeited* that demonstrates the need for social systems of trust to invest print with reliability.[10] The picture remains: an individual making a new claim creates a document, and printed copies fan out from a centralized publication mechanism into the wider world.[11]

This is sometimes a good model of how information has historically spread in print—it approximates editions of Newton's *Principia*, Darwin's *Origin of Species*, and twentieth -century scientific journals. But it is not always a good model. In the following pages I seek to chase down the kinds of texts that actually counted in scientific communication across distance during the battery's early career. The aim is to recover a world in which print mattered in scientific practice in ways that have very little to do with "publication" as we understand it. In fact, men of

[9] Jeffrey Freedman, *Books without Borders in Enlightenment Europe: French Cosmopolitanism and German Literary Markets* (Philadelphia: University of Pennsylvania Press, 2013); see also Jonathan Topham, "Science, Print, and Crossing Borders: Importing French Science Books into Britain, 1789-1815," in *Geographies of Nineteenth Century Science*, ed. David Livingstone and Charles Withers (Chicago: University of Chicago Press, 2011), 311-44.
[10] This is, of course, exactly the point of Johns's argument: if even canonical scientific books by the great figures of the scientific revolution can be shown to be infected by the threat (or reality) of counterfeiting and piracy, then, so the argument runs, *all* print needs social systems of trust if it is to be invested with credibility. This plausible conclusion, however, tells us nothing about how these systems of trust might work *differently* in various other kinds of print media where confidence in the provenance and reliability of the information they contain generally remains rather low, such as periodicals and newspapers around 1800.
[11] Even if the text may in fact appear anonymously.

science around 1800 often did not engage with a new scientific discovery or claim via a standard text prepared by the discovery's originator—especially when that person was far away. Nor did they even necessarily *expect* to. In the case of Volta's battery, although information about the discovery was overwhelmingly spread through print (though letters were also crucial), *there never was really any official printed announcement of the discovery of the battery*. Certainly, Volta did get his account published in the *Philosophical Transactions*, but this appeared months after the discovery was already widely known. It was no news to anyone: well before it had even been printed it had been superseded by other investigations. Crucially, nobody—including Volta—was seriously bothered by this. Volta's famous *Philosophical Transactions* paper is therefore exactly the wrong place to look if we want to understand the battery's early career. Historians make a virtue of using original sources; historical actors were less scrupulous—they were looking forwards, not back. They used what came to hand.

Seeking out what sources of information people really did use when working with the battery will give substance to my broad claim that scientific communications in the early nineteenth century involved a fluid and hybrid mix of different media and methods. Information spread through a range of channels: newspapers like the *Morning Chronicle*, but also specialist monthly journals, manuscript letters, magazines, public lectures, private conversations. What emerges on the large scale is a great echo chamber in which information regularly outran the control and even the consciousness of its creators. Through processes like reprinting, editing, translation, reporting on lectures or meetings, writing letters to friends about news they had heard or publications they had seen (and printing these letters in turn), men of science and other individuals collectively moved information along. All kinds of media were entangled with and

borrowed from one another, weaving together the phenomenon historians often refer to as "reception."

Working with the battery in 1800 meant participating in an ongoing process of interacting with various pieces of information from sources near and far, which were (provisionally) invested with various levels of trust. Crucially, much of this information was derivative or secondhand, copied or reprinted from elsewhere. This was inescapable: it was precisely how people around 1800 experienced all kinds of news and intelligence about distant things. One of my aims in this chapter is therefore to reconstruct what it was actually like in this period to deal with information from far away. By examining the nature of news and intelligence in Napoleonic Europe, I uncover the hermeneutic of distance that was common to many experiences, from reading foreign items in newspapers to reconstructing experiments performed by colleagues or rivals. For individuals pursuing galvanism, dealing with information across distance could be frustrating, creating gaps and ambiguities about what others had done or claims they had made. Yet if intelligence sometimes arrived in incomplete or distorted fragments, there was always the expectation that more details would arrive later, clarifying problems or hinting at new lines of enquiry to be opened up. Using whatever information came to hand as well as their own ingenuity, this scattered community reworked Volta's original discovery and applied it in startling new ways. Many of them in turn sent their own information out into the world, to be consumed by and to influence others. The result was a proliferation of interpretations and uses for the new object.

An Ambiguous Object: The Challenge of the Battery

It would be hard to overstate the long-term importance of the invention of the battery. Time and again, the appearance of Volta's pile—precisely at the turn of the nineteenth century—has been represented as a point of transition, one of the key staging-posts on the road to modernity.[12] Such views have a long history. As early as 1833, the leading French savant François Arago proclaimed that Volta's pile had brought about "a scientific revolution," calling it "the most marvelous instrument ever devised by man, not excepting the telescope or the steam engine."[13] Each generation since has sought the roots of its own electrical miracles in the Ur-moment represented by the advent of the battery. For late-Victorian commentators, living in a world where copper wires linked London to Calcutta and Paris to San Francisco, Volta's discovery had unleashed a power whose evident culmination was in the global electric telegraph.[14] It has remained central to genealogies of how electricity made the modern world.[15]

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[12] John Heilbron lays out this god's-eye view in some terse comments at the end of his magisterial history of electricity in the seventeenth and eighteenth centuries, which concludes at the moment of the pile's invention: "The pile was the last great discovery made with the instruments, concepts, and methods of the eighteenth-century electricians. It opened up a limitless field. … Its steady current provided the long-sought means for establishing a relationship between electricity and magnetism. The consequent study of electromagnetism transformed our civilization." John Heilbron, *Electricity in the 17th and 18th Centuries: A Study of Early Modern Physics* (Berkeley: University of California Press, 1979), 494. Such statements may sound faintly teleological to historians of science trained to detect "Whiggism" and other presentist errors, but in fact they are only so if they are used to make a transhistorical statement about the "true meaning" of Volta's discovery which is then applied to the interpretation of what happened in the early nineteenth century—which is not Heilbron's intention. The deeper point is that we are ourselves participating in a long-term historical process of reinterpreting what the first battery might mean *to us*. Volta's battery is iconic now, was iconic during the later nineteenth century, and, as I show in this chapter, it quickly became iconic during the complicated process by which it was announced and spread—all for different reasons.

[13] Arago's statement was made *before* electricity had any transformative impact on society (both the Morse and Cooke-Wheatstone telegraphs were developed in 1837 and commercialized in the 1840s), but several years *after* the first steam trains had begun to run in Britain! François Arago, "Eloge historique d'Alexandre Volta," *AC* 54 (1833): 420, 427-28.

[14] See for example John Fahie, *A History of Electric Telegraphy to the Year 1837* (London: Murray, 1884), 169: "The discoveries of the Italian philosophers, Galvani and Volta, at the close of the last century, marked a new era in the history of telegraphy, by furnishing a form of electricity as tractable and copious, as that derived from friction was volatile and small."

[15] Interest in this moment goes right back to the beginnings of the history of science profession with George Sarton, whose brief study of the subject ranked the discovery of the battery above even the invention of the telescope and the microscope, because "the electric cell was really a creative instrument; it opened to man a new and incomparable source of energy." George Sarton, "The Discovery of the Electric Cell," *Isis* 15 (1931): 124-57. See also Bern

But Volta's contemporaries, of course, had no idea where the discovery of the pile would lead. We must try to put ourselves in their position. This is particularly necessary because the battery was in fact greeted with almost immediate acclaim, even in the very earliest days. "Volta's Galvanic battery ranks as one of those few discoveries that mark an epoch in the history of natural philosophy," boldly declared the young Johann Wilhelm Ritter in a letter to the *Allgemeine Literatur-Zeitung*, the foremost learned newssheet of Germany.[16] He wrote those words in October 1800, only a few weeks after he had first built his own pile. All around Europe, Volta's creation was hailed as a discovery of great, even epoch-making, importance—but amidst extraordinary uncertainty about what, exactly, had been discovered. The first news of the battery was no tidy announcement, but the starting gun in the general contest to define the meaning and pursue the consequences of a multivalent and ambiguous scientific object.[17]

Recent scholarship on the early history of the Voltaic pile has revealed this indeterminacy: in the path to the battery's invention, in how it was interpreted, in how it was applied, and even in its material form.[18] Initially, the pile was received as a new *experiment*, and much work was involved in turning it into an *instrument* of scientific discovery that could be unproblematically applied to further investigations.[19] Even then, mysteries remained. Several key questions posed very early on had to wait, sometimes a very long time, for definitive answers.

Dibner, *Galvani-Volta: A Controversy that Led to the Discovery of Useful Electricity* (Norwalk: Burndy Library, 1952).

[16] *Allgemeine Literatur-Zeitung, Intelligenzblatt*, 1 November 1800, no. 180, col. 1509.

[17] On scientific objects as nodes of meaning see Lorraine Daston, ed., *Things That Talk: Object Lessons from Art and Science* (New York: Zone, 2007).

[18] See in particular, Giuliano Pancaldi, *Volta: Science and Culture in the Age of Enlightenment* (Princeton: Princeton University Press, 2003), 178-256, and Giuliano Pancaldi, "On Hybrid Objects and their Trajectories: Beddoes, Davy, and the Battery," *Notes and Records of the Royal Society* 63 (2009), 247-62. Also Geoffrey Sutton, "The Politics of Science in Early Napoleonic France: The Case of the Voltaic Pile," *Historical Studies in the Physical Sciences* 11 (1981): 329-66 (which emphasizes the divergent interpretations and uses of the battery in Britain and France); Nahum Kipnis, "Debating the Nature of Voltaic Electricity, 1800-1850," in *Nuova Voltiana: Studies on Volta and his times*, vol. 3, ed. Fabio Bevilacqua and Lucio Fregonese (Milan: Hoepli, 2000), 121-51; the five *Nuova Voltiana* volumes contain many other useful essays touching on the diverse responses to Volta's work.

[19] See especially Golinski, *Science as Public Culture*, 203-218.

First, it was initially an open problem whether the electricity from the battery was the same stuff as the "common electricity" from static electrical machines. This was not seriously doubted for very long, but never really absolutely resolved, so that the matter was even revisited by Faraday as late as the 1830s.[20] Second, although the very first printed announcement of the pile in the *Morning Chronicle* contained a description of how it had been used to decompose water into hydrogen and oxygen, repeated and serious experimental challenges to even this supposedly basic observation were presented over the next few years.[21] Third, even after the decomposition of water was accepted, arguments over how precisely electricity acted to split apart matter in this fundamental experiment continued right through the nineteenth century. Fourth, there was the issue of how the internals of the battery actually worked: was the source of the current in the contact between metals, as Volta thought, or in chemical reactions within the pile, or in some combination of the two? This was really a new version of the oldest question of all in galvanism, the original Galvani-Volta controversy on the source of "animal electricity" in experiments with frogs, and no lasting consensus was *ever* achieved on the question, at least until twentieth-century science quietly buried this once-great matter by reducing the two sides to equivalent schemes of explanation.[22] Finally, a sizeable contingent, led by Galvani's flamboyant nephew Giovanni Aldini, still held to a variation of Galvani's original idea of a separate "animal electricity" in living beings, and fought a strong rearguard action in which they appropriated the pile as evidence for their claims.[23] Their efforts, as well as the widespread use of the battery in a

[20] Michael Faraday, "Experimental Researches in Electricity, Third Series," *Philosophical Transactions* (1833), 23-54.
[21] See Chapter 3.
[22] Helge Kragh, "Confusion and Controversy: Nineteenth-Century Theories of the Voltaic Pile," in *Nuova Voltiana: Studies on Volta and his times*, vol. 1, ed. Fabio Bevilacqua and Lucio Fregonese (Milan: Hoepli, 2000), 133-57.
[23] See Chapter 2.

variety of medical therapies, ensured that associations between galvanism and vital forces persisted for a long time—especially among lay audiences.[24]

This atmosphere of contending opinions and unanswered questions heightened the excitement surrounding the pile. Before the battery, galvanism had been a difficult and rather arcane subject, of great interest to experienced electrical or medical researchers but rather forbidding to new recruits. Galvani's original experiments on frogs required delicate anatomical manipulations, and were embedded in complex discussions about the nature of nerves, muscle, and vital fluids.[25] The famous Galvani-Volta debate on the source of the "animal electricity" in these experiments (did it come from the frogs, or from the metallic conductors used to connect nerve with muscle?) had hinged on the presence or absence of almost undetectable electric charges.[26] Conceived by its inventor as a master-stroke in this debate, Volta's battery in fact marked a new beginning. It was easy to build, it required little prior knowledge of difficult techniques before experiments could commence, and it made available a steady and continuous source of electricity for the first time. Consequently it appeared as the *entrepôt* to a vast terrain, in which new discoveries lay just over the horizon, awaiting the scientific traveler willing to forge ahead. "Volta has presented to us a key which promises to lay open some of the most mysterious recesses of nature," Humphry Davy later told a lecture audience at the Royal Institution. "Till this discovery, our means were limited, the field of pneumatic research had been exhausted, and little remained for the experimentalist except laborious and minute processes.

[24] The history of the 1790s' work in "animal electricity," often glossed as merely a prelude to the invention of the battery (a reading which in fact dates back to soon after the invention of the battery itself), has been reassessed by Marco Piccolino and Marco Bresadola as a series of primarily physiological arguments: see Marco Piccolino and Marco Bresadola, *Shocking Frogs: Galvani, Volta, and the Electric Origins of Neuroscience* (Oxford: Oxford University Press, 2013), the English version of their *Rane, Torpedini e Scintille. Galvani, Volta e l'elettricità Animale* (Turin: Bollati Boringhieri, 2003); compare Marcello Pera, *The Ambiguous Frog: The Galvani-Volta Controversy on Animal Electricity*, trans. Jonathan Mandelbaum (Princeton: Princeton University Press, 1992).
[25] No scholarly biography of Galvani exists in English. The standard in Italian is Marco Bresadola, *Luigi Galvani: devozione, scienza e rivoluzione* (Bologna: Compositori, 2011).
[26] These debates and the relevant historiography are discussed in the next chapter.

There is now before us a boundless prospect of novelty in science; a country unexplored, but noble and fertile in aspect; a land of promise in philosophy."[27]

A broad community of investigators soon cohered around the battery, seeking to use it as the means to new discoveries in chemistry, natural philosophy, physiology, or medicine. In their individual intellectual journeys, these individuals may indeed have felt themselves to be voyaging through a land of promise, but it was war-torn Napoleonic Europe in which they had to live and communicate with one another.

Recovering the Experience of News

Few people opening the *Morning Chronicle* on 30 May 1800 would have begun their reading with the description of the battery. Almost everyone would have gone straight to the foreign news. The day before, the *Chronicle* and other London newspapers had reported that First Consul Napoleon Bonaparte had taken an army over the Alps at the St. Bernard Pass, an audacious strategic gamble that might well transform the war between France and Austria in Northern Italy and Bavaria.[28] Readers urgently scanning the foreign sections of the paper for more information on this and other great events happening far away would have found, as usual, a collection of snippets of intelligence from a variety of places, each labeled with a location, and, crucially, a date. Among them were Paris (25 May), Strasbourg (24 May), Berne (20 May), Frankfurt (18 May), Augsburg (15 May), Berlin (14 May), Milan (3 May), Turin (3 May), and Constantinople (14 April).[29] The dates served as a kind of timestamp, specifying the freshness of

[27] RI MS HD/2/D/1 (lecture in March 1808).
[28] *Morning Chronicle*, 29 May 1800. The passage of troops at the St. Bernard had been reported previously, but not Napoleon's presence, which was unexpected. Napoleon's dispatch to Paris from the Italian side of the Alps travelled fast: it was written on 18 May, and printed in the Paris newspapers on 24 May; these were received in London on 28 May, and the text was in print there in English the next morning.
[29] In this case all these items were sourced from the Paris newspapers.

the news by marking the moment when the information had been originally recorded or the events referred to had occurred. They varied, of course, because news took different times to travel from different places. At this particular moment, from London's point of view, events in Paris were unfolding five days, and in Milan four weeks, in the past.[30]

Time, rather than the bare number of miles, was the real metric of distance for information in the Napoleonic world. All kinds of communicative barriers—wartime blockades, mountain ranges, the condition of the roads, the existence or reliability of postal services, contrary winds at sea, and simple spatial separation—all of these reduced to measures of *how long* various kinds of information, or physical objects, or people, took to travel to other places. For us, the time information needs to move between locations is usually irrelevant, precisely because our technologies of transmission are so good that it can travel invisibly and virtually instantaneously. But in 1800 one of the most *essential* characteristics of news was that it had to travel, and that it took time—days, weeks, months—to do so. This gave news what now seems a rather paradoxical character: what was novel and fresh information in one place might be weeks-old and stale in another. The dated intelligence in the newspapers was a kind of atlas of this complex informational geography, telling readers how far they were in time, not space, from various places.

Every individual interested in goings-on in the wider world, from government ministers to curious country gentlemen, merchants to men of science, therefore participated in a never-

[30] Foreign news has traditionally been rather neglected in the large historiography on the newspaper press, which has overwhelmingly focused on studying the press in particular national contexts, especially in its relationship to political and state power—a framework in which domestic news reports were pertinent and foreign news less so. But now it is beginning to receive its due: see Will Slauter, "News and Diplomacy in the Age of the American Revolution" (PhD diss., Princeton University, 2007); Andrew Pettegree, *The Invention of News: How the World Came to Know About Itself* (New Haven: Yale University Press, 2014); Brendan Dooley, ed., *The Dissemination of News and the Emergence of Contemporaneity in Early Modern Europe* (Farnham: Ashgate, 2010); Tony Claydon, "Daily News and the Construction of Time in Late Stuart England, 1695–1714," *Journal of British Studies*, 52 (2013), 55-78.

ending process of gathering and interpreting a stream of pieces of information, of varying degrees of freshness, which had to be pieced together to form a working picture of distant occurrences and states of affairs. Pinning intelligence to dates and locations allowed readers to fit new information into an evolving mental timeline of the recent past, a temporal and spatial jigsaw puzzle of days, events, names, and places in which some pieces were missing and to which others were constantly being added. Then as now, many people relied on newspapers to do at least part of the work for them. Much more than their modern descendants, however, Napoleonic-era newspapers still left the pieces of the jigsaw visible on the page, and they still required considerable puzzle-solving skills on the part of their readers. Their issues are a series of in-action snapshots of the messy process of obtaining and ordering intelligence, captured at the moment the paper went to press.

For example, British readers in the spring of 1800 could assemble an often fragmentary narrative of the ongoing war between Austria and France from intelligence copied into the London papers from two main sources.[31] One was the Paris newspapers, which at this point in the wars were regularly carried across the English Channel in neutral ships. The other was the Hamburg newspapers, arriving by the mail boats.[32] Intelligence via these separate channels arrived at different times and came in issues that had been printed on different days.[33] Moreover, each Hamburg or Paris paper was itself in part composed of a mass of secondhand reports originating in other places on a range of dates. The Hamburg press, for example, contained

[31] This section is based on a reading of the *Morning Chronicle* and other London newspapers from May and June 1800.

[32] An additional source was the dispatches received by the government from British observers stationed with the Austrian armies in Bavaria, printed in the government's *London Gazette*.

[33] In this instance, news via Paris was usually much more up-to-date, prompting the *Morning Chronicle* to speak contemptuously of the Austrian generals' lack of access to information in the field. "Yesterday afternoon arrived the first of the Hamburg Mails due. Of course it brings no intelligence," the newspaper grumbled, before going on to mock the Austrian commander in Italy, General Melas, because his latest dispatches, printed originally in the Vienna Court Gazette, showed a "lamentable want of intelligence" (pun, perhaps, intended) that Napoleon's army was on his side of the Alps rather than in Switzerland. *Morning Chronicle*, 26 June 1800, p. 3.

intelligence from all over Central Europe, such as Austrian military dispatches originally printed in the government gazette at Vienna. They, of course, offered a rather different version of events than did the Paris newspapers.

The London papers faithfully reproduced these reports for their readers, in translation but otherwise raw and unprocessed (although a separate interpretive gloss was often provided).[34] Since most information from abroad was taken from foreign newspapers or gazettes (sometimes run by hostile governments), it was assumed that some of this news would be misleading, confused, heavily biased, or wrong.[35] Faced with messy, unclear, and often contradictory intelligence, readers had to apply their own calculus of trust, weighing the putative facts for themselves and keeping in mind that they could expect better-quality information to arrive later. All this was especially challenging when following ongoing military conflicts, in which events were fast-moving, unpredictable, and clouded by the fog of war, but similar hermeneutic processes of piecing together and assessing information were applied to intelligence of all kinds, including in science.[36] In short, readers in 1800 experienced news of the wider world not as a

[34] Although some translations must have been done in-house if the London newspaper happened to obtain its own copy of a foreign paper, there was also a centralized source of translated reports through the Post Office, which had a team of clerks making translations from foreign newspapers for the use of government ministers; for a fee these were made available to newspapers. See James Savage, *An Account of the London Daily Newspapers, and the Manner in which they Are Conducted* (London, [1811]). The extent to which editors attempted to gloss news reports and help their readers make sense of them varied. The major London daily newspapers usually contained a "leading article" which tried to synthesize and make sense out of the foreign reports. In newspapers on the Continent readers usually had to make do on their own.

[35] Newspapers were low-budget operations which in almost all cases simply did not have the resources to undertake the systematic collection of news outside their immediate localities. The professional "foreign correspondent" was an invention of the nineteenth century.

[36] And total meltdown of trustworthiness was not unknown even in scientific news. In 1804 a report of the death of the scientific traveler Alexander von Humboldt from yellow fever in Acapulco, Mexico, propagated through the European newspapers (e.g. *Hamburgischer Correspondent*, 12 June 1804; *Moniteur*, 21 June 1804) and was also subsequently announced in the June issue of the British scientific journal the *Philosophical Magazine*. But soon a letter from Humboldt's friend Abraham Mendelsohn printed in the *Publiciste* newspaper at Paris (21 June 1804) cast doubt on the reports of Humboldt's demise, claiming that Wilhelm von Humboldt in Rome had recently received letters from his brother sent from Havana. In an atmosphere of complete uncertainty over whether Humboldt was alive or dead, the Spanish *Gaceta de Madrid* (20 July 1804) voiced a wholehearted skepticism about the reliability of the original reports, printing a general appeal for information that might decide their truth or falsehood. But the curtain only came down on the confused drama after the appearance of Humboldt himself on the stage, when he

packaged portrait of that world's present state, but rather through a series of disconnected fragments of possibly dubious information about what had supposedly happened, some time ago, in various distant places.

By 1800, newspapers were the place where most educated urban Europeans got their information about distant events. But they formed part of a larger interconnected system of information involving other dealers in intelligence, among which the leading players were, of course, European states. Constituencies within the state apparatus particularly concerned with intelligence included diplomats, the "ministries of police" that ensured public order and kept a watchful eye on dissent, and (where the stakes were highest) the military and core executive power. The history of the Napoleonic *Grand Empire*'s intelligence system remains to be written, but it seems hard to doubt that the mastery of information on the move was central to the emerging modern state apparatus which historians have seen in its far-reaching reforms and its efficient, centralized, rational bureaucracy.[37] Other wartime governments from London to Moscow pursued similar goals in their own ways, seeking to obtain and master secret and public intelligence of all kinds: diplomatic negotiations, espionage, commercial information, troop

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arrived back in France on 1 August (it was only the third time his death had been announced in the European press). In fact, the reports can be traced to an article on galvanism in *Nicholson's Journal* by the London surgeon and galvanic therapist Charles Wilkinson, which contained a footnote referring to Humboldt's death (where Wilkinson got his mistaken information is unknown). This footnote had prompted a German savant called Philipp Nemnich to forward the news to the *Hamburgischer Correspondent* newspaper, which had spread the reports across Europe: *NJ* 7 (1804): 72, 303-304.

[37] Louis Bergeron, *France Under Napoleon*, trans. R. R. Palmer (Princeton University Press: Princeton, 1981); Alexander Grab, *Napoleon and the Transformation of Europe* (London: Palgrave, 2003); Michael Broers, *Europe Under Napoleon* (London: Arnold, 1996). Military historians, of course, have long been interested in Napoleon's uses of intelligence on campaign, but the uses of information systems for state-building and European Empire in this period—arguably, of much greater long-term impact—await study. For a possible model, see Jacob Soll, *The Information Master: Jean-Baptiste Colbert's Secret State Intelligence System* (Ann Arbor: University of Michigan Press, 2009).

movements, the contents of foreign newspapers, and the activities and opinions of their own populations.[38]

Although states enter into my story only intermittently, in a sense the rising power and reach of the state forms the deep background to the world of scientific information around 1800—and not simply because of direct state support for science.[39] While the wars waged by European governments in the Napoleonic period certainly created serious barriers to international scientific exchanges, at a deeper level the state was the great enabler of scientific transactions. States ran postal services or granted postal monopolies, controlled or subsidized many newspapers, managed ports and protected shipping, built some of the best roads and kept them safe for travelers.[40] Finally, as I show in Chapter 4, state functionaries—diplomats, civil servants, and even government ministers—proved invaluable allies for men of science in the transmission of scientific information across borders, especially at some of the most difficult periods of the wars.

[38] Espionage and secret diplomacy are perhaps what naturally come to mind when speaking of intelligence during the Napoleonic wars, but such high-stakes clandestine activities were embedded within a much larger system of information drawn on by governments, much of which was simply sourced from foreign newspapers. Here the problem was not secrecy or spycraft, but getting the information quickly, assessing its trustworthiness, and making sense of it in relation to other sources that might also include secret intelligence. For example, Napoleon had an office of thirty clerks employed to furnish translations from the latest English newspapers, analogous to the similar system the British government had for French and German papers at the Post Office: Barry O'Meara, *Napoleon in Exile* (London: Simpkin, 1822), 2:408. He also manipulated the news in the French press, including inserting fake foreign news to spread disinformation: e.g. Napoleon to Prince Eugène, 24 July 1805, in *Correspondance de Napoléon 1er*, vol. 11 (Paris: Imprimerie Impériale, 1863), 33, mentions fake news, purporting to originate at Berlin, deliberately inserted in the *Moniteur*.

[39] State patronage of science in any case varied widely. Whereas in Napoleonic France expert science became almost coincident with the state, and leading men of science received government salaries and were on occasion raised to high office, in Britain direct state patronage was virtually nonexistent outside astronomy. President of the Royal Society Sir Joseph Banks, the go-to informal advisor to the British government on matters of science, would—as an independently-wealthy gentleman—have been horrified by the merest hint of an offer of payment for his services.

[40] Perhaps the most complete expression of the central state as communications-enabler at this time was, in fact, the United States, where the postal service in 1816 employed 69% of the entire civilian workforce of the federal government and, as Richard John has argued, was for many people the primary manifestation of federal government in daily life. Richard John, *Spreading the News: The American Postal System from Franklin to Morse* (Cambridge, MA: Harvard University Press, 1995), 3.

Merchants and bankers formed another powerful constituency concerned with the movement of information around 1800. News being an inseparable partner of long-distance trade, the rise of the great early modern commercial cultures had fostered bustling hubs of information, from the Amsterdam exchange to the famous London coffeehouses. It was no accident that newspapers tended to be founded in these commercial centers, where information moved back and forth between the public domain and the private correspondence of merchants, who needed fast and reliable communications with each other both to exchange privileged intelligence about markets and politics, and to maintain the webs of credit and debt which structured international trade.[41] The merchants' news systems were highly developed by the end of the eighteenth century, when the Revolutionary and Napoleonic period ushered in a series of protracted upheavals in European and global commerce. War, blockades, sudden changes of government, confiscation of assets, and revolutionary economic policies caused the ruin of many established merchants and bankers—and buoyed the rise of others, most famously the Rothschilds. Another consequence was that London definitively supplanted Amsterdam as the financial center of Europe and the hub of commercial information. Amidst such uncertainty, maintaining good sources of commercial and political intelligence was of paramount importance, and the merchant communities of London, Paris, Amsterdam, Geneva, Frankfurt, Hamburg, and other European cities proved resourceful. In particular, the foreign transactions ventured by great London banking houses like Baring and Hope & Co. (who together financed the Louisiana Purchase in 1803) required reliable and swift communications with trusted agents in foreign

[41] Woodruff Smith, "The Function of Commercial Centers in the Modernization of European Capitalism: Amsterdam as an Information Exchange in the Seventeenth Century," *Journal of Economic History* 44 (1984), 985-1005. John McCusker, "The Business Press in England before 1775," in *Essays in the Economic History of the Atlantic World* (London: Routledge, 1997), 99-121. Alex Preda, "In the Enchanted Grove: Financial Conversations and the Marketplace in England and France in the 18th Century," *Journal of Historical Sociology* 14 (2001), 276-307. For an argument that links Dutch commercial information to key developments in seventeenth-century science see Harold Cook, *Matters of Exchange: Commerce, Medicine, and Science in the Dutch Golden Age* (New Haven: Yale University Press, 2007).

cities.[42] Systems of private couriers (and smuggling) meant that information on military and political events could sometimes reach merchants before it was in the hands of government ministers.[43] Even at the height of the Napoleonic blockade that cut Britain off from the Continent, it was still possible for a gentleman in London—President of the Royal Society Joseph Banks, for example—to remit money to Paris using these channels.[44] Nonetheless, even the best private intelligence systems had their limits, and on many occasions both merchants and the politicians and diplomats to whom they were tied by interdependence (and sometimes friendship) had to rely on foreign newspapers for their information on events abroad.

States, merchants and the newspapers formed the three major interlocking and interdependent constituencies in the European news system, into which were also woven a myriad of other entities, media, and channels of communication, including scientific societies, journals, and personal exchanges of letters. All were joined in a mass of interconnections, a situation evident in the daily life of one key scientific intelligence broker living in London during the Napoleonic period, Sir Charles Blagden. Blagden read the newspapers assiduously every day, and carefully cultivated friendships with important politicians, civil servants, army and naval officers, foreign diplomats, and bankers and City merchants (including the great financier Henry Hope). From all of them, he sought fresh information about political or military developments on the Continent to furnish his mental picture of distant events.[45] He was also the most important letter-writer transmitting scientific information between Britain and France during the wars—

[42] Marten Bust, *At Spes Non Fracta: Hope & Co. 1770-1815, Merchant Bankers and Diplomats at Work* (The Hague: Nijhoff, 1974); Niall Ferguson, *The House of Rothschild: Money's Prophets, 1798-1848* (New York: Penguin, 1999); Herbert Kaplan, *Nathan Mayer Rothschild and the Creation of a Dynasty: The Critical Years 1806-1816* (Stanford: Stanford University Press, 2006).
[43] In 1815 Nathan Rothschild famously had the news of the allied victory at Waterloo in London before the British government, an incident that was the source of much subsequent mythmaking about huge sums supposedly made on the information.
[44] Jean-Baptiste-Joseph Delambre to Banks, 22 November 1810, BL Add. MS 8100, fols. 65-66.
[45] For Blagden's connections and intelligence gathering see Chapter 4.

harnessing his links with government, diplomats, and bankers to get letters back and forth. For Blagden and others like him, the superintendence of scientific communication was enmeshed within and inseparable from the wider culture of information and news in which he lived.

Reprints and the Periodical Commons

Returning to our time-stamped items of foreign news in the *Morning Chronicle*: we saw how these were very often reproductions of reports that had previously appeared in other newspapers, usually from Paris or Hamburg (which, in turn, might have lifted the reports from elsewhere).[46] This is one example of a very widely-used technique that was fundamental to how news and other kinds of information travelled around 1800. The process was simple: lift some text from one publication and reprint it, often word for word, in another. But reprinting had far-reaching consequences, because the practice multiplied readerships, spreading novel and interesting material among different audiences in new locations. Important news carried to London in a single copy of a Paris newspaper, for example, would be reprinted in thousands of copies of British papers, rapidly and efficiently spreading it across the reading nation. Though perhaps most conspicuous in newspapers, reprinting was in fact employed very widely in periodicals of all kinds, including monthly scientific journals, which would regularly lift even long technical reports of experiments from other publications.[47] The ubiquity of the practice, until recently largely passed over by historians, points towards a very different set of norms of originality and periodical authorship that would only start to retreat in the second half of the nineteenth century

[46] Newspapers did make use of other sources for foreign intelligence, primarily information released by governments (such as military or diplomatic dispatches), and also sometimes the contents of letters from abroad or reports of recently-arrived travelers.

[47] See Iain Watts, "'We Want No Authors': William Nicholson and the Contested Role of the Scientific Journal in Britain, 1797-1813," *British Journal for the History of Science* 47 (2014): 397-419.

with the rise of professional newspaper reporting, journalists' bylines, and modern copyright law. [48]

We can witness the process in action by following the *Morning Chronicle*'s first report of Volta's battery. This text was actually a report of a lecture, given at the Royal Institution in London by the physician and man of science Thomas Garnett on 28 May. Printed in the *Chronicle* two days later, it was quickly picked up and reprinted by several London newspapers: the same day in the evening paper the *Star*, and again over the next few days by numerous others.[49] The texts of all these reprints matched word-for-word the text of the original *Morning Chronicle* report.[50]

The transmission of the report beyond Britain was set in train soon afterwards. In the first days of June an American ship called the *Factor* sailed from London down the Thames and out to sea, bound for home. On board were issues of some of the latest London newspapers, among them the *Morning Chronicle* of 30 May.[51] After a voyage of six weeks, both ship and newspapers arrived in New York, where the news of the battery was reprinted by that city's *Commercial Advertiser* on 19 July, and in other papers soon after.[52] From the New York papers,

[48] Historians used to working with eighteenth- and nineteenth-century newspapers and other periodical sources have certainly long been aware of the practice of reprinting, but it is only now that scholars are beginning to look in detail at how it actually did spread news and other kinds of information in practice: see Will Slauter, "The Paragraph as Information Technology: How News Traveled in the Eighteenth-Century Atlantic World," *Annales: Histoire, sciences sociales* 67 (2012), 253-78; also Bob Nicholson, "'You Kick the Bucket; We Do the Rest!': Jokes and the Culture of Reprinting in the Transatlantic Press," *Journal of Victorian Culture* 17 (2012), 273-86.

[49] *Star*, 30 May 1800. Subsequently in for example, *Sun*, 31 May 1800; *General Evening Post*, 31 May 1800; *Whitehall Evening Post*, 31 May 1800; later in *Evening Mail* 2-4 June 1800, and *St James's Chronicle*, 17 June 1800.

[50] Although a few newspapers omitted the last paragraph of the article, which did not contain any novel experiments but consisted only of a short comment on the possibility of building more powerful batteries.

[51] Provided they were of a later date than what had already been received in America, fresh London papers would have fetched a good price if sold in New York—most likely to one of the city's own newspapers (which would want to reprint from them). They were therefore a natural item to take on a voyage.

[52] *Commercial Advertiser* (New York), 19 July 1800, p. 3; *American Citizen* (New York), 25 July 1800, p. 3. The *Factor* (under a Captain John Kemp) arrived on 16 July and items of news from the papers it carried were reprinted starting the next day, but the news of the battery had to wait two more days before it was copied into the New York press, because it was naturally considered of less immediate interest than Napoleon's invasion of Italy. (By this time, of course, Italy had already been under French control for a month; Americans were experiencing European

the information was reprinted in Philadelphia on 26 July, and subsequently up and down the coast in newspapers from New Hampshire to South Carolina, by editors who had never even seen a copy of the original *Morning Chronicle*.[53] This was the earliest intelligence of Volta's pile to reach the United States, and the first batteries in America were built using these newspaper reports.[54]

Communication of the *Morning Chronicle*'s text to America happened about as rapidly as possible—it arrived along with the most up-to-date news, of any kind, on events in Europe. Its journey to France and other Continental locations was more uneven; the story reveals the element of chance and uncontrollability inherent in the movement of information, especially across wartime frontiers. Despite the ongoing conflict, London newspapers were being carried to Paris regularly and quickly—but it appears no copy of the 30 May *Morning Chronicle* ever reached the French capital.[55] In the end, the news made the trip in a copy of the *Courier de Londres*, a French émigré paper printed in London, which had reprinted it (translated into French) on 8 August, over two months after the original news.[56] This issue took about a week to

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events as they had happened six or more weeks in the past.) Details of the voyage from *Lloyds Evening Post* (London), 2-4 June 1800 (postscript), and *New York Gazette*, 17 July 1800, p.3.

[53] For example: *Philadelphia Gazette*, 26 July 1800, p.3; *Sun* (Dover, NH), 6 August 1800, p.2; *City Gazette* (Charleston, SC), 13 August 1800, p. 3.

[54] One early reproduction, reported in the press, was by the physician and public scientific lecturer Dr. John Vaughan of Wilmington, Delaware, who built a battery and repeated the decomposition of water before 3 August: see *Prisoner of Hope* (New York), 9 August 1800, p. 103, reprinting *Mirror of the Times* (Wilmington, DE), 3 August 1800. Vaughan had previously written on animal electricity, and his *Valedictory Lecture Delivered Before the Philosophical Society of Delaware* (Wilmington, DE: Franklin Press, 1800), 14-15, describes the battery in terms which are clearly drawn from the text of the newspaper reports. Vaughan was probably not the first person to build a voltaic pile in the United States, which was presumably done in New York or perhaps Philadelphia before the end of July.

[55] The Paris *Moniteur* does not appear to contain any news from the 30 May *Morning Chronicle*. Of course, it is impossible to discount the rather unlikely possibility that a copy did reach Paris but was not reprinted or seen by anyone with scientific interests.

[56] *Courier de Londres*, 8 August 1800. It is hard to give a satisfactory reason for this delay, or an explanation of why the editor of the *Courier* was combing for material in such (comparatively) old newspapers. It may be that the *Courier* was reprinting the text from some unknown intermediate British publication.

cross the Channel to Paris.[57] From that paper the report of the battery was reprinted on 17 August in the *Moniteur*, the official newspaper of Napoleonic France (and a publication with important connections to state-sponsored French science).[58] Batteries were quickly built in Paris using the newspapers' instructions, but, postponing that story for now, let us briefly trace a little of the report's further progress on the Continent. From the *Moniteur* it was copied into other French-language newspapers, including some outside France, such as the Swiss *Bulletin Helvétique* and the *Journal de Bruxelles.*[59] The second of these turned out to play an unexpectedly-important role, because it was the medium that conveyed the report to Jena in Saxe-Weimar, and so, we shall see, to the young experimenter Johann Wilhelm Ritter. Finally, copies of the *Moniteur* were now being sent to Italy, which had recently returned to French control after the decisive Battle of Marengo that followed Napoleon's crossing of the Alps. It was one of these that reached Alessandro Volta in Como on 31 August, a full five months after he had sent the letter to the Royal Society that communicated his discovery to London. It was the first news for him that his battery had been reproduced in Britain, and—a surprise—that it could be used to decompose water, an experiment he had not thought to try.[60]

Reprinting was not confined to the newspapers, but a ubiquitous feature of periodical print of all kinds. It played a defining role in the communication of galvanism and other kinds of science, and we will see it at work over and over again. Although historians have long been aware of the practice, the extent to which it was carried on has really only become evident with

[57] Simon Burrows, *French Exile Journalism and European Politics, 1792-1814* (London: Royal Historical Society, 2000). The *Courier* was probably taken across the channel on one of the neutral ships that still plied the Dover-Calais route at this time.
[58] *Moniteur*, 29 Thermidor an 8 (17 August 1800). On this publication see Chapter 2.
[59] *Bulletin Helvétique* (Lausanne), 24 August 1800, pp. 400-401; *Journal de Bruxelles*, 3 Fructidor an 8 (21 August 1800), pp. 503-504.
[60] Volta to Marsilio Landriani, 22 September 1800, in *Le Opere di Alessandro Volta*, vol. 2 (Milan: Hoepli, 1923), 5-11. Volta in fact received the same information about the London chemists' decomposition of water only three days after he read it in the *Moniteur* by a completely independent route through Austrian-controlled territory, in a letter from Landriani, a contact at Vienna.

the rise of mass digitization and full-text search—in a sense, we are just beginning to understand the unfamiliar media culture reprinting represents.[61] This is in part because scholarship on the history of print has largely been about books.[62] Books were indeed the most prized and prestigious print objects in 1800, although in retrospect their star had perhaps already begun to wane for the communication of expert knowledge in the natural sciences. Because of the focus on books, the copying and reprinting of texts has overwhelmingly been discussed in terms of literary property, copyright, and piracy—it was through books that these concepts were fought over and, indeed, came into being.[63] But we can look at the world of print around 1800 differently. Most printed objects produced at this time were not books, but issues of newspapers, periodicals, and ephemera of various kinds.[64] The text in these publications was not usually something from which a long-term profit could be extracted, and thus was not *worth* making into property.[65] The fundamental point about the ubiquity of reprinting is not that piracy and breach of copyright were tolerated in periodical print but that the concepts did not even really apply.[66]

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[61] See Will Slauter, "Upright Piracy: Understanding the Lack of Copyright for Journalism in Eighteenth-Century Britain," *Book History* 16 (2013): 34-61.

[62] For this period see especially William St Clair's magisterial *The Reading Nation in the Romantic Period* (Cambridge, Cambridge University Press, 2004), which (despite its title!) overwhelmingly focuses on books.

[63] Adrian Johns, *Piracy: The Intellectual Property Wars from Gutenberg to Gates* (Chicago: University of Chicago Press, 2010); also Johns, *Nature of the Book*.

[64] The surviving print objects from this period thus represent a population sample *massively* skewed towards books, because books made around 1800 were usually long-term investments that tended to be preserved or resold, whereas most copies of newspapers, for example, perished in the days or weeks following their creation.

[65] In the end, perhaps it all came down to economics. Reprinting of this kind was not piracy because nobody lost out, and therefore rarely if ever did anyone feel aggrieved enough to start making *accusations* of piracy. The reason is that periodicals are not sold like books. For example, it did not matter if another newspaper reprinted some important news from the 30 May *Morning Chronicle* on 31 May, because by that point the 30 May *Morning Chronicle* was no longer making any money for the proprietors; indeed it no longer really existed as a product. What they were selling was now the next day's 31 May *Morning Chronicle*—which would itself be augmented by reprints from other London papers. Rights of property were never asserted in periodical print because the rent from such property would be minimal. (Naturally the economics of periodicals did not *determine* this situation; legal and cultural factors were also crucial.)

[66] Of course no international system of copyright existed at this time, but, as we saw, the practice of reprinting was regularly carried on within the same country and even in the same city. In the case of eighteenth-century Britain, Will Slauter has shown that complaints that reprinting represented the violation of some form of property did arise in a few cases earlier in the eighteenth century, particularly when the practices of magazine and periodical journalism were actually in the process of formation with the advent of influential new titles like John Cave's

Drawing a parallel with land ownership, we might imagine book publishing as a chain of stockaded forts, where the literary property inside is quite well-defended. But outside the walls of these fortresses is an expanse of unenclosed fields, in which text is recycled, copied, re-copied, translated, shared, excerpted and anthologized, and in which news, intelligence and flows of information predominate. This is the periodical commons.

Reprinting in newspapers, scientific journals, and other periodicals was a bridge across distance in scientific communication. It widened access to information by allowing it to be distributed to new groups of readers in new localities. Not only distributed, but sold; for editors it was also good business. But text was copied not just because doing so was easy and profitable, but because readers *expected* access to the substance of the original report. Although editors might do a certain amount of reprocessing—especially on longer texts from periodicals—too much interference with core material was dangerous because the circumstances of the text's production far away were unknown. Nonetheless, even if reproduction was at times word-for-word and could look almost mechanical, human agency necessarily entered at every step to create what readers saw on the page—the movement of intelligence was an *emergent* phenomenon resulting from the actions of many widely-dispersed and now-invisible personages: postal carriers, ship captains, press-men, compositors, translators, and especially editors. Various individuals carried, edited, translated, modified, glossed, and reprinted texts along the routes between the original writer and readers, so that even if an article sourced from another publication did bear the name of an original author (as it usually did in scientific journals), it was evidently the composite production of many hands. People were not generally bothered by this;

Gentleman's Magazine: Slauter, "Upright Piracy," 40-53. But such accusations (which never received any legal sanction) seem to have been virtually nonexistent by the late eighteenth century, by which time reprinting had long been a mainstay of all sorts of periodical media. In Britain, legal copyright in the contents of periodicals was only created in 1842, well into the new era of steam printing and mass readerships; explicit protection for the text in newspapers was not legislated until 1911.

for them it was simply the natural way information travelled in periodical print. Thus scientific intelligence moved in a decentralized, multi-branched fashion, over which no one was able to exercise much control. But the combined effect was to achieve things like moving and reproducing the *Chronicle*'s text describing how to build a battery across great distances, national borders, and the frontiers of war. We have still to learn, however, how it got into the newspaper in the first place.

Accidental Publicity: The Battery Comes to London

On 20 March 1800, from the shores of Lake Como in the foothills of the Alps, Alessandro Volta wrote a letter to Sir Joseph Banks, President of the Royal Society of London.[67] The year before, Austrian and Russian troops had taken control of northern Italy, the French-backed Cisalpine Republic had ceased to exist, and Volta's university, Pavia, had been closed.[68] But tranquil retreat in his home town had been productive, and it was time to communicate the results.[69] Wanting to send a letter across international borders in 1800, Volta had two broad options, and he used both to get information about the battery to London.[70] First, he sent a four-page letter laying claim to the discovery via the mail, usually a fast means of conveyance but possibly

[67] My discussion of the initial reception of the battery builds on previous work by William Sudduth and Giuliano Pancaldi: see William Sudduth, "The Electrical Decomposition of Water: a Case Study in Electrical and Chemical Science, 1746-1800" (PhD diss., University of Oklahoma, 1976), 198-300; William Sudduth, "The Voltaic Pile and Electro-chemical Theory in 1800," *Ambix* 27 (1980): 26-35; Pancaldi, *Volta*, 211-56.

[68] As Giuliano Pancaldi has shown, Volta's decision to announce the battery via Britain—Austria's ally—was a product of Lombardy's return to Austrian control combined with Volta's prior history of productive interactions with the Royal Society. After Napoleon brought Italy back under French rule in mid-1800, however, Volta, ever the adept political pragmatist, quickly and very successfully reoriented himself towards Paris. Pancaldi, *Volta*, 212.

[69] Volta had first constructed a battery around the end of 1799. The definitive study of Volta's train of investigations that produced the battery is Pancaldi, *Volta*, 178-210.

[70] Volta to Banks, 20 March 1800, and Volta to Banks, 1 April 1800, Royal Society Archives, Letters and Papers (L&P), "Decade XI" (vol. 109), no. 137. The two letters (both in French) were merged into a single paper when printed in the *Philosophical Transactions*. Volta was drawing on a long tradition of scholarly communication, but his interaction with Banks lacked the back-and-forth symmetry of the idealized "commerce des lettres" of the Republic of Letters. Volta's discovery first crossed Europe in manuscript, but news of its reception in London returned to him in Como not in a reply from Banks, but via a mixed cascade of print and other letters via intermediaries.

unreliable, especially during wartime. An alternative, more certain but difficult to arrange and often slower, was to have correspondence carried by a trusted party who happened to be going to the required destination, and this was how a second more detailed account for the Royal Society travelled to England, in the hands of a merchant-apothecary from Volta's town.[71]

In 1800 the central site for scientific intelligence in Britain, and the center of power in British science, was not the Royal Society of London's official rooms in Somerset House in the Strand. It was in a private home: specifically, in Sir Joseph Banks's library, at his house in Soho Square. Banks published virtually nothing all his life, but he understood the link between information and power. In the 1780s he had seen off threats from a rival "mathematical" faction in the Royal Society, who had tried and failed to stop Banks concentrating the Society on himself and his close associates. Banks wanted to ensure that the victory would be lasting, and he had a brilliant strategy: housing the social life of London science under his own roof. He offered men of science a comfortable place to gather and exchange news; in return he would control the flows of scientific information in Britain, without even needing to leave his home.[72]

Presided over by Banks's in-house librarian, and hovering ambiguously between informal members' club and private residence, the library was in theory accessible daily to any respectable man of scientific interests.[73] In fact, of course, some people were more welcome than others, and everyone who entered did so as Banks's guest. The library's attractions were

[71] Volta's letters to Banks name the man as "Garovallio, apothecary of this town of Como"; Pancaldi, *Volta*, 213, identifies him as Pasquale Garovaglio, a spice and drug merchant.
[72] On Banks's dominance of London intellectual life see David Miller, "Sir Joseph Banks: An Historiographical Perspective," *History of Science* 19 (1981): 284-92; John Gascoigne, *Joseph Banks and the English Enlightenment: Useful Knowledge and Polite Culture* (Cambridge: Cambridge University Press, 2003).
[73] The usual means of gaining admittance was an introduction by some established figure. Remembering his initiation as a young man in 1780 in the company of the East India Company navigator Captain Thomas Forrest, the orientalist scholar William Marsden noted how, after a breakfast with Banks and other Royal Society figures like William Herschel and Charles Blagden led to a general invitation to return at any time, "the rooms in Soho Square were my habitual place of resort, where I met a variety of persons, and acquired information of what was going forward in the world of literature and science." William Marsden, *A Brief Memoir of the Life and Writings of the Late William Marsden* (London: Cox, 1838), 46.

certainly congenial: an unrivalled collection of scientific books, the latest British and foreign periodicals and newspapers, tea and coffee, armchairs, and always the chance of hearing something new and interesting—this was a place for conversation as well as quiet reading.[74] It was where "new discoveries of every kind were communicated and discussed," and "plans suggested and arranged for the general diffusion of scientific information."[75] Men of science were to be found congregating there almost every day, and especially during Banks's weekly gathering on Sunday evenings, termed the "*conversazione*." The library's paramount importance as a site for British science during these years has only gone unrecognized because the informal exchanges of information that took place there left few records behind.[76]

While Volta's contemporaries had a wide range of opinions on how the battery might work or how it might be applied, they were united in terming it a "discovery"—a word which, perhaps more than any other, staked out the natural sciences' claim to be uniquely oriented towards progress in knowledge and material improvement. The word was used not just for spectacular advances like the battery, but also applied to more humble "facts," such as a new chemical procedure or a previously-undescribed species of beetle. Scientific progress was equated with the accumulation of discoveries: because discoveries were the revelation of new facts of Nature, they belonged to everyone, and were true forever. "Systems," on the other hand, were invented by people, and so considered potentially-misleading, even if they might be

[74] See for example the account in Barthélemy Faujas de Saint-Fond, *Voyage En Angleterre, En Écosse Et Aux Îles Hébrides* (Paris: Jansen, 1797), 1:6-7. Also *Monthly Magazine* 37 (1814): 51-52: "here are to be found Literary and Philosophical Journals from every part of Europe, and from hence are generally disseminated, the various discoveries and improvements which are constantly making in every branch of science."
[75] "Memoir of Sir Joseph Banks," *Investigator* 3 (1821): 374.
[76] The best window onto these meetings is Sir Charles Blagden's manuscript diary (see Chapter 4), which is filled with almost daily notes of goings on in Banks's Library. The "*conversazione*" gathering took place weekly during the London "season," when the Royal Society was in session; Banks also held a "public breakfast" at his house on Thursday mornings.

necessary for ordering knowledge.[77] "Facts are Truths unchangeable," Humphry Davy put it. "Theories are only the human forms by which truths are connected."[78]

This rough-and-ready Baconian philosophy of science (especially strong in the experimental sciences, and particularly in Britain) was closely tied to the social processes by which new discoveries were announced and rewarded. As the revelation of natural facts, discovery was the common property of the scientific world, but *credit* for a discovery would belong to an individual (and, assuming the discovery was judged valid, would belong to him until the end of time). The idea of discovery as Nature's way of dispensing new knowledge in discrete pieces thus provided a naturalistic way of parceling out credit in science. "The introduction of noble discoveries" proclaimed Davy (channeling Bacon) "seems to hold by far the most excellent place among all human actions." And discoveries in "experimental philosophy," he added, "cannot but produce benefits and can by no means be turned to evil or unworthy purposes."[79]

But little agreement existed any longer as to the procedures by which discovery ought to be presented, disseminated, and judged. In the great heyday of scientific societies and academies during the eighteenth century, there had been a standard procedure (still widely in use in 1800) for making information about new science known to the world. The originator would submit a written document (variously called a "memoir" or a "paper") to a scientific society or academy (such as the Royal Society or the Academies of Paris, Berlin or St. Petersburg). There, it would be officially read out at a meeting, and later—sometimes years later—it *might* be printed and

[77] Thus, while British chemists in this period generally adhered to the new "French System of Chemistry" of Lavoisier, they frequently entertained the possibility that it would eventually be overturned.
[78] RI Davy MS HD/3/A/1 (Royal Institution lecture from 1809).
[79] RI Davy MS HD/2/D/1 (Lecture from 1808). The source for Davy's quotation is Bacon's *New Organon*, Book 1, Aphorism CXXIX.

distributed (not always very widely) in a collection of "memoirs."[80] Some academies (but not the Royal Society) would also deliver an official judgment on the work. Crucially, the moment when the discovery was registered and became in a sense the common property of the scientific world was *not* when it was printed, but when it was read at the meeting and recorded in the academy's minutes.

The Royal Society in 1800 maintained a variant of this procedure. Papers were given or sent to Joseph Banks or to the Society's secretaries, they were subsequently read out loud at its meetings, and then—if judged fit by the Society's Council—they were printed in the *Philosophical Transactions*.[81] The *Transactions* appeared (usually) twice a year, and it was not a scientific journal but representative of a quite different class of publication: what were called the "memoirs" or "transactions" of the academies and societies.[82] In a strict sense, these were not *periodicals* at all, but only *serials*: they were not issued on specific and predicable days at clearly-defined intervals, but irregularly and slowly. In contrast, for readers in 1800 a journal was a periodical which came out regularly (usually monthly) and was packed with fresh information, such as letters sent to the editor, new articles, and reprints of material from elsewhere. In sum, journals were rapid and contained intelligence; official society publications were slow and did not. What the *Philosophical Transactions* and similar publications did offer was prestige, polish, and the stamp of official approval by an elite body—things which journals at this time rather lacked. Sometimes these things would be preferable to speedy printing in a

[80] See James McClellan, *Science Reorganized: Scientific Societies in the Eighteenth Century* (New York: Columbia University Press, 1985); James McClellan, "The Scientific Press in Transition: Rozier's Journal and the Scientific Societies in the 1770s," *Annals of Science* 36 (1979): 425-49.

[81] It seems likely that this would have made for very tedious listening on some occasions ("dull enough." was Sir Charles Blagden's laconic verdict on one Royal Society meeting: *Blagden Diary*, 26 March 1801). Yet even the most technical papers were read out loud, underscoring the importance of this ritual as an act of public announcement of new information received by the Society.

[82] The *Transactions* had been converted from a quasi-independent periodical of the Republic of Letters into an official "memoirs"-type publication in 1752. Around 1800 it usually came out twice a year, although the precise time of publication varied and in fact in the year 1800 itself the *Transactions* was issued in three parts.

journal. The difference between the ways these two kinds of publications were embedded in the flow of time strongly shaped how they were used in the day-to-day practice of science.

With this in mind we can understand what happened when Volta's first letter arrived at Banks's house in London. Banks was in the occasional habit of circulating new papers sent to the Royal Society among his circle before they were read at one of the Society's meetings, and towards the end of April he lent the manuscript to a friend, the surgeon Anthony Carlisle.[83] Carlisle in turn took the letter to his friend William Nicholson, a natural philosopher, chemist and scientific writer, known as an adept electrical experimenter—and also the editor of *Nicholson's Journal*, one of the two principal London scientific journals.[84]

Nicholson and Carlisle began their attempt to repeat Volta's work on 30 April, quickly building a pile out of half-crown silver coins and zinc discs.[85] Early in the experiments, Carlisle noticed some bubbles of gas in the water used to make the electrical contact at the top plate. At Nicholson's suggestion they ran the brass wires from the battery into a tube of water—and found hydrogen released at one wire, while the other tarnished and was oxidized.[86] This they interpreted as the decomposition of water by galvanism. Over the next few days Nicholson improved the experiment, achieving the clean production and collection of both hydrogen and oxygen using two platinum wires. This decomposition of water with the pile was soon hailed as

[83] Charles Blagden's Diary records several examples of informal canvassing of this sort on Banks's part (for which the term "peer review" would be a misleading anachronism). It is unclear precisely when Banks received the letter; William Nicholson stated that Banks gave it to Carlisle "at the latter end" of April: *NJ* 4 (July 1800): 179. Banks's choice of the medical man Carlisle underscores how much galvanism was still associated with the investigation of living organisms in 1800; additionally, Volta himself had envisioned the battery as an "artificial electric organ"—a model of an electric fish made out of inorganic materials—and named it thus in his first letter.
[84] The other was the *Philosophical Magazine*, founded in 1798. On Nicholson and his publication see Watts, "'We Want No Authors.'"
[85] *NJ* 4 (July 1800): 179-187.
[86] This happened on 2 May.

important new discovery in its own right, opening up the vast possibilities of the battery. Years later, Davy would call it "the true origin of all that has been done in electro-chemical science."[87]

Nicholson was now in a delicate position. He and Carlisle had made a discovery of their own, and they needed to stake a public claim as soon as possible.[88] His own journal, he thought, would be the perfect venue. It had been launched only three years before, and would benefit from the prestige. But there was a problem: Banks was still waiting for the arrival of the second of Volta's two letters.[89] Consequently, Volta's discovery of the battery had still not even been announced at the Royal Society, and if Nicholson preempted this event he would be giving the Society a grievous insult.[90] His journal already challenged the Banks circle's control of scientific intelligence, and here he had to tread especially carefully, because he and Carlisle would never have made the discovery at all if Banks had not given Carlisle private access to Volta's letter. And for that reason keeping the decomposition of water a temporary secret was also not an option—Carlisle had a duty to inform Banks of the results of their investigations. Sensitivities about when information could or could not be made public held this deadlock together, and an ambiguous situation arose in which news about Volta's pile circulated informally among London

[87] Humphry Davy, "On the relations of electrical and chemical changes," (Bakerian Lecture for 1826), *Philosophical Transactions* 116 (1826), 383-422, quote on 386.
[88] Not least because they had themselves made the discovery so quickly and easily after building their first pile.
[89] The second letter had been announced in the first letter. Volta apparently did not expect that Banks would wait for his second letter before formally announcing the contents of the first; as his second letter shows, he thought his first letter would have been already communicated to the Royal Society by the time the second arrived.
[90] As Nicholson put it in the issue of his journal for 1 July: "It appeared proper to avoid the publication of facts, originally flowing from the liberal communication of the worthy president of the Royal Society, until the paper of the inventor [Volta] had been read to that learned body; and this could not be done till very lately [on 26 June], because the latter part of his memoir did not arrive till long after the first four pages." *NJ* 4 (July 1800): 179. It is not clear exactly when the second part of Volta's account of the battery (transmitted with Garovaglio) did arrive in Britain. Pancaldi, *Volta*, 213 gives an estimate of "apparently before 3 June," but this seems doubtful: it is derived by assuming that when Nicholson publically laid claim to the discovery of the decomposition of water he must have been sure that the decomposition was not reported in Volta's second letter. But in fact *not* knowing whether a prior claim to the discovery of the decomposition of water would later be received from Volta would surely have made Nicholson *more* likely to stake a public claim after the *Chronicle* broke the news (see below), since he would at least garner some credit for independently noticing it even if Volta later turned out to have observed it previously. Moreover, Royal Society meetings were held on 12 and 19 June, but the discovery was only announced at the meeting of 26 June. Allowing some time for the second letter to be translated into English before it was read, it seems likely that it reached Banks around mid-June.

men of science without any public announcement. The center of these conversations was Joseph Banks's library, where someone (perhaps Carlisle) gave a brief account of the construction of the battery and the decomposition of water during the Sunday evening *conversazione* on 11 May.[91] Within a short time others in London were building batteries for themselves.

Enter Thomas Garnett, a doctor and man of science who was then in the midst of giving the first ever course of public lectures at London's Royal Institution, which had opened its doors to enormous interest only two months before. The Institution served a dual function: the pursuit of "useful knowledge" for social improvement, and public instruction and "amusement" through lectures.[92] It represented a new direction for science in Britain—not because it launched the enterprise of "public science," but because it *institutionalized* it, at the heart of fashionable London Society, in a fine permanent residence in the middle of Mayfair, with Royal patronage and with lectures with great aristocratic figures in the audience (which contained, it was often remarked, many women).[93] Previously it had been rather unclear who constituted "the public" of science (beyond those who practiced it). The Royal Institution created a new kind of dedicated space where a well-heeled lay public audience for science in Britain could be housed, edified, and entertained.[94]

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[91] *Blagden Diary*, Sunday 11 May 1800: "Went to Sir Jos Banks's: expt. of Volta repeated: bubbles of air from it." The London *Medical and Physical Journal* 4 (1 August 1800): 115 also stated that an account of Volta's experiments had earlier been given "at one of Sir Joseph Banks's meetings" (i.e. the *conversazione*).

[92] "Our object ought undoubtedly to be both amusement and instruction," Garnett had written to the RI Managers, noting that part of the audience "will come chiefly for amusement or because it may be fashionable." Garnett to RI Managers, 23 December 1799, reproduced in *Henry Bence Jones, The Royal Institution: Its founder and its first professors* (London: Longman, 1871), 167.

[93] On the early RI see Morris Berman, *Social Change and Scientific Organization: The Royal Institution, 1799-1844* (Ithaca: Cornell University Press, 1978), and David Knight, "Establishing the Royal Institution: Rumford, Banks, Davy," in "*The Common Purposes of Life": Science and Society at the Royal Institution of Great Britain*, ed. Frank James (Aldershot: Ashgate, 2002), 97-117; compare the earlier kind of "public science" described in Larry Stewart, *The Rise of Public Science: Rhetoric, Technology, and Natural Philosophy in Newtonian Britain, 1660-1750* (Cambridge: Cambridge University Press, 1992).

[94] Following Harold Mah, I take "the public" to mean a historically-contingent *idea* of a certain kind of group as audience, patron, and legitimating force, rather a specifically-identifiable set of human beings in the past: Harold

Garnett's course was on chemistry, and he was due to deliver a lecture discussing the chemical composition of water. Like other men of science in London he had heard about the experiments made with Volta's battery, and, keen to show his audience some novel and interesting demonstrations, he was able to borrow one.[95] In the evening of Wednesday 28 May he gave the lecture later reported on by the *Morning Chronicle*, which included a demonstration of the decomposition of water with the pile.

Newspaper readers in Britain, France, Germany, and the United States reaped the benefits; for Garnett the lecture brought only trouble, and he probably later wished he had never given it. In fact, however, his difficulties began not because of the newspaper report, which only appeared two days afterwards, but for another reason. The day after the lecture, word reached Sir Joseph Banks of a dreadful *faux pas*: Garnett had publicly ascribed Volta's work "to the French Philosophers."[96] Banks wrote to Count Rumford, the leading personality behind the Institution, demanding to know if this was true, and "from whom the Doctor procured his information." Rumford was unable to determine how Garnett had first heard the news, but confirmed the mention of the French. Rumford and Banks then concocted a humiliating statement of apology for Garnett to read at his next lecture, "to atone for his mistake and to make amends to Professor Volta."[97] The day after Banks learned of this scandal, the *Morning Chronicle*'s report appeared,

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Mah, "Phantasies of the Public Sphere: Rethinking the Habermas of Historians," *Journal of Modern History* 72 (2001): 153-82.

[95] Garnett borrowed his battery from the chemist and Fellow of the Royal Society Edward Howard.

[96] Probably Banks's informant was Blagden, who attended Garnett's lecture (a "bungling" performance, he noted in his diary), and then saw Banks the next morning: *Blagden Diary*, 28-29 May 1800.

[97] An odd choice of phrase, as Volta was 700 miles away and so heard neither lecture nor apology. Rumford to Banks, 29 May 1800; Rumford to Banks, 30 May 1800, reproduced in Bence Jones, *Royal Institution*, 156-57 (Banks's own letter does not survive). The apology read as follows: "Having by mistake on Wednesday, in the course of my public lecture, ascribed to the French philosophers a new and interesting discovery relative to galvanism, which, on inquiry, I find belongs to Professor Volta, of Milan, I feel it to be my duty to state this fact in a public manner, in order that Professor Volta may not be deprived in any degree of the honor of a discovery which so justly belongs to him and to him alone. The first news of this discovery, which arrived in this country in the beginning of last month, was communicated in a letter from Professor Volta to the President of the Royal Society, and it was in consequence of the information contained in that letter that Dr. Carlisle, Mr. Nicholson, Mr. Howard,

making a mockery of Royal Society protocol by dramatically publicizing the discovery before it had even been formally announced at a meeting. Banks's reaction to this is not recorded. He was probably furious.

Garnett's lecture must not have been so very ill-informed, however, as the *Morning Chronicle*'s account ascribed the discovery to "Signor Volta" and made no mention of the French.[98] In fact, it credited Volta with *more* than he had actually done, because it ascribed to him not just the invention of the pile but also the decomposition of water, stating only that Nicholson and Carlisle had "repeated" the process.[99] When Nicholson read this, he was understandably rather upset—was this his reward for his scrupulous delicacy in not publishing his discovery until Volta's letter had been read at the Royal Society? In a rare move for a British man of science at this time, he responded by direct engagement in the newspaper, writing a letter of "correction" to the editor. Although the pile was Volta's, he said, the decomposition of water was "not *repeated*… but originally made, by my friend, Mr. Carlisle, and myself." It was not "perfectly regular in the Doctor [Garnett] to anticipate the publication of those who had worked

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and others have been enabled to construct the necessary apparatus, and to repeat the ingenious Professor's very interesting experiments."

[98] The *Morning Chronicle* issue was printed before Banks and Rumford had arranged Garnett's public apology.

[99] Who wrote the article? Someone who sat in the lecture audience, almost certainly, who will probably remain impossible to identify (it seems very unlikely that Garnett had any involvement). The principal paid news-gathers for the London papers were the men who took down parliamentary debates and trials in the law courts (newspapers did not usually have any other reporting staff), and it is possible that one of these people was being sent to the Royal Institution to take notes for the *Chronicle*. Reports of Garnett's previous lectures had already appeared in several newspapers: e.g. *Oracle*, 12 April 1800. These reports may appear to prefigure the new kind of newspaper focus on the person of "the man of science" in Britain which I argue in the next chapter appeared with a surge in newspaper interest in Humphry Davy Royal Institution lectures around 1810. However, what really aroused press interest in the spring of 1800 was not Garnett or the content of his lectures, but the opening of the Royal Institution itself, a unique event involving a sizeable segment of London aristocratic Society, including media celebrities like the Duchess of Devonshire. One newspaper even went so far as to call the opening of the RI "an event the most dignified and important in its objects, and the most promising in its results, of any that has ever distinguished the British Nation." *Portsmouth Telegraph*, 24 March 1800.

out the discovery," and who were only waiting "to see the claims of Volta first laid before the Royal Society, before they themselves addressed the scientific world."[100]

His wait lasted four long weeks. Volta's work was finally read at the Royal Society only on 26 June—by which time Voltaic batteries were already being publicly advertised for sale in the London newspapers.[101] Simultaneously, Nicholson went to press with his account of the decomposition of water, so that the issue of his journal for 1 July contained the first detailed discussion in print of experiments with the battery.[102] The enthusiastic response probably did a great deal to soften any lingering feelings of indignation. Nicholson and Carlisle's work generated great excitement and many further investigations, both in Britain and abroad, and made *Nicholson's Journal* into one of the most active venues for the communication of new results in galvanism during the next years.

Intelligence is a thing easily leaked, and all but impossible to get back in the bottle once it is out. This was not usually a serious issue in science around 1800, primarily because the facts of discovery were all supposed to be made public anyway. In Britain, secrecy was particularly frowned on; it carried the stamp of the tradesman. "The keeping of secrets among men of Science is not the custom here," Banks declared on one occasion, "and those who enter into it cannot be considered as holding the same situation in the scientific world as those who are open and communicative."[103] Yet the *mechanism* by which information passed from private to public still mattered, because it was also the mechanism by which credit for a discovery could be verified and fixed. Volta's rather idiosyncratic strategy of sending his work in two parts,

[100] *Morning Chronicle*, 3 June 1800.
[101] E.g. *Morning Chronicle*, 17 June 1800, where batteries were being advertised for sale "on very moderate terms" by Francis Lowndes, self-described "Medical Electrician."
[102] *NJ* 4 (July 1800): 179-87.
[103] This was on the occasion of the scandal surrounding William Hyde Wollaston's discovery of Palladium, which he kept secret for commercial reasons. Banks's verdict was damning: "I do not think any Chemist in or out of the Society will ever converse with him in the same open and undisguised manner hereafter as they used to do and still will do with each other." Banks to Richard Chenevix, 22 March 1805, *Banks Corr.* 5:421-22.

magnified by his distance from London and the slow journey of the merchant carrying his second letter, had widened the gap of time between the discovery being informally *known* among a localized group of men of science, and its being officially announced at a Royal Society meeting. That gap was where the problems had arisen.

Twenty years before the gap would not have mattered, because the Royal Society was the only entity in London (beyond individuals) dealing in scientific intelligence. But now new information channels from private to public were destabilizing informal customs governing the announcement of discoveries. The Royal Institution and its grand public lectures, the consequent sudden interest of the newspapers in science, Nicholson's independent monthly journal (founded 1797, the first of its kind in Britain)—all these were new. The reign of the societies and academies was not over by any means, but they would now have to make their way amongst competitors in the field of scientific intelligence. Thus the episode with the battery does not illustrate the *failure* of the eighteenth-century model of scientific communication, but rather its end.

No one in the London scientific world was happy with the way news about the battery had spread. Banks was upset because the Royal Society had been preempted by the Royal Institution, and (worse) by a newspaper; Nicholson was frustrated by the Royal Society's delay, which forced him to hold back publication of his experiments in his own journal, only to be (inaccurately) scooped by the *Morning Chronicle* which had no such qualms; Garnett had merely been trying to show his audience something new and interesting, and it had backfired badly. In his letter to the *Morning Chronicle* Nicholson had complained that Garnett had made the Royal Institution "an office for collecting the conversations of philosophers, and immediately publishing them in the Daily Papers"—in other words, a place where private information

exchanged face-to-face could metamorphose into uncontrollable and highly mobile print news. This was problematic for both Nicholson and Banks, who had very different visions of scientific information but were united in wishing to assert some personal control over it. In contrast, the *Morning Chronicle* stood outside the scientific community; its editor did not care about these sensitivities. The newspaper was simply treating the battery as an interesting novelty; its concern with the pile was only incidental, the irony being that its hastily-composed and partly-inaccurate report, reprinted so many times, was the most potent vector in spreading the news further afield.

A final point: none of these troubles had anything to do with the actual publication of Volta's letters to Banks, which followed in the *Philosophical Transactions* a few months afterwards, by which time the contents were of minor interest.[104] The newspaper was a problem for London men of science not because it interfered with the authorized publication of Volta's text, but because it interfered with the public *announcement* of the discovery. Banks would not have cared if the newspaper had printed a report of the battery after it had been announced at a Royal Society meeting; similarly Nicholson was waiting for Volta's discovery to be "laid before the Royal Society" before he printed his own text on the decomposition of water. But whereas Banks held himself aloof from periodical print, Nicholson, as the editor of a journal, was sharing the periodical commons with the newspapers; he was in fact perhaps closer than he would have liked to admit to the *Chronicle's* way of doing things. Even if much of what *Nicholson's Journal* contained was reprinted material and snippets of intelligence rather than original publications, the model of scientific information his and similar journals embodied meant that public announcement and print publication of a discovery *could* actually happen at the same time. In

[104] Part II of the Philosophical Transactions for 1800 appears to have been issued in September, as a translation of Volta's paper into English was printed in the issue of the *Philosophical Magazine* published on 1 October. This was actually unusually fast—ordinarily the volume would have appeared some time in November.

1800 this was still rather unusual and, at a deep level, *accidental*. Only decades later would it become the standard way of doing things in science.

A Romantic Encounter: Ritter in Jena

The university town of Jena in the Duchy of Saxe-Weimar was at the heart of German Romanticism in 1800. Among the group of radical young thinkers who formed a self-conscious *avant-guard* in philosophy, literature, and the sciences, the resident *Physiker* was a 23-year-old student, Johann Wilhelm Ritter. Like Davy, he had been an apprentice to an apothecary, abandoning the trade to pursue what he saw as a purer scientific calling. Enrolling at Jena in 1796—an auspicious time, just before the advent of Friedrich Schelling's *Naturphilosophie*—he gravitated to the circle that formed around Schelling, the Schlegels, and the poet-philosopher Novalis.[105] He also became a favorite of Goethe, who to an extent held aloof from the Jean Romantics but was just as opposed as they to the arid materialism of eighteenth-century natural philosophy.[106] Drawing inspiration from the experiments of Volta and Alexander von Humboldt on combinations of metals, Ritter had thrown himself into work on galvanism, publishing a book on the subject in 1798.[107] It contained numerous careful and inventive experiments, but it had a grander aim in view: Ritter wanted to show that galvanic phenomena were integral to the

[105] An introduction to Ritter and Romantic science is Walter Wetzels, "Johann Wilhelm Ritter: Romantic Physics in Germany," in *Romanticism and the Sciences*, ed. Andrew Cunningham and Nicholas Jardine (Cambridge: Cambridge University Press, 1990), 199-212. The definitive biography is Klaus Richter, *Das Leben des Physikers Johann Wilhelm Ritter: ein Schicksal in der Zeit der Romantik* (Weimar: Böhlaus Nachfolger, 2003).
[106] On science and the Weimar and Jena circles see Robert Richards, *The Romantic Conception of Life: Science and Philosophy in the Age of Goethe* (Chicago: University of Chicago Press, 2002).
[107] Johann Wilhelm Ritter, *Beweis, dass ein beständiger Galvanismus den Lebensprozess in dem Thierreich begleite* (Weimar, 1798). The story of galvanism in Germany prior to the advent of the battery is told in Maria Trumpler, "Questioning nature: Experimental investigations of animal electricity in Germany, 1791-1810" (PhD diss., Yale University, 1992); see also Stuart Strickland, "Reopening the texts of romantic science: The language of experience in J.W. Ritter's Beweis," in *Trends in the Historiography of Science*, Boston Studies in the Philosophy of Science vol. 151 (Dordrecht: Springer, 1994), 385-96.

processes of life and, moreover, part of a unifying principle of "polarity" in the natural world.[108] Ritter never lost sight of this goal, which endeared him to the Jena circle but hampered the wider reception of his work. In Ritter's dynamic and vitalistic conception of Nature, the battery was no inanimate arrangement of metal: it was a special object, one of the links in the great chain of Being.[109] Perhaps no experimenter embraced the battery as Ritter did. He spent hours subjecting his body to the galvanic current, pursuing, painfully, a connection between galvanism and the processes of human sensation. In the end, this may have hastened his early death, aged thirty-three. Self-experiment was the altar on which he sacrificed himself to prove his faith as a seeker of Nature's secrets. Ritter saw experimental discovery as a quest for a glimpse behind the veil of the goddess, an insight into the harmony between his own body and the living universe.[110]

Political and social geography shaped how scientific information moved through Ritter's world, framed amidst the last years of the Holy Roman Empire and subsequently Napoleon's hegemony in Central Europe. In contrast to the situation in Britain and France, German science had no center, but instead was pursued by a multitude of people scattered over the 800 miles from Bern in the southwest to Königsberg in the northeast.[111] As in Jena, a university—patronized by the local prince—was often the nucleus around which a small circle of scientific devotees formed. Similar groups of greater or lesser size were dispersed throughout Germany, from a sizeable community in Berlin down to small towns where enlightened learning might be represented by a single doctor or apothecary. This decentralized structure encouraged the

[108] This was not a materialist position but the opposite: a vitalist physics. See Stuart Strickland, "Circumscribing Science: Johann Wilhelm Ritter and the Physics of Sidereal Man" (PhD diss., Harvard University, 1992).
[109] Stuart Strickland, "The Ideology of Self-Knowledge and the Practice of Self-Experimentation," *Eighteenth-Century Studies* 31 (1998): 453-471, 455.
[110] On Ritter and self-experiment see Strickland, "Ideology of Self-Knowledge"; more generally Simon Schaffer, "Self-Evidence," *Critical Inquiry* 18 (1992): 327-62.
[111] Similar points might apply to northern Italy and other places where science was actively cultivated but not centralized—though in the Italian case the situation is complicated by the Revolutionary and Napoleonic reorganizations, and eventually direct rule from Paris.

formation of a large number of periodicals, which linked readers scattered across German-speaking Europe.[112] General learned periodicals of the Republic of Letters had connected German scholars since the seventeenth century—beginning with the *Acta Eruditorum*, which, setting the pattern for these publications, covered all fields of intellectual life, from theology and history to mineralogy and mathematics.[113] Increasingly more important to the practice of the experimental sciences, however, were the new specialist scientific periodicals founded in the last decades of the eighteenth century, which served as regular connections between widely-dispersed communities of scientific enquirers who needed a common channel to share new experiments and observations.[114] These periodicals—conducted, unlike the Latin *Acta*, in the vernacular—shared many features with the monthly journals that appeared around the same time in Britain and France.[115] The difference was that British and French periodicals were defined by their relationship to the national center of scientific power and intelligence—either in opposition to it, as with *Nicholson's Journal's* relationship to Joseph Banks's circle in London, or by being united with it, as with the *Annales de Chimie*, founded by the French Académie's leading chemists. But in Germany no such strong center existed, and journals were free to form their own virtual centers with little competition. This happened in a cluster of specialized journals in the German states, in which chemistry was likewise strongly represented—among them Lorenz Crell's *Chemische Annalen*, Gilbert's *Journal de Physik*, and Scherer's *Allgemeines Journal der Chemie*.

[112] See Thomas Broman, "J. C. Reil and the 'Journalization' of Physiology," in *The Literary Structure of Scientific Argument: Historical Studies*, ed. Peter Dear (Philadelphia: University of Pennsylvania Press, 1991), 13-42.
[113] Hub. Laeven, *The "Acta eruditorum" under the editorship of Otto Mencke (1644-1707): the history of an international learned journal between 1682 and 1707*, trans. Lynne Richards (Amsterdam: APA-Holland University Press, 1990).
[114] In the physical sciences, this began with Lorenz Crell's *Chemische Annalen*: see Karl Hufbauer, *The Formation of the German Chemical Community, 1720-1795* (Berkeley: University of California Press, 1982), esp. 62-82.
[115] The *Acta* continued to appear in Latin up until it ceased publication in 1782.

Although Ritter's Romantic science and his intellectual climate at Jena were in many ways unique, his first interactions with the battery in fact provide an excellent example of how men of science around 1800 worked with different sources of information when pursing ongoing experimental investigations. Ritter was painfully aware that information about the battery reached him relatively late, and he found himself having to construct a picture of what had already been done with the pile, piecing together different pieces of the intelligence jigsaw like a newspaper reader trying to work out the moves in a distant military campaign. Uncertain about what had already been done in London and elsewhere, he had to craft his experiments and opinions in response to a changing idea of other people's techniques and assertions. When doing this, he drew on a whole range of different communications media. His experience illustrates how this period's hybrid and criss-crossing scientific intelligence system looked to someone on the ground.

Over a few weeks in September 1800, information about the battery reached Ritter by *four* independent channels. The first news came in a chain of letter-writing that had begun with Volta. In the first half of the month, Johann Heinrich Voigt, a professor at Jena who also edited a new scientific journal, the *Magazin für den neusten Zustand der Naturkunde*, received a letter from the Prague physician J. N. Mayer. Mayer was forwarding on extracts of a letter sent to him by the Italian nobleman Marsillio Landriani, Volta's main scientific correspondent at Vienna, to whom Volta had described the pile in a letter written around the end of April.[116]

[116] The description from Landriani was subsequently printed by Voigt in his journal: *Magazin für den neusten Zustand der Naturkunde* 2 (1800): 215-19; see also Landriani to Volta, 8 May 1800, in *Epistolario di Alessandro Volta*, vol. 4 (Bologna, 1953), 5-7. By the time he wrote this reply to Volta, Landriani had built a battery in Vienna and (he told Volta) was about to send a special gold and silver version of the battery as a present to the Elector of Saxony. But four months went by before the battery was known in Jena, which was less than 300 miles away from Vienna (and only 100 from Dresden) and not cut off by war.

Voigt was able to build a replica of Volta's apparatus from the letter, which, however, described a "crown of cups" configuration (Volta's other form of the battery) rather than the pile. He then asked the young Ritter, as the local expert on galvanism, to join him in experimenting around 14 September, after which Ritter seems to have taken control of the investigations.[117] About this time, the much-echoed voice of Thomas Garnett was heard in Jena, speaking from a copy of the *Journal de Bruxelles* newspaper, which had reprinted the account of the Royal Institution lecture from the Paris *Moniteur*.[118] The *Journal de Bruxelles* text was itself soon reprinted in Jena, not in a newspaper but in a periodical recently founded to promote the new *Naturphilosophie*, Schelling's *Zeitschrift für spekulative Physik*.[119]

From the *Journal de Bruxelles* Ritter took two crucial items of information: the description of the pile form of the battery, and Garnett's account of its effects on water (which mentioned the production of hydrogen gas but not oxygen). This threw Ritter into a feverishly-creative two-week program of experiments. Soon he had decomposed water into hydrogen *and* oxygen, and separately collected the gasses, just as, he did not yet know, Nicholson and Carlisle had already done.[120]

Ritter was fascinated by a strange aspect of this experiment that had also troubled Nicholson and Carlisle in London four months before. One wire in the water (the positive) produced oxygen, and the other (the negative) hydrogen. The wires continued to produce bubbles of gas even when held several inches apart. But if the water at the positive wire is being chemically split into oxygen and hydrogen, and the oxygen thus released is rising as bubbles,

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[117] Ritter described his experiments with the battery in *Magazin für den neusten Zustand der Naturkunde* 2 (1800): 356-409 (account dated by Ritter 28-30 September 1800, and followed by editorial comments by Voigt) and in his letter (dated "October 1800") printed in *Allgemeine Literatur-Zeitung*, Intelligenzblatt, 1 November 1800, no. 180, col. 1509-1511; these give key details of the micro-chronology and the different sources of information Ritter used.
[118] *Journal de Bruxelles*, 3 Fructidor an 8 (21 August 1800). It is unclear why a copy of this paper ended up in Jena.
[119] *Zeitschrift für spekulative Physik*, 1 (1800): 149-52.
[120] Ritter used gold wires rather than the platinum Nicholson and Carlisle had employed.

what on earth is happening to the hydrogen *left over*? Why is it not also released at the positive wire? And how could it *possibly* appear at the negative wire (as appears to be the case), when, in the water between the two wires, no chemical changes seem to be occurring, and no bubbles of gas can be seen? "This new fact still remains to be explained," Nicholson had commented, "and seems to point at some general law of the agency of electricity in chemical operations."[121]

Almost the whole of the nineteenth century would pass before this notorious paradox was definitively laid to rest.[122] But in fact Ritter was one of the few who refused to be troubled by it, because for him it was no paradox at all but a beautiful piece of experimental evidence for one of the essential tenets of his own *Naturphilosophie*-inspired chemistry: water as an elemental substance, the base of all matter. Something was happening at the wires to turn water into hydrogen and oxygen, and this something seemed to Ritter incompatible with the basic result of Lavoisier's chemistry that water was hydrogen in combination with oxygen in a 2:1 ratio (an idea that was less than 20 years old in 1800, and still doubted in some quarters).[123] Ritter pursued the problem with some clever new experiments. In one, he performed the "so-called decomposition" in a V-shaped tube with the two wires in separate portions of water, divided by a

[121] The symmetric argument obviously applies equally to oxygen left over at the negative wire. As Nicholson and Carlisle put it in 1800: "We had been led by our reasoning on the first appearance of hydrogen to expect a decomposition of the water; but it was with no little surprize that we found the hydrogen extricated at the contact with one wire, while the oxigen fixed itself in combination with the other wire at the distance of almost two inches." *NJ* 4 (July 1800): 183. Hasok Chang has termed this the "distance problem"; see his *Is Water H2O? Evidence, Realism and Pluralism* (Dordrecht: Springer, 2012), 71-131.

[122] A reminder of the difficulties investigators faced in 1800 in making sense of even the most seemingly basic galvanic phenomena. Although various schemes of explanation were proposed over the years, arguably no chemist was ever able to dispose of this paradox in a way that gained wide assent until Arrhenius's theory of ionic dissociation in the 1880s, which required a complete rethinking of the nature of water; see Chang, *Is Water H2O?*

[123] There seems to be a longstanding misconception in much of the historical literature that Ritter believed that hydrogen and oxygen were composed of positive and negative electricity combined with water. In fact, according to Bernd Klengel, this view was not held by Ritter at all, who actually rejected the fluid theory of galvanism on which it is based; rather, the idea arose from an inaccurate précis of Ritter's views given in Paris by the Prussian physician Michael Friedländer: Bernd Klengel, *Über Galvanismus und deutsche Träumereien: Zur Rezeption romantishcer Naturforschung in Frankreich zwischen 1800 und 1820* (Stuttgart: Steiner, 2010), 81.

denser lower layer of concentrated sulfuric acid. Oxygen and hydrogen were produced separately at both sides, with no apparent change in the acid.

In the first description of his work printed in Voigt's magazine, Ritter mostly let his experiments speak for themselves, only asserting that the separate production of hydrogen and oxygen from water proved that no decomposition had taken place. He used much bolder language a few weeks later in a letter to the *Intelligenzblatt* ("Intelligence-sheet") of the *Allgemeine Literatur Zeitung*, a daily publication which was the preeminent source of intellectual news in Germany. Water was "*an entirely simple body*," it was "the bearer of all chemical individuality on Earth… the homogeneous source of weight in all Earth-matter." Materialism and the hated Newtonian philosophy had received their death blow. "The inanimate Automaton that modern natural philosophy has made of our Earth has run down… the world is One again at last."[124]

In the end, few outside Ritter's Jena circle were prepared to accept a claim that must entail, as one skeptic put it, "the ruin of the magnificent edifice of modern chemistry."[125] And for Ritter the news of the invention of the battery was a bittersweet experience, the memory clouded by a lingering ghost of what might have been. Just as he was getting ready to write up his investigations for print in Voigt's magazine, he received some more, and unwelcome, intelligence. Extracts of letters from London from Banks and Dr Edward Ash, forwarded on to Voigt by the Gottingen naturalist Johann Friedrich Blumenbach, contained hints of similarly-detailed experiments on water by British chemists.[126] The full blow came a few days later: a

[124] *Allgemeine Literatur-Zeitung*, Intelligenzblatt, 1 November 1800, no. 180, column 1511.
[125] Johann Trommsdorff to Jean-Baptiste Van Mons, 16 March 1801, extract in Van Mons's *Journal de chimie et de physique*, 1 (1801): 98-99.
[126] Banks to Blumenbach, 13 May 1800 and 11 July 1800; Edward Ash to Blumenbach, 3 August 1800, printed in *Magazin für den neuesten Zustand der Naturkunde* 2 (1800): 292-94. Probably these were sent via the official messengers that kept London in touch with the Electorate of Hannover, which was in personal union with Britain under George III; Banks and Blumenbach had used the messengers as a conduit on previous occasions.

copy of *Nicholson's Journal* had reached Halle, some forty miles to the north of Jena, and in a letter from a friend there he received news of the experiments previously announced by Nicholson, Carlisle, and others in London.[127] Ritter's experiments still had some novelty, but less than he had hoped. In the end, he had to settle for asserting a claim to having independently discovered what was already known elsewhere.

Consequently Ritter was (helpfully for us) very precise in his report of his experiments with the battery about the dates and different kinds of information he had received: he wanted to demonstrate how *little* intelligence he had when experimenting—aware, of course, that his audience would have to take his assertions on trust.[128] As Ritter knew, men of science around Europe did not have an equal start in the race to develop the Voltaic battery: they were all working on different incommensurate tracks of time, defined above all by their access to information. Yet when it came to the judgment of "discovery," this was considered irrelevant. The scientific world was international and where someone happened to discover something was considered accidental—even if in practice uneven flows of information through a particular locality could confer crucial advantages. What if—we can imagine Ritter asking himself—Volta had made a different choice, and had announced the battery only through Vienna, rather than through London *and* Vienna?[129] Then perhaps he, Ritter, rather than the London men, would have been the first to investigate the chemical action of the battery on matter—perhaps his own ideas about the elemental nature of water would have got a better hearing. With this time-critical aspect of intelligence so evident and with the expectation of more discoveries just over the

[127] The letter was from Horckel, a medical professor; it was a reply to a letter from Ritter, who had just written to Horckel describing his experiments. Excerpt of Ritter's letter printed in *Annalen der Physik* 6 (1800): 470-71.
[128] As do we. However, I have no reason to doubt Ritter's account—backed up by Voigt—of when he received his information.
[129] Austria was, after all, Volta's oldest patron and employer, and the ruler of northern Italy in the spring of 1800.

horizon, it was becoming more important than ever to stay informed about the progress of science beyond one's immediate circle.

Le Jugement de Paris

Sometime during the afternoon on 8 August 1793, eighteenth-century science came to an end. Or at least, that was how it must have appeared to the savants of the Paris Académie des Sciences, confronted with the decree of the National Convention legislating their venerable institution out of existence. Disruptive in the short term, the Revolution's break with history cleared the ground for the subsequent emergence of a fertile new matrix of institutions and sites for French scientific life.[130] Educational institutions led the way, with the foundation of the École Normale, the École de Santé and the École Polytechnique in 1794 providing teaching posts, laboratory facilities, and in the case of the Polytechnique training for the next generation in physical science. The two main French scientific journals from before the Revolution, the *Annales de Chimie* and the *Journal de Physique*, both revived and took on a new importance, as did a minor scientific society founded in 1788 for the exchange of scientific news, the Société Philomathique, which provided an alternative venue for collective activity after the Académie's demise.

Nonetheless, the most important development was the foundation of the Institut National, which held its first meeting in December 1795. The Institut was, in a limited sense, the old Paris Academies raised from the dead, with the Académie des Sciences reborn as the "Première

[130] The Académie's abolition ultimately had little to do with science as such; rather, it slid into the abyss (along with every other learned academy in France) amidst the Jacobins' general obliteration of the last vestiges of elite and corporate privilege. On its closure and the aftermath, including the subsequent foundation of the Institut, see Roger Hahn, *The Anatomy of a Scientific Institution: The Paris Academy of Sciences 1666-1803* (Berkeley: University of California Press, 1971), 226-312; Charles Gillispie, *Science and Polity in France: The Revolutionary and Napoleonic Years* (Princeton: Princeton University Press, 2004).

Classe" of three "classes" (sections).[131] But it was supported by an entirely new state and a new political culture. The Institut was originally conceived as the embodiment of a broad vision of Republican intellectual life in which society was to be transformed by new human sciences, allied to the natural knowledge of the Première Classe. Napoleon, however, thought this program of the "idéologues" corrosive to political and social stability, and when he came to power in 1799 he soon imposed his own vision of intellectual activity on the Institut (the "moral and political sciences" section, the idéologues' main stronghold, was totally eliminated in 1803). With the marginalization of the human sciences and politics, knowledge of nature came to stand in for knowledge in general, and the natural sciences' Première Classe—to which Napoleon himself had been elected in 1797 while only a 28-year-old general—emerged as the preeminent intellectual body in France. Napoleon signaled this by the patronage he lavished on favored *savants* like Berthollet, Monge, and Laplace; furthermore, his distinctive type of rationalized bureaucratic administration melded the sciences with the economic and military interests of the state (his Minister of the Interior from 1800-04 was the chemist Jean-Antoine Chaptal).[132] In Napoleonic France, the natural sciences thus attained a level of cultural prestige and state endorsement which exceeded anything they had ever enjoyed before anywhere in the world—but at the price of subservience to the interests of the state and Napoleon himself.

When the battery came to Paris in August 1800 the Institut National was naturally the undisputed center of scientific information in the capital. It met continuously throughout the year, every five days until 1802 and afterwards weekly.[133] At meetings incoming letters or other

[131] Unless otherwise noted I will used "the Institut" to refer to the Première Classe—i.e. the scientific part only. The *Première Classe* was the "Classe de Sciences Mathématiques et Physiques," the best translation of which is probably "mathematical and natural sciences" ("sciences physiques" covered all non-mathematical science, including chemistry, natural history, and medicine).
[132] Maurice Crosland, *The Society of Arcueil: A View of French Science at the Time of Napoleon I* (Cambridge, MA: Harvard University Press, 1967).
[133] The reason for the change was the abolition of the Revolutionary calendar's ten-day week, the *décade*.

reports of discoveries were read, and "memoirs" on new work were presented and then parceled out to separate committees for examination, while other committees reported back on their findings. Hence the activities of the Institut were a reliable source of scientific news. Much of this found its way into periodical print: in the government newspaper the *Moniteur*, in the *Annales de Chimie* (run by the Institut's chemists) and in the Société Philomathique's *Bulletin des Sciences* newssheet. These were all venues controlled by the Institut's members, but their dominance of print was balanced to an extent by other outlets. British scientific intelligence often arrived through reprints in the Geneva-based *Bibliothèque Britannique*; within Paris, a degree of opposition to the scientific establishment was presented by the *Journal de Physique*, edited by the cantankerous Jean-Claude Delamétherie.

For the French the pile was a foreign discovery, arriving in France in a snippet of newspaper intelligence months after experimentation with batteries was already underway in Britain and Italy. Had the battery been discovered in Paris, it would have been the Institut's to judge and control. But even the Institut's system was insufficient to master discovery at a distance. During the battery's early career members of the Institut had to work hard to impose order and meaning on a mass of information on the discovery originating both within France and abroad. Moreover, in part because of the battery's ambiguous status, approaches diverged even within the French scientific elite.[134] The Institut never did manage to deliver a judgment on the battery that commanded wide assent; each attempt was destabilized by the arrival of fresh intelligence from without.

The first experiments on Volta's pile in Paris were performed by a colorful outsider, Étienne-Gaspard Robertson—showman, lecturer, balloonist, and impresario of the "phantasmagoria" magic lantern show, which he performed every evening in the gothic setting of

[134] On the divergent views among the elite see Sutton, "Politics of Science."

a decaying ex-convent, the *Cour des Capucines*.[135] After reading the newspaper account in the *Courier de Londres*, Robertson quickly built a battery of his own, and, beginning on 28 August, started publishing reports of experiments in the *Journal de Paris*, the major daily newspaper where he advertised his shows.[136] He even incorporated the battery into his nightly *fantasmagorie*, which already featured galvanic and electrical effects.[137]

Nonetheless, Robertson still made sure to submit his work to a meeting of the Institut, on 29 August.[138] The *savants* were not impressed that Robertson's experiments "had been printed in several newspapers before they were even communicated to the Institut."[139] But he provided a reason to formally refer the battery to a "*Commission du Galvanisme.*"[140] Led by the physician Jean-Noël Hallé, the commission began experiments at the Paris Ecole de médecine, where a battery was soon being "kept in daily activity." Over the next weeks, the Commission's progress was announced by regular bulletins from Hallé at the Institut, followed by printed news in the *Bulletin des Sciences*.[141]

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[135] The British-sounding "Robertson" was a stage-name; his original surname was "Robert."

[136] *Journal de Paris*, 10, 15, 17 Fructidor, 2e jour complémentaire an 8 (28 August, 2, 4, 19 September 1800). Robertson's report implies that he saw the news of the battery in the *Courier de Londres* rather than in the slightly later reprinting in the *Moniteur*.

[137] For example, the advertising in *Journal de Paris*, 30 Ventôse an 8 (21 March 1800) promised "Apparition de Fantômes des Ombres de Franklin, de Mahomet terrassant l'ange de la mort, &c.—Les séances varient & commencent alternativement, soit par les Illusion, les Optiques, le Ventriloque, l'Harmonica, ou les Expériences peu connues de physique, sur les Gaz, l'Air, la Combustion, le Galvanisme ou l'Électricité.—Tous les jours à 6h & demie.—On y jouira de l'expérience de *la Femme invisible*."

[138] *PVSA* 2:218, 11 Fructidor an 8 (29 August 1800).

[139] *PVSA* 2:369, 11 Messidor an 9 (30 June 1801).

[140] The commission comprised Hallé, Pelletan, Sabatier (all physicians), Charles and Fourcroy. All were drawn from a previous commission set up in 1796 to examine Alexander von Humboldt's experiments on animal electricity. During some of the experiments they were joined by other members of the Institut including Laplace and Vauquelin.

[141] Beginning on 3 September when Hallé told the Institue of the commission's experiments "to verify the phenomena announced by the English newspapers." *PVSA* 2:221, 16 Fructidor an 8 (3 Sept. 1800); *PVSA* 2:228, 26 Fructidor an 8 (13 Sept. 1800); *PVSA* 2:232, 1 Vendémiaire an 9 (23 Sept. 1800); *PVSA* 2:273-274, 6 Frimaire an 9 (27 Nov. 1800); *PVSA* 2:280, 26 Frimaire an 9 (17 Dec. 1800); *BDS* 2 (Fructidor an 8; Aug./Sept. 1800): 144; *BDS* 2 (Vendémiaire an 9; Sept./Oct. 1800): 151; *BDS* 2 (Frimaire an 9; Nov./Dec. 1800): 165-67. It may seem remarkable that no intelligence of the battery had yet arrived from northern Italy, which had been in French hands since late June and where Volta's work was by now quite widely known. In fact, an army medical officer, Citizen Biron, had seen Volta perform experiments with the battery in Italy; returning to Paris, he built a battery and presented it to the Institut—unfortunately for him, three weeks after the news had arrived in Paris from London and

Chemical phenomena were of minor interest in the first French investigations of the battery during August-December 1800. Because of the older association between galvanism and physiology, the savants were initially primarily interested in the battery's effects on living bodies and its possible applications to medicine.[142] Hallé himself even performed several inconclusive medical trials.[143] Even after other approaches began to gain ground, this medical-physiological view remained strong among a large number of investigators, both in France and abroad. In 1802 its Parisian adherents formally organized themselves as the Société Galvanique, with regular meetings for experiments at the Oratorie, a former church near the Louvre—and even briefly a dedicated vehicle of intelligence, the *Journal du Galvanisme*.[144] A second mode of enquiry was to treat the battery as an electrostatic object using the methods of eighteenth-century electricity. This was later developed further by the young Jean-Baptiste Biot, who in 1801 began a program of assimilating the battery into the new "Laplacian" model of *la physique*, giving it a precise treatment in terms of the distribution of electric charges.[145] Both approaches seemed more relevant frameworks than chemistry in the first months of the battery's career in France. In December 1800 a report in the *Bulletin des Sciences* summed up the *Commission du*

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ten days after Robertson had offered his own experiments with the battery to the Institut. *PVSA* 2:224, 21 Fructidor an 8 (8 Sept. 1800).

[142] On the continuing importance of the physiological and medical approaches see Christine Blondel, "Animal Electricity in Paris: From Initial Support, to Its Discredit and Eventual Rehabilitation," in *Luigi Galvani International Workshop Proceedings*, ed. Marco Bresadola and Giuliano Pancaldi (Bologna: University of Bologna, 1999), 167-85.

[143] *BDS* 3 (Messidor an 9; Jun./Jul. 1801): 31-32.

[144] The *Journal du Galvanisme* appeared monthly from 1803 to 1804; members of the Société also frequently communicated their work in Delaméthrie's *Journal de Physique*. One of them, the physician Lagrave, produced a macabre new variation on Volta's battery: a voltaic pile built not of metal but out of slices of muscle and brain: *JP* (Ventôse an 11; Feb./Mar. 1803): 235-36. Many researches tending in this direction are collected (often reprinted from journals and other print sources) in the Société Galvanique member Pierre Sue's sprawling *Histoire du Galvanisme*, 4 vols. (Paris: Bernard, 1802-1805).

[145] *BDS* 3 (Fructidor an 9; Aug./Sept. 1801): 45-48; *AC* 39 (30 Fructidor an 9; 17 Sept. 1801): 242-50; Sutton, "Politics of Science," 345-50; Eugene Frankel, "J.B. Biot and the mathematization of experimental physics in Napoleonic France," *Historical Studies in the Physical Sciences* 8 (1977): 32-72.

Galvanisme's findings. It covered the decomposition of water very briefly; most space was devoted to its effects "on living bodies" and to the battery's electrostatics.[146]

Of course there is really no reason why we should expect the pile to have been received as a chemical device in France, as it had been in Britain. The *Morning Chronicle's* intelligence transmitted through the *Courier de Londres* was brief and ambiguous; it described how to build a battery but hardly furnished a new framework in which to investigative it. Only when information from other places began to reach Paris did the chemistry of the battery start to look important. Details of Nicholson and Carlisle's experiments arrived in France sometime during October via a reprint from *Nicholson's Journal* in the *Bibliothèque Britannique*; even then, however, no one in France seems to have showed much interest in investigating the decomposition with gold or platinum wires to release oxygen as well as hydrogen.[147] What really kick-started chemical investigations of the battery in Paris was apparently Ritter's work on the production of the two gases in separate containers—or rather a somewhat inaccurate rendering presented to the Institut in January by a Prussian physician just arrived in Paris, Dr. Michael Friedländer.[148] "Galvanism," declared the *Bulletin des Sciences*, "having appeared only interesting to physiology, is transposed into the field of chemistry, where it seems inclined to

[146] *BDS* 2 (Frimaire an 9; Nov./Dec. 1800): 165-67.
[147] *BB* 15 (1800): 3-23; this issue of the *Bibliothèque Britannique* also reprinted William Cruickshank's and Wiliam Henry's experiments with the battery from the July and August issues of *Nicholson's Journal*. Hallé's report published in December 1800 in *BDS* for Frimaire an 9 describes only a decomposition in which one wire is oxidized. The *Annales de Chimie* contains nothing on the battery until a very belated reprint in February 1801 of Robertson's experiments (probably lifted from the *Journal de Paris*).
[148] Friedländer presumably brought a copy of Ritter's experiments in *Voigt's Magazin* with him when he travelled from Berlin to Paris, as his French text is a summary of Ritter's German account and whoever engraved the accompanying plate when Friedländer's report was printed in the *Journal de Physique* must have been copying directly from the plate from *Voigt's Magazin*; *PVSA* 2:291, Nivôse an 9 (6 Jan. 1801); *JP* 52 (Pluviôse an 9; Jan./Feb. 1801): 101-106. For Friedländer (subsequently an important mediator between French and German science) and the difficult reception of Ritter's work in Paris see Bernd Klengel, *Über Galvanismus und deutsche Träumereien: Zur Rezeption romantishcer Naturforschung in Frankreich zwischen 1800 und 1820* (Stuttgart: Steiner, 2010), 67-87.

shake up even the latest theories."[149] During the first months of 1801, Parisian savants finally began to puzzle over the "most singular phenomenon" of the separate production of the oxygen and hydrogen.[150] The enigma divided the Parisian scientific elite. Monge, Berthollet and Hassenfratz thought the water lost one of the gasses at each wire but retained the other in excess; Fourcroy, Vauquelin and the young Thenard thought that the missing gas must somehow be conducted invisibly through the water to the other wire. Unsurprisingly, no one in Lavoisier's homeland seemed inclined to accept Ritter's contention that the water had not been decomposed at all.[151]

Wartime Exchanges, Peace, and a Prize

Early in February 1801, aged 22, Humphry Davy stepped off the mail coach in London and into a new life. He had come to talk about a job at the Royal Institution. There, more quarrels between Count Rumford and lecturer Thomas Garnett (now also in poor health) had followed the debacle with the lecture on the battery. And so Rumford had brought the young Davy up from Bristol, out of the radical provincial obscurity of Doctor Thomas Beddoes's "Pneumatic Institution," and into the metropolitan heart of British science—officially to hire him as assistant lecturer, but really to make him Garnett's replacement.[152]

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[149] *BDS* 3 (Floréal an 9; Apr./May. 1801): 13.

[150] See Cuvier's report of the Institut's activities in *Moniteur*, 6 Floréal an 9 (26 April 1801), pp. 906-907: "How is it that the oxygen and hydrogen, originating from the same molecule of water, appear at separate points? And why does each appear exclusively at a wire connected to only one of the ends of the pile, and never at the other?"

[151] *BDS* 3 (Floréal an 9; Apr./May. 1801): 13; Ritter's view is dismissed in Cuvier's report in the *Moniteur* as "so inconsistent with the totality of all other chemical phenomena, that it would have been impossible to accept even if no other satisfactory explanation of the experiment could have been found."

[152] Davy to Grace Davy, 11 February 1802, using transcription from Davy Letters Project (http://www.davy-letters.org.uk/); original in London Science Museum Archive, MS 333/3: "I am to have as soon as I have prepared myself for the situation the professorship of Chemistry &c with 300 guineas a year; the profits of my labours &c &c—I hope to be able to undertake it next year till I do undertake it I am to be Director of the Chemical Department." (Davy was promoted to Lecturer in June 1801 when Garnett left the RI, and formally appointed Professor of Chemistry on 31 May 1802.)

It was the capstone to a successful year. Just when Davy's work on nitrous oxide (laughing gas) was finished and about to be printed, Beddoes's circle at Bristol had received news of the battery from London.[153] More details had come in the next issue of *Nicholson's Journal*, carried down to Bristol on the mail coach.[154] "We have been repeating the Galvanic experiments with success," Davy announced to his old mentor Davis Giddy, "an immense field of investigation seems opened."[155] A few months later he had started to make his own inroads. "I have made some important galvanic discoveries," he proclaimed to his friend Samuel Taylor Coleridge, "which seem to lead to the door of the temple of the mysterious god of Life."[156]

Such raptures did not long survive Davy's move to London. Davy later vigorously opposed the suggestion—unpalatable to his Royal Institution audience because of connotations of materialism—of any link between galvanism and the essence of life. In fact, his own work already pointed away from the living world. To Giddy he declared that galvanism was "a process purely chemical," depending "wholly on the oxydation of metallic surfaces."[157] This conceptualization of the battery as an ongoing chemical reaction soon gained broad acceptance in Britain—London chemists like Nicholson already held similar views—but its reception on the Continent was more mixed.

Davy publicized his results in a series of short letters to Nicholson, appearing in *Nicholson's Journal* between September 1800 and February 1801. Partly thanks to Nicholson's own discovery that the battery could decompose water, *Nicholson's Journal* had quickly become

[153] It is unclear how Davy first heard the news, but it seems likely he received it through both the newspapers and a short note that appeared in the *Philosophical Magazine* at the beginning of June; his *Researches, Chemical and Philosophical: Chiefly Concerning Nitrous Oxide* (London: Johnson, 1800), printed by the time he wrote to Giddy on 3 July, contains an oblique reference on p. 568 which matches the news in the *Philosophical Magazine*.
[154] *Nicholson's Journal* was published in London on 1 July, and it was certainly in Bristol by the time Davy wrote his letter of 3 July.
[155] Davy to Davis Giddy, 3 July 1800, reproduced in John Ayrton Paris, *The Life of Sir Humphry Davy* (London: Colburn, 1831), 58-59.
[156] Davy to Coleridge, 26 November 1800, Morgan Library, New York City, MA 1857.11.
[157] Davy to Davis Giddy, 20 October 1800, reproduced in Paris, *Life*, 72-74.

a central exchange for further experiments with the pile. Between 1800 and 1806, the Royal Society's *Philosophical Transactions* contained only two papers on galvanism (one of them offered by Davy, as a necessary prelude to becoming a Fellow).[158] *Nicholson's Journal* contained dozens of communications. Its speed and the possibility of publishing shorter pieces of experimental intelligence like Davy's letters made it well-suited to the feverish pace of work inaugurated by the appearance of the battery.[159] The same phenomenon occurred in other monthly journals like the *Annalen der Physik* or the *Bibliothèque Britannique*. Across Europe, the application of techniques of periodical and news journalism to scientific communications grew up in symbiosis with the pursuit of progress in galvanic discovery.

Armed with his view of the battery as a series of chemical processes, Davy moved ahead by varying his experiments in as many ways as possible, first trying different versions of the decomposition apparatus and later reconfiguring the inside of the battery itself. Like Ritter (and Volta) he tried his own body in the circuit—but unlike them, he was uninterested in reporting subjective sensations; for him, the body was merely a means to reveal new chemical phenomena.[160] He initially used himself as a connector to separate the decomposition process into two halves, so that oxygen was given out in one vessel and hydrogen in another—but soon substituted animal muscle fibers, which gave the same result. Davy was fascinated, as Ritter was, by the separate production of the gasses, but it did not shake his faith in the compound nature of

[158] Not counting Volta's original memoir on the battery. Davy's memoir is *Philosophical Transactions* 91 (1801): 394-402, although in fact even before this was read to the Royal Society news of the most important results had already appeared in *NJ* 5 (May 1801): 78. The other *Philosophical Transactions* memoir on galvanism was by the chemist William Hyde Wollaston (already a Fellow); it presented an experimental argument for the identity of galvanism and "common electricity." *Philosophical Transactions* 91 (1801): 427-34.

[159] A similar point was made long ago by Samuel Lilley, "Nicholson's Journal, 1797-1813," *Annals of Science* 6 (1948): 78-101.

[160] This is all the more striking because reports of subjective bodily sensations had been central to Davy's previous work with nitrous oxide. No doubt Davy did carry out self-experimental investigations with the battery, but his letters to Nicholson—like other contributions in *Nicholson's Journal*—were focused squarely on its effects on inanimate matter.

water. Instead, he tried varying the substances in the vessels, decomposing ammonia and sulphuric acid and recording a long series of observations of the battery's action on different acids and alkalis.[161]

Crucially, his contributions were articulated in dialogue with others writing in *Nicholson's Journal* at the same time, taking advantage of its regular monthly rhythm. When the Woolwich chemist William Cruickshank described a new trough configuration for the pile—which soon became standard in Britain but not elsewhere—Davy quickly switched over to this form.[162] Volta's pile had been built in a way which had made it hard to get at what was going on inside; the trough opened up the battery and reduced it to essentials, eliminating Volta's sodden cardboard and presenting a clean arrangement: repeated series of two metals separated by a liquid. Davy started using it to vary the components of the battery, trying different acids instead of the water. His conclusion was that the power of the battery depended on the ability of the liquid to oxidize the zinc, and this led him to build a battery "more powerful than any hitherto constructed" using nitric acid.[163] A crowning achievement, after he moved to London, was to use carbon to create a battery containing no metals—thus posing a problem for Volta's theory that the galvanic current arose from metallic contact.[164]

Davy's arrival in London coincided (unbeknownst to him) with the launch of a small reputation on the Continent. Reprinting spread information through different scientific journals just as it moved intelligence through the newspapers. Since shipping printed material was both difficult and expensive, it formed the principal means by which scientific work printed in

[161] *NJ* 4 (September 1800): 275-81.
[162] Cruickshank's new battery described in *NJ* 4 (September 1800): 258-259. Cruickshank was "chemist to the Ordnance" at the Royal Artillery Hospital at Woolwich; see A. Coutts, "William Cruickshank of Woolwich," *Annals of Science* 15 (1959): 121-133.
[163] *NJ* 4 (November 1800): 337-42. Later experiments showed that hydrogen was given off inside the battery, providing further evidence that chemical change was behind the whole process: *NJ* 4 (December 1800): 394-402.
[164] *Journals of the Royal Institution* 1 (nos. 4, 5; January 1802): 79-80.

journals in one country was disseminated among foreign audiences. Only a single copy of a text needed to make the trip. For example, a copy of the issue of *Nicholson's Journal* containing the first decomposition of water experiments reached Halle (then part of Prussia) by late September, where its information was reprinted in one of the leading German journals, Ludwig Wilhelm Gilbert's *Annalen der Physik*, which formed a clearing house for new galvanic information in Germany just as *Nicholson's Journal* did in Britain.[165] Davy's letters to Nicholson were also reprinted in translation by Gilbert as later issues of *Nicholson's Journal* reached Halle, so that Davy became a voice in the active conversation around the battery among the journal's German contributors.[166] Gilbert transformed Davy's texts, splicing them together, hedging them with editorial notes, altering sections or adding new headings to make them clearer to the German audience, and sometimes deleting whole paragraphs.[167] In an important sense, this was not a straightforward republication or translation of Davy's work, but new documents in a new communicative context beyond Davy's control or immediate knowledge.

In France, a similar process was in operation; there, however, it was carried on by a monthly journal whose explicit mission was to reprint British science: the *Bibliothèque Britannique*. This was run out of Geneva (part of France since 1798) by the Swiss savant Marc-

[165] *Annalen der Physik* 6 (1800): 340-60, reprinting from *NJ* 4 (July, August 1800). It was this copy of Nicholson's Journal for July which was summarized in the letter Ritter received from Halle informing him he had been effectively scooped by Nicholson and Carlisle. It is difficult to date the appearance of issues of the German journals precisely, but this one (6. Band, 3. Stück) would probably have appeared around late September or early October 1800 (its contents were announced in the *Kaiserlich privilegirter Reichs-Anzeiger* newspaper for 7 October)

[166] Arrangements for importing British scientific journals into Germany remain obscure (the probable route being via Hamburg, at least before 1806). Nonetheless, it seems likely that a key figure was the Leipzig bookseller-publisher Johann Ambrosius Barth, who published Gren's *Journal der Physik* (predecessor of the *Annalen der Physik*), as well as the *Annalen* itself after 1809. Volta was able to order the first volume of *Nicholson's Journal* from Barth (his main German bookseller) in 1798, but decided not to purchase the *Journal* in 1800—even though he knew of Nicholson's work on the decomposition of water from the *Moniteur*—because, having to be imported from Britain, it was too expensive! He was, however, later able to get full information on experiments by Nicholson, Davy, and other British investigators of the battery via the reprints of *Nicholson's Journal* arriving in the *Annalen* (which Barth was now sending him). Volta to Barth, 10 April 1798, in *Epistolario di Alessandro Volta*, 3:376-78; Volta to Barth, 28 December 1800, in *Epistolario di Alessandro Volta*, 4:27-29; Barth to Volta, 7 July 1801, in *Epistolario di Alessandro Volta*, 4:47-48.

[167] *Annalen der Physik*, 7 (1801): 114-131; 8 (1801): 1-21, 300-315.

Auguste Pictet. Pictet had strong connections with the Institut but acted independently, taking pride in often being the first to bring new British work to Paris.[168] Supplies of British material were kept up all through the wars thanks to an intricately-managed and much-varied chain which began with a London bookseller, Joseph Deboffe, whose shop in Soho was only a few minutes' walk from both Joseph Banks's library and the house where Nicholson edited his journal. The system involved, at various different times, fellow Genevans living in London, a merchant at Calais, agents at Hamburg and Amsterdam, a London banker, a Dover innkeeper, and eventually the use of prisoner-of-war exchange ships with the support of the French foreign minister, Talleyrand. Reprinting experiments by Davy and several other contributors to *Nicholson's Journal*, the *Bibliothèque* noted that galvanism had "electrified natural philosophers all over Europe," even in England, "a country generally better characterized by its talent for useful improvement, than for an abundance of fundamental ideas."[169]

The *Bibliothèque Britannique*'s operation ensured the flow of intelligence between Britain and France (in one direction), but such arrangements did not necessarily exist across other cultural or political borders. In particular, galvanism was pursued very actively in Germany after the announcement of the battery, by Ritter and other members of a thriving community of contributors to the *Annalen der Physik*.[170] Many of the chemical investigations published in the *Annalen* might have been interesting to British researchers, but in fact rather little of this work seems to have made any impact outside Germany.[171] This is one manifestation of a general feature of international science around 1800: connections between Britain and Germany were

[168] The *Bibliothèque Britannique* has been comprehensively studied in the very useful David Bickerton, *Marc-Auguste and Charles Pictet: The Bibliothèque Britannique (1796-1815) and the Dissemination of British Literature and Science on the Continent* (Geneva: Slatkine Reprints, 1986); on the supply system see especially 332-69.
[169] *BB* 16 (January 1801): 23-59; quote on 23.
[170] In Berlin, one group formed a "Philomatic Society" on the French model for "mutual communication of their scientific investigations, especially concerning galvanism." *Annalen der Physik*, 8 (1801): 22.
[171] In particular, the experiments of Paul Louis Simon of Berlin on the decomposition of water apparently remained unknown outside Germany—with consequences for the controversy on muriatic acid detailed in Chapter 3.

much worse than connections between Britain and France.[172] Surprisingly, this was true even when Britain was at war with France but had a regular postal connection to Central Europe through the key North Sea port of Hamburg (only cut off in 1806 with Napoleon's Continental blockade). "It is quite melancholy to see the slowness of scientific communications between England and Germany," complained the British chemist Richard Chenevix in 1803. "We are at least a year in arrears one with the other, and many German works and authors are wholly unknown to us."[173]

Several explanations can be given. First, amidst the fragmented political and scientific geography of German-speaking Europe, there was no go-to center for London to keep in touch with; researchers and periodical publishing operations were scattered across dozens of cities and small towns, and nowhere came close to rivaling Paris and London as centers of scientific information. Even (perhaps especially) at the most difficult periods of the wars, British and French men of science put their efforts into keeping open links with each other, often leaving German investigators out of the picture.[174] Second, there were several active go-betweens between Britain and France (such as Charles Blagden in London and Pictet in Geneva), but they were lacking in the British-German case. Richard Chenevix might have been a good candidate (he made a tour through Saxony in 1803-04), but he in fact was openly hostile to German science, which he proclaimed ruined by *Naturphilosophie* and metaphysical speculation. "Ritter the galvanist is the only man of real talent I have met with," he grumbled, "and his head and

[172] The connection also appears to have been rather asymmetric: German journals reprinted translations of British material more than British journals reprinted the German journals.
[173] Chenevix to Banks, 25 September 1803, *Banks Corr.* 5:319.
[174] See Chapter 4. Links between France and Germany appear to have been a little better. Michael Friedländer (the Prussian physician based in Paris who announced Ritter's work to the Institut) and Christoph Pfaff (in Kiel) even jointly edited a short-lived periodical, whose purpose was to bring news of French science to German readers, the *Französische Annalen für die allgemeine Naturgeschichte* (1802-03).

morals are overturned by the new philosophy of Schelling."[175] Others in Britain shared his skepticism. Charles Blagden denounced an "unaccountable propensity to mysticism and extravagance of every kind" in German thinking; Davy praised Ritter's experimental technique, but bemoaned his adherence to "pseudo-platonism" rather than the solid British empiricism of Bacon and Locke.[176] Language was another obstacle: French was widely known in Britain, German much less so.[177] Wartime divisions were not the sole determiner of international scientific exchanges, proving less strong in this case than a combination of informational geography, cultural or philosophical barriers, language, and the existence of skilled go-betweens.

In any case, after the treaty of Lunéville was signed between France and Austria in February 1801, an end appeared in sight even to the long-running hostilities between Britain and France, now the only players in the war. On 1 October 1801 the two powers agreed preliminary peace terms. A few days before, Alessandro Volta had arrived in Paris, seeking patronage from the new ruler of northern Italy, and in November he read an account of his experiments to the Institut.[178] Napoleon was present, and, suitably impressed, he "proposed" that the Institut might honor Volta with a gold medal. Volta was the first foreign *savant* to have read a memoir there since the peace, and the award would demonstrate, he told them, "from the first moments of the general peace, the desire to gather the enlightened knowledge [les lumières] of all those who cultivate the sciences."[179] Similar connections between peace and progress were expressed across the Channel by William Nicholson, who celebrated the end of "a war unprecedented for its objects, its extent, and the activity with which it was carried on." It had been, he lamented, the

[175] Chenevix to Banks, 17 August 1804, *Banks Corr.* 5:368
[176] Blagden to Banks, 15 October 1804, *Banks Corr.* 5:384; RI Davy MS HD/3/B/4 (Lecture from 1810; RI Davy MS HD/1/A/2 shows Davy made similar statements in earlier lectures). There were of course many German *Naturforscher* who did not embrace *Naturphilosophie* or were vehemently critical of it, but the movement and the broader post-Kantian intellectual tradition did strongly color perceptions of German science in Britain.
[177] Davy for example was typical in reading French but not German.
[178] On Volta's visit to Paris see Pancaldi, *Volta*, 235-40.
[179] *PVSA* 2:425, 16 Brumaire an 10 (7 November 1801).

source of so much disruption to scientific communications. Now information could flow freely once more. "Men have again recollected that they are brothers," he rather optimistically declared, "and the permanent interests of humanity have once more resumed their place."[180]

When the final treaty for the Peace of Amiens was signed in March 1802, a crowd of British men and women rushed to the French capital, previously closed to them for nine years. One of the first to arrive was the Royal Society's leading Francophile, Sir Charles Blagden, who made sure to use the reopened postal service between London and Paris to exchange regular private bulletins of "philosophical news" (as he put it) with his friend Sir Joseph Banks in London.[181] On 12 April he was able to give an account of his presentation to First Consul Bonaparte. Blagden was impressed—"his apprehension is extremely quick, his judgement sound, & his capacity very great indeed"—and believed Napoleon committed to peace in Europe.[182] The next month he met Bonaparte again. "I have a project in which I wish England would concur with us," Napoleon told him. "I mean to propose a prize for discoveries in Galvanism: I am fond of Galvanism, and consider it as promising great things."[183]

A few weeks after his talk with Blagden, Napoleon wrote to his minister of the Interior announcing the terms of this *Prix du Galvanisme.*[184] In the spirit of the new European peace, it would be open to French *savants* and "foreigners of all nations." The intention was to

[180] "Philosophical Journal: New Series" (dated 1 Dec 1801), bound with *NJ* 5 (1802).
[181] Blagden to Banks, 1 April 1802, *Banks Corr.* 5:157.
[182] Blagden to Banks, 12 April 1802, *Banks Corr.* 5:160.
[183] Blagden to Banks, 8 May 1802, *Banks Corr.* 6:178-180. Soon afterwards, Blagden recommended that Banks arrange for Napoleon to be elected a Fellow of the Royal Society "in the most honourable manner that could be done, as it is evident that he prides himself on being a patron & protector of the sciences." (Blagden to Banks 25 May 1802, *Banks Corr.* 5:189). Banks does not appear to have been receptive to the suggestion! Blagden also proposed that Banks persuade George III to send Napoleon "a little paddock of Kanguroos" to adorn the grounds of the Palace of St. Cloud—another unrealized project for "the establishment of mutual good-will & confidence between the two nations."
[184] Napoleon to Chaptal, 26 Prairial an 10 (15 June 1802), copy forwarded to the Institut in *Pochette* for 4 Messidor an 10 (23 June 1802), Archives of Acadèmie des Sciences, Paris. A comprehensive account of the *Prix du Galvanisme* is Stéphanie Deprouw, "Un héritage des Bonaparte: le Prix du Galvanisme, 1802-1815, et le Prix Volta, 1852-1888" (Master's thesis, École Nationale des Chartes, 2008).

"encourage and to fix the attention of philosophers" on galvanism, which was, he had decided, "the road to great discoveries." An annual medal (value 3,000 francs) would be given each year for "the best experiment made on the Galvanic Fluid," and there would be a one-off grand prize of 60,000 francs (about £2,400, a large sum) for an experimenter who would "make an advance in electricity or Galvanism comparable to the achievements of Franklin or Volta in those sciences." Both were to be judged by a committee from the Institut. The news appeared in the *Moniteur* on 8 July and quickly spread through the European newspapers.[185] But it had probably been all but forgotten by British readers when in January 1808, at the height of the great blockade that cut off Britain from the rest of Europe, the *Moniteur* brought the news to London that the Institut had awarded the annual medal to Humphry Davy.

Conclusion: Fragments and Copies

The aim of this chapter has been to use the spread of Volta's battery to map out the media world of science around 1800, with a particular focus on print. I have suggested that, despite the extraordinary richness and breadth of work in the history of the book in the last few decades, our knowledge of the ways pieces of print mediated information exchanges in science is still very incomplete. When we let go of modern ideas of how publication spreads new science and seek out what kinds of texts actually counted in moving information about the battery, a picture emerges of various print objects embedded within a hybrid media world. In this world, even the best-connected people necessarily worked with a fragmentary and partial picture of the rest of the scientific enterprise; everyone wanted good information about others were doing, but getting it was hard and depended on the actions of people far away. Contingent and often unpredictable flows of information actively shaped (but did not determine) what the battery would be for

[185] *Moniteur*, 19 Messidor an 10 (8 July 1802), p. 1223. Appearing in Britain for example in *Observer*, 25 July 1802.

different people at different times. Men of science interacted with such information using newspapers, issues of journals and other periodicals, and letters—and of course through unwritten channels such as formal society announcements, lectures, and conversations. Print was absolutely central to this fast-moving culture of scientific information—yet formal, controlled publication of key original texts by particular authors is only one piece (and in some cases a small piece) of the picture. Books and other important original works could and did still have a major impact on elite science around 1800. But they were created using longstanding practices of individual authorship, largely insulated from the day-to-day flows of information in the messy periodical commons. In contrast, many texts that counted in exchanges of timely information and news were derivatives: reprints from journals, from newspapers, printed versions of letters already copied elsewhere in manuscript, secondhand reports of someone else's work. Fragments and copies were how most people in most places got their information about distant things.

Around the end of the eighteenth century, techniques and norms of scientific communication were changing in ways bound up with shifts in the broader culture of information. Many aspects of the picture I have sketched here were rather new and still developing—from the upstart specialist monthly journals like *Nicholson's* and the *Annalen der Physik*, to the unpredictable but growing interest of the newspapers in some areas of science. The ebbs and flows of the Revolutionary and Napoleonic wars, and their variable impact on communications of all kinds, added a further element of uncertainty. Men of science took a variety of different stances in relation to this changing world and were far from agreed on how to orient their existing institutions and practices within it. Going into the nineteenth century, the proper relationship of scientific practitioners to information was—and for a long time would remain—unclear.

Chapter 2: Galvanism in the Newspapers

Introduction: Preparations for Deep Salvage

Why would newspapers print scientific news at the beginning of the nineteenth century? When they did, what was its form and its function in their pages, and the effect on science's public image? What kinds of relationship did members of the European scientific community have to the press, and what happened when the aims of newspapers cut across the authority of scientific practitioners? I take two observations and one principle as a guide when addressing these questions. The first observation: throughout the nineteenth century newspapers were among the most widely-distributed media presenting information about science to the wider population. For the majority of readers, we might say, the papers largely defined the experience of consuming printed news about science. Any history of the place of science in wider society and culture is clearly going to be incomplete without an understanding of what the newspapers were doing. The second observation is a corrective to the first. We should not necessarily expect to find all that many scientific items in the newspapers, as a proportion of other news. In most cases, we will be diving for scientific artefacts at the bottom of a vast ocean of other ephemeral human culture.[1] We might even want to start from the assumption that there is no particular intrinsic reason why scientific matters should find their way into a newspaper at all; most science didn't

[1] Thomas De Quincey once characterized the newspaper press as a "sea" and a "vast abyss" which "has swallowed treasures without end, that no diving bell will bring up again." *Tait's Edinburgh Magazine*, 1 (1834): 594. The metaphor retains its force even though De Quincey was reckoning without the high-tech salvage diving robotics of the full-text OCR search. I have repeatedly drawn on this powerful technology, but of course it has several limitations, the most obvious and insurmountable of which is that few newspapers from this period have in fact been digitized. For London, we have only four out of the over forty titles available to contemporaries during the decade of the 1800s: three major morning dailies (*Times*, *Morning Chronicle*, *Morning Post*), and one Sunday paper (*Observer*). For a full picture—and a better sense of the overall context of particular news items—these papers must therefore be supplemented with some old-fashioned "diving-bell" work turning pages or spinning microfilm in a newspaper archive. Ultimately the haul of treasures gleaned this way can never be as abundant as that brought up with digital searches, but I have done my best to ensure that what I say is not biased by the arbitrariness of digitization. Some Napoleonic French newspapers have been (patchily and poorly) digitized by Google Books (and digital copies of the *Moniteur* are available from the Bayerische Staatsbibliothek, http://www.bsb-muenchen.de/), but obtaining good searchable OCR versions is difficult. Often we have no choice but to enter the diving bell, descend, and hope for the best.

make the newspapers (and still doesn't), and everything that did will be by definition an unusual case. And the principle is this: when we do turn up pieces of news that refer to science, we should look first to the agency of the *newspapers* to understand them, and look to the scientific community second. Rarely do newspapers and science speak with one voice, but the newspapers operate the presses. It is an important historical problem to work out the degree to which the aims of newspapers and scientific practitioners did or did not converge, the reasons for this, and what the consequences were for the flow of information about science, and for science's public image.

This chapter investigates what newspapers were doing with science in Britain and France during the Napoleonic period. We still know astonishingly little about the relationship between science and the newspaper press before the twentieth century.[2] We need to reverse-engineer a complex engine without blueprints, and we should be wary of assuming too much about what is going on inside. Neither our experience of modern science journalism nor indeed of modern newspapers will be much use as guides. Following our principle to look to the newspapers first, we see the heart of the engine is the print shop floor, its pistons the hand-presses moving up and down through the night, stamping out tomorrow's news. Who controls what happens there? How did science look from the editorial office? Usually scientific practitioners have not been in a position to exert very much control over how the press deals with the sciences. The newspapers act as an independent force, able to make news from science without needing to offer very much

[2] There is a growing body of work on science in British nineteenth-century magazines, especially in the wake of the ground-breaking Science in the Nineteenth-Century Periodical Project: see for example Geoffrey Cantor et al., *Science in the Nineteenth-Century Periodical: Reading the Magazine of Nature* (Cambridge: Cambridge University Press, 2004). But the monthly and weekly magazines and other periodicals studied in work of this kind are very different kinds of publications from daily or weekly newspapers in terms of content, style, length, readership, and overall aims.

accountability in return. This was certainly the case in Britain during our period. But things were rather different in Napoleonic France, and this is why it will pay to study both together.[3]

In Britain the amount of science in the press during this period was indeed rather small. It occupied few column inches in comparison to other human activities of interest such as domestic politics, war, diplomacy, crime and punishment, high Society, and sports.[4] Yet for all that, the natural sciences were still much more regular sources for news in the papers than other areas of elite intellectual endeavor, from moral philosophy and theology to classical scholarship, history, or mathematics. Their presence was not negligible, and I show in this chapter how science's visibility and intellectual authority in the press increased during this period in both Britain and (for quite different reasons) in France. This happened in tandem with new kinds of engagement with the press on the part of men of science themselves, not always with entirely successful results from their point of view.

Galvanism and the science of electrochemistry which grew out of it both had a disproportionate conspicuousness in the newspapers. They were at the center of a perfect storm of novelty that made them attractive to the press. Galvanism furnished a large number of new discoveries, often visually spectacular and sometimes lurid or unsettling, and succeeding each

[3] In studies of public or popular science in the eighteenth and nineteenth centuries, newspapers have usually been passed over completely or at best used only as sources from which to cherry-pick isolated pieces of evidence, rather than being historicized on their own terms. Why? One reason is De Quincey's "diving bell" problem. Searching for rather rare pieces of information on science in the unindexed sea of the press is laborious. Moreover, when the artefacts are brought up, they are hard to use effectively because we still lack good interpretive tools for dealing with science in the daily press (it is simple to recognize that it is problematic to read the press as a straightforward barometer of "public opinion" on scientific subjects, but going beyond that observation is less easy). A second possible reason, more specific to the earlier period covered here, is that historians of science tend to like authors, and newspapers have no authors—in the sense that a piece of writing in a newspaper (unlike a book, or even an anonymous periodical review) is often intrinsically impossible to attach to a particular person. Finally, perhaps newspapers have simply not been amenable sources for the kinds of questions historians have framed about the interrelationships between science and society—which have traditionally been focused on interrogating the social dimensions of *elite* scientific knowledge. Scientific items in the newspapers, representing neither genuine popular agency from below nor the "popularizing" efforts of scientific experts from above, could all too easily be written off as epistemically-irrelevant cultural detritus.

[4] Perhaps more words were devoted to the "science of pugilism" (bare-knuckle boxing) than to all the natural sciences combined in the British press at this time.

other over a relatively short time. These were frequently entangled with other newsworthy things, from the patronage of fashionable Society figures to the imposing personage of Napoleon and his special project of the Prix du Galvanisme. I begin with a tour through one of the most intense and lively bursts of scientific news in Britain at this time, triggered by the visit to London—during the 1802-03 Peace of Amiens—of the Italian impresario of "animal electricity," Galvani's nephew Giovanni Aldini. With his dramatic experiments showing the strange effects of galvanism on the bodies of animals, and even more notoriously, an executed criminal, Aldini found himself one of the principal "curiosities" of London's winter social Season. The press latched onto him enthusiastically, and his visit generated a craze for galvanism in Britain. Descending into the metropolitan news cycle, I survey what the papers did with Aldini, arguing that at this time there existed neither a well-defined way for the press to report on scientific subjects nor a clear motive for them to do so, and consequently the newspaper sensation surrounding Aldini was fuelled by his entanglement with two much more established and regular subjects of the British press: fashionable Society, and crime and death. None of this endeared Aldini to British gentlemen of science, and he failed in his aim of convincing them that his uncle's "animal electricity" was still a viable hypothesis. Nonetheless, there was very little the Royal Society elite could do about the widespread enthusiasm for Aldini's brand of galvanism. In Britain the influence and role of the sciences in public life was still uncertain, while the newspaper press was independent, assertive, and imbued with a vigorous disrespect for all kinds of authority. Moreover, news journalism was also highly disreputable, and British gentlemen of science could not enter that field against Aldini without getting tarnished themselves.

I then move across the Channel to Napoleonic France. Here, independent journalism was weak and the news tightly controlled, while the cultural prestige of the natural sciences, strongly

endorsed and promoted by the regime, was soaring to unprecedented heights. Looking at French opposition to Aldini, I explore how elite French *savants* were granted an extraordinary level of control over scientific information in the press via Napoleon's official newspaper, the *Moniteur*, many of whose articles they wrote themselves. The state was a mutual beneficiary of this arrangement, which provided a wellspring of ready-made and politically-safe news whose main themes were steady rational progress and national scientific glory. The situation in France provides an excellent point against which to calibrate our picture of science in the British press, but understanding it does much more than furnish comparison: it will turn out to be essential for a proper understanding of the international circulation of scientific information.

Returning to London, I look at how the British newspapers began to manifest new attitudes towards science at this time due to the innovative public role for chemistry forged by Humphry Davy in his lectures at the Royal Institution (RI). In the wake of his major discoveries after 1807, Davy's lectures operated as a public forum where he dispensed new information straight out of his own scientific research in electrochemistry, a field he invented to replace galvanism, now tainted by association with Aldini's controversial experiments. Focusing on the detailed reports of Davy's lectures that began to appear in the press during his later years at the RI (when newspapermen were sent to take down notes in the audience), I argue that by regularly announcing novel information from his own research in Britain's premier site of public instruction, Davy unexpectedly hit on a formula that made him amenable to the newspapers in a way that also broadly served his own purposes. Sections of the press became allies in his project to imbue chemistry with a "philosophical" dignity and an active public, even patriotic, role in British society. Yet Davy himself was deeply unsatisfied with the press coverage of his lectures, worrying about the inaccuracy of reports over which he could exercise no direct control. And

when he did things of which the newspapers did not approve, they treated him mercilessly. His experience would stand as a warning to his scientific successors that the press was a powerful but ultimately unreliable ally.

Unfortunately it remains difficult to situate the Napoleonic period in relation to longer-term trends in the relationship between science and the newspapers, simply because on either side of the terrain I sketch out here our historical map of that relationship is virtually blank.[5] And while this chapter aims to furnish a component of that larger missing history—which I believe is essential to any integrated account of science in society over the long nineteenth century—within this dissertation I am more concerned with broad synchronic connections. The newspapers were the supreme information technologies of this period, and tracking the movement of information *within* the scientific world around 1800 will make more sense when we understand what newspapers were doing, and why. (This goes especially for our understanding of the monthly scientific journals, which, as we saw in the last chapter, were close kin to the newspaper press.) Many of the ways science found its way into the papers during this period now look unusual, sometimes bizarre. But we should at least begin by assuming that the newspapers knew the business they were engaged in, and work from there to triangulate their activities against the different ways the scientific community handled its own more specialist varieties of news.

[5] A lone exception is Jeffrey Wigelsworth, *Selling Science in the Age of Newton* (Farnham: Ashgate, 2010), which however focusses only on newspaper advertisements and so has little to say about science-as-news. James Secord, "Extraordinary Experiment: Electricity and the Creation of Life in Victorian England," in *The Uses of Experiment: Studies in the Natural Sciences*, ed. David Gooding, Trevor Pinch and Simon Schaffer (Cambridge: Cambridge University Press, 1989), 337-83 also in passing draws attention to the role of newspapers in shaping the controversy over Andrew Crosse's electrical production of life in the 1830s.

The Frog, the Ox, and the Hanged Man

Early in March 1803 the *Times* newspaper presented its readers with a curious spoof of recent diplomatic and scientific news. A remarkable "Galvanic operation" had just been performed at a dinner held at Naples for the French and the British ambassadors to the Neapolitan court. "Without the least sensible violence, without the smallest dispute for precedence, or violation of etiquette," the two ambassadors had been compelled, by only "five moderate shocks" of galvanic electricity, to grasp and shake each other by the hand.[6] But the full "arcana and mystery of this celebrated experiment" was understood only by a certain "Professor," who, the *Times* was happy to announce, had just favored the newspaper with a description of how galvanism might be used for improvements "equally surprizing and beneficial." In a further coup for international diplomacy, First Consul Napoleon Bonaparte would be jolted into shaking hands with his celebrated Royal Navy antagonist, Sir Sidney Smith, as well as with British Secretary of War William Windham. The miserly rich would be electrified into philanthropy, and rude office clerks sparked until they delivered polite bows. Votes in Parliament ("Aye" or "No" as required) might be delivered by the galvanic stimulus of MPs, and even the current byzantine intricacies of party politics would be clarified by experimental means, with galvanism's potential for "loosening the tongue of a politician more than a hundred miles off, and compelling him, by the irresistible volubility of his electrified organ, to enounce whether he belongs to his own party or any other." Finally, the Professor would even "engage to perform the whole *mechanical* parts of Government by Galvanism, at considerably less than one-fourth of the present expense."

In this baroque piece of humor the *Times* was poking fun at a dramatic surge in scientific news in the London papers over the previous two months. The cause was Luigi Galvani's

[6] *Times,* 9 March 1803, p.3. The *Times*'s writer was apparently quite attached to the humorous possibilities of galvanic diplomacy, because this article in fact followed a shorter spoof on the Naples handshake and galvanism printed a few days before. *Times,* 4 March 1803, p. 3.

nephew, Giovanni Aldini (the *Times*'s "Professor" of galvanism), who used the opening of travel between Britain and the Continent afforded by the Peace of Amiens to exhibit his new experiments in London in the winter of 1802-03. Possibly no other single scientific event during the decade 1800-1810 left quite so broad a scattering of traces in the British newspaper press as Aldini's visit, and it provides an excellent record for an inquiry into how the London press operated in relation to galvanism and other sciences.[7]

Aldini's trip to Britain was part of a self-promotional campaign to present new arguments in a complex series of debates that many in the European scientific community already considered closed.[8] The disagreements had originated in Volta's responses to Galvani's *Commentarius,* the work which introduced the world to galvanic electricity in 1792. In the *Commentarius,* Galvani had used the dissected legs of frogs to stake a claim about the causes of muscular motion, pointing to a special kind of "animal" electricity existing within living bodies and perhaps with its source in the brain; muscular contractions were brought about by this animal

[7] My survey of the London press turned up well over fifty items (some reprints) related to Aldini, ranging from the *Times*'s extended spoof article down to three-line jokes.

[8] The debates of the 1790s were highly complex and multi-sided, though because of the effective reboot of galvanism achieved by Volta's battery in 1800, their precise details were of much less interest by 1803. In fact there was no single Galvani-Volta controversy over animal electricity, but a broad variety of different positions formed through responses to the *Commentarius* and to subsequent experiments, offered up by investigators in Italy, France, Germany, Scotland, England, and elsewhere; this is well-demonstrated in Naum Kipnis, "Luigi Galvani and the Debate on Animal Electricity, 1791-1800," *Annals of Science* 44 (1987): 107-42 and Walter Bernardi, "The Controversy on Animal Electricity in Eighteenth-Century Italy: Galvani, Volta and Others," in *Nuova Voltiana: Studies on Volta and His Times*, vol. 1, ed. Fabio Bevilacqua and Lucio Fregonese (Pavia: Università degli studi di Pavia, 2000), 101-114. Marcello Pera, *The Ambiguous Frog: The Galvani-Volta Controversy on Animal Electricity*, trans. Jonathan Mandelbaum (Princeton: Princeton University Press, 1992) offers a good introduction to the main arguments, but the philosophical orientation means that the debates can appear more clean-cut than they actually were; as a historical study it is now superseded by Marco Piccolino and Marco Bresadola, *Rane, torpedini e scintille. Galvani, Volta e l'elettricità animale* (Turin: Bollati Boringhieri, 2003), and their English version of this work, *Shocking Frogs: Galvani, Volta, and the Electric Origins of Neuroscience* (Oxford: Oxford University Press, 2013). See also John Heilbron, "The Contributions of Bologna to Galvanism," *Historical Studies in the Physical and Biological Sciences* 22 (1991): 57-85, and on the early British response to Galvani, L. S. Jacyna, "Galvanic Influences: Themes in the Early History of British Animal Electricity," in *Luigi Galvani International Workshop Proceedings*, ed. Marco Bresadola and Giuliano Pancaldi (Bologna: University of Bologna, 1999), 167-85. For two different perspectives on Aldini's London demonstrations, see Charlotte Sleigh, "Life, Death and Galvanism," *Studies in History and Philosophy of Science* 29C (1998): 219-48, and Iwan Rhys Morus, "Radicals, Romantics and Electrical Showmen: Placing Galvanism at the End of the English Enlightenment," *Notes and Records of the Royal Society* 63 (2009): 263-75.

electricity, or "galvanic fluid," flowing in an electric discharge from the nerves to the muscles. Volta countered this in a series of publications over the next few years, locating the source of the electric currents not in the living body but in the contacts formed by the metallic conducting arcs Galvani had used in his experiments. He articulated a "contact theory" of galvanic action in which conductors were divided into two classes: the first class were metals, the second were "humid" conductors (encompassing animal matter as well as various liquids). A current could be produced by different combinations of the classes of conductors; Volta's battery was conceived as the definitive experimental embodiment of this idea.

By 1802, there was a growing consensus, led by the French Institut National, that Volta's experiments had killed the hypothesis of animal electricity (although opinions differed on how far Volta's own contact theory actually explained the action of the battery).[9] Aldini aimed to challenge this view of things. His argument was built around the introduction of some new and rather gruesome experiments with warm-blooded animals to the repertoire of galvanism. To make evident the flow of animal electricity, Aldini brought forward a new experiment, a circuit consisting of only organic components: a pair of frog's legs, an ox's head, and a living person, himself.[10] On the completion of the circuit, the frog's legs contracted, indicating an electrical current. This was Aldini's best argument in favor of the existence of animal electricity, but he had several supporting experiments that made for much more dramatic viewing, in which the effect of galvanism on living matter was shown by convulsing animal bodies with strong currents from the battery.

[9] In particular, British investigators like Davy tended to prefer chemical explanations of the battery's operation.
[10] The circuit was created by the experimenter (insulated from the ground) inserting one hand into the ox's ear (or, in a variation, touching the ox's spinal marrow), and with the other hand holding the frog by the end of the legs. To complete the circuit, the spinal marrow of the frog was lowered so as to come in contact with the ox's tongue. On contact, "strong convulsions" were observed in the frog's legs, indicating, Aldini believed, the flow of animal electricity. John [Giovanni] Aldini, *An Account of the Late Improvements in Galvanism* (London: Cuthell and Martin, 1803), 3.

Aldini visited Paris in the autumn of 1802, where the Institut proved totally unreceptive to his arguments that the debate over his uncle's ideas should be reopened.[11] He retorted by helping to organize the Société Galvanique in Paris to promote research into the animal electricity aspects of galvanism.[12] Aldini then crossed the English Channel to try his luck in England, sent on his way by a grand banquet in his honor from the galvanists of the new Société.[13] Arriving in London around the beginning of November, he first tried to secure support from the obvious source: Sir Joseph Banks. After Aldini turned up at his house in Soho square with letters of introduction from several Italian and French savants, Banks wrote to Charles Blagden, then in Paris, for information on the Italian's time in the French capital.[14] The report was not promising. "Aldini gave great offence," Blagden wrote, "by egregious puffing & the other arts of Charlatans."[15] Although venturing that the Institut had perhaps been "rather too hard" on the Italian, he advised Banks to maintain a skeptical attitude. Nonetheless, an account of Aldini's experiments was still read at the Royal Society, where it apparently received a rather muted reception.[16] A large-scale public demonstration was next on Aldini's agenda, and Banks

[11] In a critical report read at the Institut on 13 October 1802, Biot and Hallé maintained that all the "facts" were explained by a version of Volta's contact theory. *PVSA* 2:579-581, 21 Vendémiaire an 11 (13 October 1802).
[12] *Moniteur*, 14 Vendémiaire an 11 (6 October 1802) p. 49. Napoleon's Prix du Galvanisme was a strong spur to the Société's establishment, and Aldini himself clearly cherished hopes of winning the prize. See Christine Blondel, "Animal Electricity in Paris: From Initial Support, to Its Discredit and Eventual Rehabilitation," in *Luigi Galvani International Workshop Proceedings*, ed. Marco Bresadola and Giuliano Pancaldi (Bologna: University of Bologna, 1999), 167-85; the most through account of the *Prix du Galvanisme* is Stéphanie Deprouw, "Un héritage des Bonaparte: le Prix du Galvanisme, 1802-1815, et le Prix Volta, 1852-1888. L'etat et l'encouragement à la recherche sur l'électricité" (Master's thesis, École Nationale des Chartes, 2008).
[13] The banquet climaxed with toasts to the scientific advantages of the Peace, to Bonaparte's encouragement of galvanism, and "to the reunion of all men of science occupied with electricity, whether metallic, or animal." *Moniteur*, 1 Brumaire an 11 (October 23 1802), p. 128.
[14] Aldini arrived on 4 November bearing letters of introduction from Fabbroni, Lacépède, and also the veterinarian Jean Baptiste Huzard. BL Add. MS 8099 fols. 387, 270, 257.
[15] Blagden to Banks, 12 November 1802, *Banks Corr.*, 5:235.
[16] RS JBO 37:593-601, 25 November 1802. Aldini's paper was never printed in the *Philosophical Transactions*. The minutes of the Royal Society Committee of Papers lists it somewhat mysteriously as having been "withdrawn": RS CMB/90b, 23 December 1802. Nonetheless, Aldini was not completely cold-shouldered: he did receive the customary invitation issued to visiting foreign men of science to dine with the select "Royal Society Club" at the Crown and Anchor tavern on the Strand. "Attendance/account books," RS RSC, vol. 11, 9 December 1802. He also made a short trip to Oxford earlier in November: *NJ* 3 (December 1802): 299.

grudgingly agreed to forward a written request to the Royal Institution, the Italian's preferred venue.[17] The RI Managers responded by informing Aldini that "as the Public of England are always much disgusted with experiments of cruelty," they would on no account allow him to demonstrate in their lecture theatre. They further advised Aldini that he ought "to perform his experiments in future in as private a manner as he conveniently can."[18]

Aldini did exactly the opposite. He spared no effort in seeking out opportunities for public demonstration, and found them forthcoming from London's medical community.[19] The results can be seen in one of the first items on Aldini to appear in the London press, which describes his demonstrations at the Great Windmill Street anatomy theatre in Soho.[20] Headed "Galvanic Miracles," the paragraph appeared in the *Morning Post* on 6 January 1803, and was quickly reprinted—in the ubiquitous process we examined in the last chapter—in a clutch of other London papers, and even at Paris in the *Moniteur* (which remarked how in London "the talk is only of the effects of Galvanism").[21] It described how Aldini had shown "some curious experiments on the body of a dog newly killed, by which the company then present were exceedingly astonished by the powers of Galvanism." When Aldini applied the galvanic battery:

[17] Aldini to Banks, 29 November 1802, BL. Add. MS 8099 fol. 135. Banks had initially refused to help, saying that because of his gout it would be "impossible" for him to do anything on Aldini's behalf: Draft note Banks to Aldini, 29 November 1802, BL Add. MS 8099 fol. 135.
[18] RI, Managers' Minutes (facsimilie), vol. 3, p. 62 (minute for 6 December 1802).
[19] Aldini's main supporter seems to have been Dr. George Pearson, a physician, chemist, and promoter of smallpox vaccination.
[20] The anatomy theatre had once been part of the home of William Hunter but was then run by his successor, the surgeon James Wilson.
[21] *Moniteur*, 23 Nivôse an 11 (January 13 1803), p. 453. In London the evening newspaper the *Sun* reprinted the paragraph the same day, while the morning newspapers the *Morning Chronicle*, *True Briton* and the *Morning Herald* inserted it the next morning. It was also taken up by the Sunday newspaper the *Observer*. *Sun*, 6 January 1803, *Morning Chronicle*, 7 January 1803, *True Briton*, 7 January 1803, *Morning Herald*, 7 January 1803, *Observer*, 9 January 1803.

> "both head and body were thrown into the most animated muscular motions. The body started up with a movement by which it passed over the side of the table. The head equally moved; its lips and teeth grinning violently."[22]

Most of Aldini's London demonstrations are only recoverable from newspaper paragraphs like this one. Here, however, we can triangulate the paragraph against something closer to "what really happened" using a set of manuscript notes taken by an anonymous medical student in the audience.[23] These show that the gruesome galvanized dog was part of a longer program of experiments Aldini performed in Great Windmill Street before an (all male) audience composed of medical professionals, students, and several interested gentlemen, including the French Ambassador General Andréossy, accompanied by the Chargé d'Affaires of the Ottoman Empire and the British diplomat and antiquarian Sir William Hamilton.[24] The newspaper chose to report only the most arresting and graphic of Aldini's numerous experiments on galvanism, focusing on the disturbing and compelling novelty of galvanic electricity producing the semblance of life in death.[25] The paper did not trouble to say anything about Aldini's purpose in doing the experiment, or his overall claims about the actual existence of animal electricity. It was interested in the sensational and wonderful scientific anecdote.

That Aldini's experiments were simplified and sensationalized in the press will probably come as no surprise. We know that newspapers sensationalize things. But in fact we are already

[22] *Morning Post*, 6 January 1803. The paragraph then commented how "a curiosity has been expressed to have these experiments tried on a criminal newly executed"—a curiosity soon fulfilled, as we shall see.
[23] Royal College of Surgeons Archives, MS 0262. The notes are apparently a fair copy of an earlier rough draft written on the scene.
[24] Additional details of who was present given in the *Star* newspaper, 23 December 1802, p.2.
[25] One might expect that some observers would read profound implications into experiments like these. If the movement of bodies could be excited artificially by galvanism at the whim of the experimenter, and if galvanic electricity was the true "vital fluid" flowing through the nerves, then to what degree was part or all of the living organism reducible merely to the action of chemical and physical laws? Was galvanism the bridge between living matter and the will? What implications were there for the incorporeal existence of the mind and soul? Did this point towards materialism? No doubt many found Aldini's experiments unsettling, but there seems to have been basically no serious public reflection on anything like these questions—even after the news of Aldini's similar trials on human corpses. Possibly the jokey vein of many of the newspaper reports helped to defuse any anxiety by channeling feelings of uncanniness into a humorous outlet.

beginning to tread on dangerous territory, because newspapers around 1800 were in many ways very different from their modern descendants, and we shall see that an assumption of even this innocuous kind would be very wide of the mark for newspapers in Napoleonic France. Before going a little deeper we therefore need to step outside the paragraph text and look at how British newspapers actually operated at the beginning of the nineteenth century.

In 1803 London had a good claim to be the news capital of the world. It could boast nine daily morning papers and seven evening dailies, besides ten papers printed two or three times a week and some twenty weeklies, mostly published on Sundays.[26] One contemporary guidebook, the *Picture of London*, estimated that the metropolis's presses stamped out 232,000 newspapers per week.[27] In the end, all newspapers were businesses, and the press was a thriving commercial system, the product of over a century of competitive innovation in a very active marketplace for news. Polite opinion, however, generally considered newspapers disreputable, and news journalism occupied a very bad position at the bottom of the already dubious trade of writing for money. In Henry Brougham's phrase, it was "dirty work," no kind of pursuit for a gentleman.[28]

[26] There is a large literature on the British newspaper press in this period; broad surveys include Jeremy Black, *The English Press 1621-1861* (Thrupp, Stroud, Gloucestershire: Sutton, 2001), Hannah Barker, *Newspapers, Politics and English Society, 1695-1855* (London: Longman, 2000). Recently, Uriel Heyd, *Reading Newspapers: Press and Public in Eighteenth-Century Britain and America* (Oxford: Voltaire Foundation, 2012) has valiantly tackled the immensely challenging question of reading experience and reception. Useful comparative detail is provided by Hannah Barker and Simon Burrows, eds., *Press, Politics and the Public Sphere in Europe and North America*, 1760-1820 (Cambridge: Cambridge University Press, 2002); see also Andrew Pettegree, *The Invention of News: How the World Came to Know About Itself* (New Haven: Yale University Press, 2014). More broadly on the history of news and newspapers in the late-eighteenth-century Atlantic World see William Slauter, "News and diplomacy in the age of the American Revolution" (PhD Dissertation, Princeton University, 2007).

[27] *The Picture of London for 1809* (London: Phillips, 1809), 327. Individual leading papers had daily sales of up to about 5,000 (the comparatively low figure was due to the inflated price created by the stamp duty); this was about the same as what the top monthly publications like the *Monthly Magazine* sold each month. The *Times*, during this period in the process of becoming the leading daily, sold 5,000 copies each day in 1815 (it would reach 40,000 by 1851). A true mass newspaper press (with daily circulations of some titles in the millions) did not arrive until the late nineteenth century.

[28] Arthur Aspinall, "The Social Status of Journalists at the Beginning of the Nineteenth Century," *Review of English Studies* 21 (1945): 1. Even into the 1830s newspapermen were frequently debarred from "good society" on account of their occupation: *Westminster Review* 2 (1836): 195.

"Nothing but a thorough-going blackguard," declared Sir Walter Scott, "ought to attempt the daily press."[29]

Yet at the beginning of the nineteenth century the newspapers were vigorously proud of their role in British public life. They saw themselves as guardians of constitutional rights and spirited defenders of "English liberty," and indeed in 1803 no other major country's papers were quite so free to do as they liked except those of the United States.[30] Masters of scurrilous takedowns and indignant invective, they considered themselves answerable to no one and entitled to dish out judgement on everyone. As several men of science discovered, exposure to their acerbic barbs was the price of being a public figure. Britons frequently complained about their ferocious newspapers, but they were also generally quite proud of their free press system—except when on the wrong end of its irreverent humor. "What a wonderful idea is afforded," exclaimed the *Picture of London*, "of the agency and influence of the press in this Empire, and how easily is it accounted for that we are the most free, and the most intelligent people, on the face of the earth!"[31]

Individual issues were expensive, crowded, and short.[32] They consisted of a single sheet folded to make four large pages, the first and sometimes the last of which were filled with

[29] *The Journal of Sir Walter Scott, 1825-32* (Edinburgh: David Douglas, 1891), 675-676. Entry for 3 April 1829.
[30] Their greatest eighteenth-century victory had been to wrest the right to report legislative debates from a reluctant Parliament, which they obtained in the early 1770s: Black, *English Press*, 129-32. Newspapers have naturally perpetuated a strongly "Whiggish" teleology of their own history as a march towards ever greater liberty of the press and the newspaper's gradual assumption of its "true" role in civil society, reinforced in the first comprehensive history of the British newspapers, F. Knight Hunt's *The Fourth Estate* (1850). This history tended to emphasize instances of conflict and struggle against repressive forces, such as in this period the prosecution of editors for libel in the 1800s and 1810s, and the newspapers' engagement with progressive politics, the classic survey being Arthur Aspinall, *Politics and the press, c. 1780-1850* (London: Home & Van Thal, 1949). In fact, the British newspapers around 1800 enjoyed exceptional freedom and influence in comparison with those in other countries, despite limitations like libel prosecutions and the stamp tax. Moreover, recent historical work has emphasized the narrowness of giving primacy to political writing in the press at the expense of seeing newspapers as a product including a wide variety of content; see Black, *English Press*, 59.
[31] *The Picture of London for 1809*, 327.
[32] Before the *Times* adopted a steam printing machine in 1814 all newspapers were printed by hand, on presses which were only slightly-improved versions of the machine invented by Johannes Gutenberg three and a half

advertising.[33] With only two or three pages in which to print the news, the papers generally used tiny margins and small type to pack information onto the page. The price was kept high by the stamp duty, a specific tax on news and the most infamous of the government's "taxes on knowledge."[34] In 1803 an issue generally cost 6 pence—about the price of a very solid dinner of roast beef in a cheap London eating house.[35] This ensured that newspapers could not be regularly purchased by members of the working classes, which was exactly the government's intention. Nonetheless, copies of a newspaper often had multiple readers, with those available for browsing in taverns, pubs, coffeehouses and reading rooms perused by many people who could not afford to buy their own.[36]

In a sense, the stamp duty *defined* what counted as a newspaper in Britain: legally, a newspaper was any publication appearing more frequently than once a month that contained writing about current events. In contrast, the untaxed periodical press encompassed serials which did not offer fast and frequent access to news, like monthly magazines and review periodicals. This distinction was reflected in the ways the newspapers and the untaxed monthly periodicals treated science. Whereas "improving" periodicals like the *Monthly Magazine* would frequently offer readers material of a more generally interesting or educational nature, science almost always appeared in the British newspapers as present-centered information, closely tied to recent *events* rather than to a systematic body of knowledge that could be communicated to the reading public. With his paper taxed at 3½ pence per (folded) sheet, a British newspaper editor would not

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centuries before. At best, two skilled operators could produce no more than 250 printed sides an hour, which even with multiple presses in operation put severe limitations on print runs.

[33] A few weekly newspapers had more than four pages.

[34] The price of paper was also inflated by an additional tax. In 1803 the stamp duty was over half the cost of an issue: 3½d for a single-sheet (i.e. four-page) paper.

[35] *Epicure's Almanack; Or, Calendar of Good Living* (London: Longman, 1815), reissued as Janet Ing Freeman, ed., *The Epicure's Almanack: Eating and Drinking in Regency London* (London: British Library, 2013), 135.

[36] Obtaining a rough measure of the average number of readers per issue remains a difficult problem; contemporary guesses varied, with twenty readers being a popular eighteenth-century estimate for the London press. Jeremy Black, *The English Press in the Eighteenth Century* (London: Croom Helm, 1987), 104-106.

be inclined to use expensive column inches to print science for its own sake. Any scientific information that did get into a newspaper's pages would have to enter as news first and science second.

When you think about it, newspapers are an odd species of print object. They bundle together, for sale as a single unit, a wide range of disparate pieces of information whose only common feature is that they are all, in some ineffable ways, news.[37] And in the British press around 1800 this bundle was constructed without much of the organizational formatting that is now familiar to newspaper readers. A few items were given headlines or separated off from the rest, but usually individual items of news were not in distinct articles. Paragraphs on different subjects were simply spliced together, following on from one another in a way which now looks rather incongruous and makes for odd reading, but which for the original consumers was simply the normal way of experiencing printed news. There were no journalists' by-lines, no "stories" as we would understand them—everything was anonymous. In short, news was not authored. Not only was a large proportion of material lifted and reprinted from other newspapers and various additional print sources, including scientific journals, but the actual originality or lack thereof was often irrelevant. Unless they assiduously read a dozen papers every day, readers would often not have been able to tell what text really originated with the paper they were reading. Information actually original to a particular newspaper was drawn from various sources: police clerks for crime reports, servants and sedan chair carriers for gossip and the recent movements of Society figures, and a few paid reporters taking notes in Parliament.[38] There was no reporting

[37] Unless they are advertising, although this too can be conceptualized as a kind of news. On the "strangeness of periodical news" more broadly see C. John Sommerville, *The News Revolution in England: Cultural Dynamics of Daily Information* (Oxford: Oxford University Press, 1996).

[38] See James Savage, *An Account of the London Daily Newspapers, and the Manner in which they Are Conducted* (London, [1811]). In the case of the report of Aldini's experiments on the galvanized dog it is impossible to know who was the source of the report. It seems unlikely that it was a person on the staff of the newspaper, as the modern practice of reporters going out to look for stories in the field did not exist. We can also rule out Aldini personally

staff in the modern sense. The first items in the paper, after the advertisements, were usually on foreign events.[39] These were followed by the domestic (mostly London) news, in which longer reports on Parliament and on cases and trials in the law courts were surrounded by a mishmash of paragraphs—the atomic unit of news—on a range of subjects including the theatre, the opera, fatal accidents, duels, executions, royalty and scandal, the comings and goings of Society figures, the latest fashions, ships just arrived in port, and so on. When looking at what science is doing in the early-nineteenth-century papers, it can help to think of newspapers less as coherent and unified publications than as special print technologies for the repeated and regular packaging and sale of the latest haul of various new pieces of information.[40] In sum, they were packages of novelties. Some new information was obviously monumentally important, like Napoleon's defeat of the Austrian and Russian Emperors at the Battle of Austerlitz. Some was obviously trivial: that the Duchess of Devonshire had just held a supper party, or a recent violent altercation in a London street.[41] Most news fell somewhere between these extremes.

It seems reasonable to expect that for editors and most readers, almost all kinds of material that related to science would have registered towards the trivial and unimportant end of the spectrum.[42] Moreover, at this time the papers had no well-defined way of dealing with

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composing the paragraph, as he could not write or speak fluent English. The information probably reached the paper through one of Aldini's friends in the medical community, or from a medical student in the audience.

[39] At this time the foreign news section in British newspapers might include a "leading article," a longer editorial comment on the latest news and one of the few places where the paper actually deployed a distinctive voice.

[40] "New" ought to be understood in two senses here: recently-created, and/or unfamiliar to the reader. In a newspaper the packaging is done repeatedly, indefinitely, under the same title, and (until the Internet at least) at regular intervals; Sommerville, *News Revolution in England* argues that this periodicity is actually at the root of many characteristics of the news from the seventeenth into the twentieth century.

[41] *Morning Herald*, 11 January 1803, p. 2; "Dreadful Fracas," *Morning Post*, 7 January 1803, p. 3.

[42] This is still the case for much scientific news. There is a subtle point lurking here relating to the different scales of historical time at work in scientific development compared with the daily news cycle. Scientific discoveries and other innovations can and do effect enormous changes in the long term, but on the short-term scale of news reports they would frequently have been less important to a large number of newspaper readers than other pieces of more pressing daily information like a rise in the price of corn, the result of a cricket match, or the details of a minor naval skirmish. Innovations which historians of science have in hindsight understood as important may not have had the

scientific news. A paragraph like the one on Aldini's dog experiment certainly had an inherent novelty-value and lurid fascination that made editors want to print it, but it was something of a misfit in the larger population of news paragraphs. The subsequent explosion of galvanism in the press was driven more by cross-fertilization with other types of news more familiar in the British newspapers around 1800 and which they were more in the business of selling.[43] This cross-fertilization was not calculated to endow Aldini with the mantle of a sober scientific enquirer or advance his claims about animal electricity.

A staple feature of the press at this time was the "fashionable world," the small clique of individuals close to the apex of British society whose lives, entertainments, friendships, loves and scandals were the object of wide discussion and occasional censure among a large number of spectators, and of whom the supreme example around 1800 was perhaps still Georgiana, Duchess of Devonshire.[44] All London newspapers printed items on the "fashionables," and some made a regular practice of offering up short paragraphs of new intelligence (from social itineraries to scandalous gossip) under special headings such as "Fashionable World" or "Mirror of Fashion." This category of news was widely regarded as superficial and frivolous but it clearly sold well.[45]

Aldini arrived in London at the beginning of the "Season," the winter months when not only the "fashionables" but the upper classes in general descended on the capital and which determined the calendar for the opera, balls, theatricals, and even sittings of Parliament and

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same significance to contemporaries at the moment they were made public, before even the scientific practitioners involved knew how they would pan out.

[43] We might in part attribute this to the package model followed by the newspapers: the disparate mixed-bag character of news in a newspaper actually encouraged the formation of unusual connections between different circulating topics.

[44] On the tight links between fashionable Society, gossip, and satire, see Vic Gatrell, *City of Laughter: Sex and Satire in Eighteenth-Century London* (New York: Walker, 2007).

[45] For example, the serious-minded Savage, *Account of the London Daily Newspapers*, deplored the takeover of newspapers by "frivolous and abusive subjects."

meetings of the Royal Society. His way into the fashionable elite seems to have been though General Andréossy, the French ambassador who attended Aldini's early Great Windmill Street demonstration and who was an object of great celebrity in the newspapers.[46] On 4 February, at the home of his British supporter Dr George Pearson, Aldini gave a series of galvanic demonstrations for Andréossy and a constellation of aristocratic and political stars, among them cabinet members Lord Pelham (Home Secretary) and Lord Castlereagh, as well as Lord Hervey (the under-secretary of state for foreign affairs), the Duke of Roxburghe, various sons of peers, and "several other gentlemen of rank." Reports in the newspapers, after running through the important personages present, described how:

> "The head of an ox, recently decapitated, exhibited astonishing effects; for the tongue being drawn out by a hook fixed into it, on applying the exciters, in spite of the strength of the assistant, the tongue was retracted, so as to detach itself, by tearing itself from the hook; at the same time a loud noise issued from the mouth, by the absorption of air, attended by violent contortions of the whole head and eyes."[47]

After this stomach-churning exhibit, the *Star* newspaper reported, "the company partook of an elegant *dejune* and a *conversazione* in Dr. Pearson's house."[48]

A demonstration by Aldini was clearly one of the essential attractions of the social Season. With great interest generated by reports of his experiments made on a human corpse a few days before, Aldini even exhibited on 24 January before the supreme patron of fashionable entertainment, the Prince of Wales, who, together with his Royal brothers, also had the dubious pleasure of seeing an ox's head subjected to galvanic currents. "The whole of the experiments

[46] The "fashionable world" columns commented on everything from Andréossy's dinner arrangements to his clothing. See for example *Morning Post*, 21 January 1803, p. 2, which described a dinner just given by Andréossy to the "*Corps Diplomatique*," including cabinet members Hawkesbury, Pelham, and Castlereagh, consisting of "all the delicacies of the season, and wines of the first qualities."
[47] *Morning Post*, 14 February 1803, *Times*, 15 February 1803 and *British Press*, 15 February 1803, p. 4.
[48] *Star*, 12 February 1803, p. 3.

were performed with the greatest precision and effect," enthused the press, noting that the Princes, and the other "distinguished characters" present, had "testified their entire satisfaction."[49]

An inevitable consequence of getting mixed up in "fashionable" news was to be the subject of jokes. In fact, it is hard to say whether it was the jokes or the "fashionable" connection which came first for galvanism in 1803, since Aldini's experiments were already a prime field for humorous exploitation, taking place right on the boundary where the exhibition of the sublime powers of Nature came full circle and met the ridiculous and grotesque. Two days after the *Morning Post*'s report on Aldini's demonstrations, a paragraph of the same paper's "fashionable world" column proclaimed that:

> "The effect of Galvanism upon dead bodies, restoring them, as it were, to a state of active and vigorous exertion, has been hailed by several of our *antiquated beaus* with transports of joy."[50]

This mildly risqué galvanic joke was the first of a cascade of similar short, irreverent items related to Aldini that appeared in the press over the succeeding weeks.[51] Directly below it in the *Post*'s column, readers would have encountered the following paragraph:

> "Dr. Aldini, the celebrated Galvanic Operator, is said to have dined with the Duke of Queensberry the other day."

This pair of paragraphs twisted Aldini's galvanism together with another piece of news, then enjoying high public visibility. The Duke of Queensberry was a long-standing subject of fashionable gossip; in his younger days "the most brilliant, most fashionable, most dissipated

[49] *Morning Post*, 25 January 1803 and *Morning Herald*, 25 January 1803.
[50] *Morning Post*, 8 January 1803.
[51] It nestled amidst a collection of other short paragraphs concerning, for example, a "splendid ball" recently given at Paris, the imminent arrival of the Royal Family in Town, a joke about the endemic debts of "our Noble prodigals," a new opera at Covent Garden, and the expected marriage of the daughter of the Earl of Jersey to a son of the aristocratic Ponsonby family. Such material was the daily diet of late-Georgian newspaper readers.

young man in London, the leading character at Newmarket, the support of the gaming-table, the supreme dictator of the Opera-house, the pattern whose dress and equipage were to be copied by all who aimed at distinction."[52] One of aristocratic Society's most notorious womanizers; he was now elderly (at 77), but still famously lecherous.[53] Humorously equating electrical, vital, and sexual powers, the *Morning Post*'s paragraph combined the strange phenomena of galvanism with a reference to another preoccupation of the fashionable world that winter: the old Duke's apparently failing health.[54] Other papers took up the *Morning Post*'s joke and rolled it out further, and the newspapers' humor even inspired a caricature print featuring an imaginary scene in which Aldini galvanizes Queensberry to rejuvenate his vital powers.[55]

Many more jokes appeared in the newspapers over the succeeding weeks, so many that only a few can be offered here. While the *Morning Herald* added "our modern Country Dames" to the *Post*'s aged rakes, commenting how they "resemble the effects of Galvanism—animated nature, without impulsive reason," other papers tied galvanism to another perennial subject of the fashion columns: the theatre. "As *Galvanism* moves the *muscles* so effectually," frivolously declared the *Morning Post* at the end of January, "the managers of our Theatres intend trying it

[52] James Home, ed., *Lady Louisa Stuart: Selections from Her Manuscripts* (New York and London: Harper, 1899), 82.
[53] Queensberry was widely known as "Old Q," or in one somewhat less affectionate caricature, as "the Old Goat of Piccadilly." Gatrell, *City of Laughter*, 405-408; *DNB*.
[54] Regular reports on his Grace's latest rallies and relapses were scattered through the Season's newspapers, with one paper even falsely reporting his death; in the end he surprised the followers of fashion by living on for another seven years. Death report in *British Press*, 11 March 1803, p. 4; with a reversal worthy of a Monty Python sketch, the same paper soon announced him to be "somewhat better" on 13 March 1803, p. 4.
[55] On 26 January the theatre and fashion section of the *British Press* newspaper commented how "We lately observed that a veteran Duke, with whom Dr. Aldini is said to have dined, wanted no other apparatus [playing on 'galvanic apparatus'] than a pair of bright eyes to *animate* his Grace, and a late *amourous adventure* justifies our observation." The "amourous adventure" referred to was a recent escapade, also much-reported in the papers, in which the invalid Duke was said to have ungallantly carried off a young Italian maidservant in his carriage. *British Press*, 26 January 1803, p. 3. *Morning Herald* 18 January 1803, reports the escapade, obliquely referring to Queensberry as to "An aged Peer" whose "invincible gallantry has now, for half a century, supplied the beau monde with scandal, and the newspapers with paragraphs." The caricature print "Galvanism in Piccadilly" (which plays on this episode) can be viewed at http://catalogue.wellcomelibrary.org/record=b1159954.

on some of the actors and actresses who want *expression of countenance*."[56] Not to be beaten, two weeks later the *Times* announced a forthcoming play in which "all the sentiments are mechanical, the jokes mathematical," and whose audience would repeatedly "be convulsed into laughter, upon the proper occasions, by the Galvanic Electricity."[57] Although we can now only catch faint echoes, the vogue for galvanism and the jokes must have moved back and forth between newspaper paragraphs and drawing room conversation. "I should have liked being of William's party to Aldini," wrote Maria Stanley, Baroness of Alderley, in response to her sister's description of her husband's visit to one of the demonstrations; "I wish I knew how to make a battery and try some experiments on the ox's and sheep's heads, and I wish I could hear what the wise folk say about it."[58] The newspaper paragraphs detailing the actual demonstrations and listing the fashionable figures who had attended served to bring readers into the anatomy theatres to become virtual observers of the exciting and gruesome experiments, while the jokes formed a meta-level satirical gloss that connected Aldini to other contemporary news, allowing Georgian London to have yet another laugh at its own expense.

The short irreverent newspaper paragraphs we have just encountered were an important component of the packages of novelties many newspapers offered their readers. They were usually the compositions of penniless young men with literary aspirations, informally associated with the newspaper and paid for each item of news, humor, or scandal delivered to the editorial offices. One such "paragraph-monger," as he called himself, was the writer Charles Lamb, who worked for the *Morning Post* at this time and who years later recalled the horror of having to

[56] *Morning Herald*, 12 January 1803; *Morning Post*, 31 January 1803.
[57] *Times*, 16 February 1803.
[58] Maria Josepha Stanley to Louisa Clinton, 30 January 1803, in *The Early Married* Life *of Maria Josepha, Lady Stanley*, ed. Jane Adeane (London: Longman, 1899), 253. Aldini's experiments were done in the masculine space of the anatomy theatre and were clearly considered too "indelicate" for mixed audiences. Women could therefore only experience them at second hand, either by reading about them in the press or by hearing about them from men who had attended.

wake at five o'clock each morning to spend an hour and a half grinding out his "quantum of witty paragraphs."[59] For all we know, some of the newspapers' jokes on galvanism may be Lamb's productions.[60] Drawing on "the chat of the day" and items in previous days' papers, the paragraph-monger would condense the circulating London gossip into nuggets of novelty for insertion in the columns of the next morning's issue. The latest scientific news could furnish ready material for this task, not only Aldini's galvanism but also more unlikely topics such as the discovery of the asteroid Pallas, or the observation of a new comet in 1805.[61] One of the more common ways for science to appear in the British newspaper press at this time was as part of a joke.

But the most widely-read of all the items of news related to Aldini was connected with a different news staple of the British press: crime and death. Let us imagine ourselves in the place of a reader browsing the *Morning Herald* newspaper on Friday 21 January, 1803. Opening the paper, he or she would have encountered a thrity-five-line paragraph prominently set off and headed with the now familiar word: "GALVANISM." The paper recounted how the body of a criminal, hanged a few days before, had after execution been "conveyed to a house not far distant, where it was subjected to the *Galvanic* Process, by Professor Aldini," under the eyes of small gathering of medical men.[62] The consequences had been impressive, showing "the eminent

[59] Charles Lamb, "Newspapers Thirty-Five Years Ago," reprinted in *Last Essays of Elia*, vol. 3 of *The Prose Works of Charles Lamb* (London: Moxton, 1836), 155-61.
[60] Lamb is on record as having made humorous written allusions to galvanism in 1803, referring to "this Galvanic age" in a letter to a friend: Charles Lamb to John Rickman, 16 July 1803, in *Lamb's Friend the Census-taker: Life and Letters of John Rickman*, ed. Orlo Williams (Boston: Houghton Miffin, 1912), 90.
[61] *Morning Chronicle*, 6 August 1801; *Morning Post*, 11 December 1805.
[62] *Morning Herald*, 21 January 1803. The context here was very different from Aldini's other demonstrations; fashionable Society was not in attendance, and the direct audience was a small private group of mostly medical professionals, all with strong stomachs and accustomed to witnessing human anatomical dissection. Obviously, it would have been in extremely poor taste to make this anything other than a strictly medical event, carried out from pure motives for the benefit of humanity—in this case in the hope of discovering electrical techniques for reviving people who had been drowned, suffocated, or fallen into "suspended animation."

and superior powers of Galvanism to be far beyond any other stimulant in nature." Perhaps with a growing chill slowly running down the spine, our reader would have learned how:

> "On the first application of the process to the face, the jaw of the deceased criminal began to quiver, and the adjoining muscles were horribly contorted, and one eye was actually opened. In the subsequent part of the process, the right hand was raised and clenched, and the legs and thighs were set in motion. It appeared to the uninformed part of the by-standers as if the wretched man was on the eve of being restored to life."

In fact, the newspaper hastened to add, a ghastly resurrection would not have been possible, as several of the man's friends had pulled on his legs as he hung on the scaffold, "in order to put a more speedy termination to his sufferings." The chilling paragraph proved irresistibly contagious, seized and reprinted by editors of many other publications; it is perhaps not a bad candidate for the decade's most widely-copied and widely-read piece of scientific news.[63]

The macabre experiments first described in the *Morning Herald*'s paragraph quickly became some of the most famous associated with galvanism. They form a well-known episode in the history of science, but what is less commonly realized is that for the original London newspaper readership they made a grisly and unexpected coda to a news narrative running in the press over the previous weeks. Crime and death were staple subjects of the newspapers: long reports of trials at sittings of the Old Bailey criminal court were supplemented by paragraphs on

[63] To use a suggestive anachronism, the paragraph went viral. A newspaper editor, seeing it in another newspaper, would almost invariably decide to reprint it in the next issue of his own publication. Immediately after its debut in the *Morning Herald*, it appeared in almost all the major London papers: the morning dailies the *Times*, *Morning Post*, *Morning Chronicle*, *True Briton* (which all printed it the next day), numerous evening papers like the *Star*, the *Courier* and the *Sun*, and Sunday papers such as the *Observer* and *Bell's Weekly Messenger*. Harvested from the London papers, it appeared in the provincial press up and down Britain: in Exeter on 27 January (*Trewman's Exeter Flying Post*), in Newcastle on 29 January (*Newcastle Courant*), and in Aberdeen on 2 February (*Aberdeen Journal*). Across the Atlantic, readers in Boston (*Boston Gazette*, 14 March) and New York (*New York Gazette*, 18 March) could feel the chill of being confronted over breakfast with image of the quivering of the corpse's jaw, the opening of the lifeless eye, and the clenched fist of the dead hand as soon as the London papers arrived and their news was reprinted in the United States. Beyond the newspapers, the paragraph variously turned up in whole or in part in (for example) the *Monthly Magazine*, the *European Magazine*, the *Christian Observer* magazine, and even the news sections of the medical men's *London Medical and Physical Journal* and the scientific journal the *Philosophical Magazine*.

discovered bodies, riots, highway robberies, "dreadful accidents," and suicides. The story here was a sensational double murder committed by George Foster, a journeyman coachmaker whose short public fame began with the discovery of two drowned bodies in the Paddington Canal in the middle of December 1802. When the bodies proved to be those of Foster's semi-estranged wife and infant child, Foster was arrested under "suspicion of murder." At his trial on 14 January, he made a weak and self-contradictory defense with a dubious alibi, and the jury took only twenty minutes to find him guilty. By the time of his execution three days later, the papers claimed, he had fully confessed to pushing his wife and child into the canal, and as he went to the scaffold to be "launched into eternity," as one report put it, "his air was dejected in the extreme, and the sorrow manifested in his countenance, depicted the inward workings of a heart conscious of the heinous crime he had committed." The three news events of the unhappy Foster's arrest, his trial, and his execution were all very widely reported in the daily press, with the *Times*, the *Morning Chronicle* and the *Morning Post* (for example) all covering the trial at considerable length.[64] The report of the galvanic experiments on his corpse, appearing a few days after paragraphs detailing his execution, added a last piece to this widely-consumed series of crime, arrest, trial, and punishment. It must have made the experiments all the more disturbing for the original readers; for them, Foster was not just a faceless anatomical subject but someone whose life-story they had been avidly consuming a week before in reports of his trial.

But, inevitably, the horrible experiments generated their own jokes. The *Observer* feared for "public tranquillity" when galvanism could so easily "open people's eyes, and set their jaws in motion"; surely Aldini ought to "to keep the living silent" rather than "set the dead to

[64] Arrest: *Times*, 21 December 1802 p. 3; *Morning Chronicle*, 21 December 1802 p. 4; *Morning Post*, 21 December 1802, p. 4. Trial: *Times*, 15 January 1803 p. 3; *Morning Chronicle*, 15 January 1803, pp. 3-4.; *Morning Post*, 15 January 1803, pp. 3-4; also *Morning Herald*, 15 January 1803, p. 3. Execution: *Times*, 18 January 1803, p. 3; *Morning Chronicle*, 18 January 1803, p. 3; *Morning Post*, 18 January 1803, p. 3.

talking."[65] And the *Morning Chronicle* dreaded the terrible consequences of the possible combination of galvanism with another current scientific curiosity:

> "While such high expectations are raised from the power of Galvanism, which can make the dead breathe, and murdered dogs bark, it must be the hope of all those who know the high price of beef, that no Galvanic miracle may be wrought on the Mammoth, whose teeth have been so long inactive; better to leave him quietly hanging to his chains, to the much greater gratification of all the curious, than that he should burst down the old Academy Rooms, and set all Pall Mall in an uproar."[66]

Aldini left Britain at the end of February 1803, hurrying back to France as the Peace of Amiens began to unravel and Europe prepared for another war.[67] Measured by his own scientific aims, his visit was a clear failure. He had not convinced anyone that mattered among British men of science that it was still worth taking the idea of animal electricity seriously. For Joseph Banks, everything Aldini did must have only served to confirm Blagden's warning of "egregious puffing and the other arts of charlatans." Aldini wrote up an account of his experiments, which he left in England for publication.[68] Anonymously reviewing the book in the *Edinburgh Review*, the Edinburgh surgeon John Thomson roundly denounced Aldini's work as a fine example of "the vanity of systematizing upon an imperfect series of experiments." Thomson professed to have "looked in vain" for "the important discoveries which the author promises." Aldini's trials on animals might well be "very curious," but they did not furnish "any proof of the presence of a

[65] *Observer*, 30 January 1803.
[66] *Morning Chronicle*, 21 February 1803.
[67] He stopped off at Calais to try some experiments on the long-distance conduction of galvanic electricity through the sea, and in March was back at Paris and attending the weekly meetings of the ever-supportive Société Galvanique, to whom he delivered an account of his experiments on Foster's body. Aldini, *Account of the Late Improvements in Galvanism*, 217-21; *Moniteur,* 1 Germinal an 11 (22 March 1803).
[68] It was published as Aldini, *Account of the Late Improvements in Galvanism*.

peculiar electricity in living bodies." And the experiments on Foster's body were simply "rather disgusting than instructive."[69]

But as a phenomenon in the newspapers, Aldini succeeded spectacularly. People remembered and talked about him for years afterwards; probably the most famous cultural progeny of his experiments, Mary Shelley's *Frankenstein*, was written over a decade later, in 1818. It also seems reasonable to give the newspapers' enthusiastic embrace of Aldini most of the credit for the appearance of the metaphorical sense of the verb "galvanize" in our language.[70] In the next few years, a band of home-grown imitators kept alive the craze for animal galvanism which Aldini had launched in Britain, showing experiments similar to his in lectures all around the country in venues ranging from London's Lyceum Theatre in the Strand to taverns in provincial towns.[71] Perhaps the most prominent of these galvanic lecturers was the enterprising surgeon Charles Wilkinson, who took his experiments on what he called a "galvanic tour" of Britain and Ireland in 1804.[72] Wilkinson was himself the subject of various newspaper reports, and galvanism remained a prominent scientific topic in the press, recurring in various ways

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[69] *Edinburgh Review*, 3 (October 1803), 194-98. The Wellesley Index to Victorian Periodicals cites Henry Brougham's manuscripts as indicating that John Thomson (1765-1846) was the writer of this review. But, somewhat arbitrarily, the Wellesley then gives the attribution instead to Humphry Davy. The evidence for this choice appears flimsy, and Davy is recorded as having written no other periodical reviews; according to his brother, he always refused requests to do so. Thomson, on the other hand, is on record as having reviewed several medical works for the *Edinburgh* at this time.

[70] The newspapers' jokes in 1803 would appear to be the earliest use of the verb "galvanise" in the figurative sense in which it is now deployed. The *Oxford English Dictionary* does not give an instance of the figurative use of the verb before 1853—but the usage can be found in plays and other literary works as early as 1806. See, for example, Thomas Dibdin, *Five Miles Off, Or, The Finger Post: A Comedy, in Three Acts* (London: Barker, 1806), 47: "I was completely galvanised!"

[71] The scientific showman "Mr. Hardie" ran a series of well-attended galvanic shows at the Lyceum; frequent newspaper advertisements promised that "the functions of Smelling, Biting, Chewing, Swallowing, Winking, and other voluntary Motions, will be induced in the Head of a Sheep, Ox, or other large Animal." *Morning Chronicle*, 29 September 1804. The self-educated Sheffield chemist, engineer, and itinerant lecturer Charles Sylvester (see Chapter 3) performed galvanic demonstrations in Nottingham, Sheffield, Rotherham, Leeds, York, Hull, Chesterfield, and Mansfield during 1804 and 1805; a surviving handbill from one of his Nottingham lectures promised "the astonishing Effects of Galvanism upon Dead Animals," to be shown at a tavern called the Spread Eagle in the marketplace. Collection of Nottingham broadsides and handbills, BL Shelfmark 1888.c.18/1(45).

[72] *NJ* 10 (January 1805): 56-58; *Caledonian Mercury*, 7 January 1805; Wilkinson was also the author of a book on the subject, the *Elements of Galvanism*, 2 vols. (London: Murray, 1804).

ranging from an "Ode to Galvanism" in the *Morning Post* in 1805 to more reports of experiments on corpses.[73]

The association of galvanism in most people's minds with grotesque electrical experiments on human and animal bodies did not please Britain's leading men of science. Humphry Davy disliked it so much that he came to wish the word banished from the scientific vocabulary entirely. At the heart of the issue, perhaps, were different conceptions of what counted as *important* scientific information; from the point of view of the newspapers and their readerships, Aldini's experiments were an extraordinary new thing under the sun (to borrow Lord Byron's reference to *Ecclesiastes*), made even more readable by their connection to other interesting things like fashionable Society and a grisly murder.[74] Unfortunately for Aldini, amid all the newspapers' jokes and lurid accounts of bellowing ox heads and galvanized dogs, his actual argument about the existence of animal electricity—the lynchpin of his particular claims to scientific novelty—had been almost completely obscured. And for their part, scientific opponents like Thomson and Davy simply thought his experiments rather pointless (as well as in rather bad taste); they maintained the discussion was closed and Aldini had done nothing really new to suggest otherwise.

The strategy adopted by British men of science during the publicity whirlwind generated around Aldini by the newspapers appears to have been mostly to keep quiet, ignore it, and wait for it to go away. They simply had very little influence over how the press treated science. Someone could conceivably have challenged Aldini with a letter to a newspaper, as William

[73] *Observer*, 4 December 1803; "Ode to Galvanism" in *Morning Post*, 11 October 1805. The most prominent repetition of Aldini's experiment on Foster's corpse was by the Scottish doctor Andrew Ure in November 1818, reported for example in *Observer,* 15 February 1819; another was subsequently made on an executed criminal at Cambridge: *Cambridge Chronicle,* 13 August 1819.

[74] Byron, in *English Bards and Scotch Reviewers* (1809): "Thus saith the Preacher: 'Nought beneath the sun / Is new'; yet still from change to change we run / What varied wonders tempt us as they pass! / The cow-pox, tractors, galvanism and gas, / In turns appear, to make the vulgar stare, / Till the swol'n bubble bursts—and all is air!"

Nicholson did when unhappy with the *Morning Chronicle*'s report on the Voltaic pile, but in general the disreputable British press was not considered a suitable venue for debating scientific matters: to engage publicly in this way would be beneath the dignity of a scientific gentleman. Elite scientific opinion remained absent from the papers (and the view of general readers) because of the particular British relationship between science and the press. In the French case it was an entirely different story.

Science and the Newspapers under Napoleon

Following Aldini back across the English Channel at the end of the Peace of Amiens, let us dive into the pages of the French press in July 1803, two months after the resumption of war. Consider the 25 July issue of the leading newspaper of Napoleonic France, the *Moniteur*. Dominating one of its four pages is a single item of scientific news, the text of a report read by the young savant Jean-Baptiste Biot at a recent public session of the Institut National on "the present state of knowledge relative to galvanism." Although the text never mentions Aldini by name, it is directed squarely at him and his work, and at enthusiasm for his experiments in Paris.

This is a strikingly different kind of newspaper writing from the many items on Aldini in the British press. The most obvious difference is in length: the thirteen paragraphs fill nearly two thirds of the page (and part of the next). That is one sixth of the entire newspaper. Scientific material in the British papers during this period simply did not occupy so large a proportion of column space.[75] Moreover, again in contrast the British situation, the piece is authored. These are Biot's words, with his name printed above them, endorsed with the headline "Institut National."

[75] The length of items of scientific news is a rather crude metric but still indicative of the level of importance accorded to the text and its function within the larger "package of novelties" offered by a newspaper. One short paragraph or a joke about Aldini's experiments might be regarded in uncharitable terms as virtually interchangeable with other entertaining curiosities. But the same cannot be said of longer, multi-paragraph items like those in the *Moniteur* or the accounts of Davy's lectures I describe below.

Here is an authoritative statement that we are reading the carefully-chosen phrases of a man of science rather than the composition of an anonymous newspaper-writer like Charles Lamb.

Then there is the tone. The text is relatively accessible and in places vividly and even lyrically written, but it makes no special effort to be exciting. It is serious, self-assured, devoid of lurid fascination. There are no jokes. This is the standard style in this newspaper, and Biot deploys it strategically, eloquently, and spiced with a little rancor. He depicts Aldini as a "*faux savant*" exploiting popular credulity, and mercilessly attacks all his claims about a connection between galvanic electricity and the powers of life. The discoveries of galvanism are extraordinary, Biot affirms, but the extent of their application has been over-estimated. Galvanism is a purely physical phenomenon, to be explained in terms of contact electricity, as Volta has already done. Endless experiments on frogs, and still worse Aldini's trials on dogs, oxen, and human bodies, are "experiments made at random, without goal and without method." They are "debris piled at the feet of the monument being raised up by Genius, obstructing rather than aiding the work." Chance can indeed sometimes yield great discoveries, just as it can "throw a precious diamond under the feet of the coarsest man," but, Biot asserts, "the eye of the lapidary is always necessary for reckoning the price." Better than transient novelties are plain facts, which "the true friends of science" will celebrate because they add "to the edifice of human knowledge some materials capable of resisting the ravages of time."[76]

Biot's report is not particularly unusual; in fact it is fairly representative of the kind of scientific content the *Moniteur* regularly carried. But in Britain material of this length, detail, and seriousness would have been found only in the pages of one of the scientific journals or some general magazines. So why did this French newspaper print it? The answer to this question

[76] *Moniteur*, 6 Thermidor an 11 (25 July 1803), pp. 1367-68.

hinges on the relationship of the French press to the Napoleonic state, and to men of science like Biot operating under the state's patronage.[77]

The French newspaper press under Napoleon remains under-studied, and this is partly a product of his great success in controlling the news and turning it to his own purposes. There is certainly a sense that its endless parade of propaganda, military dispatches, proclamations, tirades against England, appropriately-filtered foreign news, and cultural filler like florid theatre reviews, can induce—in the words of the historian who has researched it most thoroughly—"*un sentiment de profound ennui.*"[78] Napoleon simply did not want the press to be interesting in the manner of the British newspapers, which he loathed. His attitude to the press was determined by the basic structure of his regime: authoritarian rule from the political center, backed up by plebiscite and popular acclaim at home and glorious military victories abroad.[79] Public political discussion and the extremes of both left and right were suppressed in favor of national unity, efficiency, and domestic peace, and above all to guarantee the stable and orderly polity which had proved so elusive during ten years of Revolutionary governance. Anything in the press which Napoleon saw as a threat to this stability, he eliminated. This primarily meant political writing, but also largely absent from the French papers are other mainstays of the British press

[77] On the many-layered interactions between science and the French state in this period, see especially Charles Gillispie, *Science and Polity in France: The Revolutionary and Napoleonic Years* (Princeton: Princeton University Press, 2004).
[78] André Cabanis, *La Presse Sous le Consulat et L'Empire, 1799-1814* (Paris: Société des Etudes Robespierristes, 1975), 1; this work remains the key reference for the Napoleonic press.
[79] In his classic survey of the character of Napoleonic France, Louis Bergeron succinctly defines Napoleon's own vision of his rule: "the close of the Revolution, a return to order and stability, a re-joining of the history of France with the first stage of the Revolution, but only the first stage." Louis Bergeron, *France Under Napoleon*, trans. R. R. Palmer (Princeton: Princeton University Press, 1981), 4. While Napoleon was often perceived outside France (and especially in Britain) as a quasi-Jacobin Revolutionary unleashing upheaval across Europe, inside France, the basic package offered by his regime was order, rational progress, pragmatic and moderate reforms, and national glory, all guaranteed by a strong, efficient, and stable sovereign power vested in himself.

model like scandal, reports of suicides, and crime. As Louis Bergeron puts it, "the press existed in his eyes only for service to the State and as an instrument of propaganda."[80]

Bonaparte moved quickly to ensure the press would serve this purpose. The Revolution's potent combination of thrilling political events and the sudden abolition of censorship and *Ancien Régime* press privileges (state-granted publishing licenses) had given rise to a deluge of print news, quickly peaking in 1790 with an astonishing 335 different titles published in the capital alone.[81] When Napoleon came to power in November 1799, there were over 65 newspapers in the Paris region.[82] Two months later he had suppressed all but 13, and given those to understand that they survived on sufferance.[83] By 1811, only 4 Paris newspapers remained, all subject either to state direction or censorship, and all serving up a blandly-flavored diet of carefully-controlled daily news to the readers of the Empire.[84]

The *Moniteur* was at the center of Napoleon's news system, and crucial for science's presence within that system.[85] A few weeks after his *Brumaire* coup, the paper was made the official newspaper of the Consulate for printing decrees, legislative sessions, and other kinds of news originating with the government, and placed under the control of Napoleon's new Secretary of State, Hughes-Bernard Maret.[86] This created the conditions for a new kind of relationship between science and the press. In a move which is perhaps best understood as a component of the extensive series of reforms in early 1800 following the promulgation of the Constitution of

[80] Bergeron, *France Under Napoleon*, 8.
[81] Claude Bellanger et al., *Histoire Générale de la Presse Française*, vol. 1 (Paris: Presses universitaires de France, 1969), 436.
[82] Cabanis, *La Presse Sous le Consulat et L'Empire*, 41.
[83] *Moniteur*, 29 Nivôse an 8 (18 January 1800). An exception was also made for any "journals occupied exclusively with the sciences," as well as "the arts, literature, commerce, announcements and public notices."
[84] Cabanis, *La Presse Sous le Consulat et L'Empire*, 11-41. The four survivors were the *Moniteur, Journal de l'Empire* (formerly *Journal des Débats*), *Gazette de France*, and *Journal de Paris*.
[85] The *Moniteur* estimated its own circulation at 6,000 copies; the true figure seems to have been somewhat lower: Cabanis, *La Presse Sous le Consulat et L'Empire*, 320-22.
[86] The *Moniteur* proclaimed its status as "the only official newspaper" from its issue of 27 December 1799. This was accompanied by a new statement on the masthead that "One article will be particularly devoted to the sciences, arts, or to new discoveries."

the Year VIII, and which coincided almost exactly with Napoleon's mass closure of most of the Revolutionary newspapers, Maret wrote to the secretaries of the different classes of the Institut proposing a special arrangement with the *Moniteur*.[87] The scientific Première Classe received his letter on 5 February.[88] Maret informed them that the *Moniteur* was now the newspaper by which the Government intended to make known to all Europe not only political, military or administrative announcements but also "everything which must contribute to national glory in letters, the sciences, and the arts." He therefore requested that the class send him reports of new discoveries or other articles which it deemed interesting and wished to have published "without delay." The class voted to accept his request.[89] This formal arrangement underwrote the presence of articles like Biot's in the *Moniteur*, and ensured that items provided by the Institut were a regular feature in its pages all through the Napoleonic period. They ranged from short paragraphs briefly announcing new discoveries to spectacularly-long articles such as the Institut's report on Volta's pile, which spanned six of one issue's twelve columns.[90]

The kind of scientific news this system produced fitted rather well into Napoleon's model for the press. From the state's point of view, scientific reports filled some of the void created by the suppression of domestic political news, and filled it with an alternative kind of novel material which was safe, potentially enlightening, and completely consonant with Napoleon's expectation that the press promote an image of "*la gloire nationale*."[91] And although Maret's arrangement

[87] This series of consolidating reforms included a rearrangement of the structure of the legislature, the introduction of the Prefects to French *Departéments*, and the creation of the *Banque de France*.
[88] Maret to the Secretaries of the scientific sections of the Institut, 16 Pluviôse an 8 (5 February 1800), *Pochette* for 21 Pluviôse an 8 (10 February 1800), Archives of Acadèmie des Sciences, Paris. *PVSA*, 16 Pluviôse an 8 (5 February 1800), 2:100
[89] *PVSA*, 11 Ventôse an 8 (2 March 1800), 2:114; the Classe was approving a strongly favorable report on Maret's request produced by Tessier and Monge, preserved in *Pochette* for 6 Ventôse an 8 (25 February 1800), Archives of Acadèmie des Sciences, Paris.
[90] *Moniteur*, 7 Nivôse an 10, (28 December 1801), pp. 369-70.
[91] For example, the other main features in the issue of the *Moniteur* that contained Biot's report were a ponderous relation of the magnificent welcome supposedly given to Bonaparte on a visit to Flanders, and a series of formulaic

ensured the *Moniteur* remained preeminent, other French newspapers also manifested a strong interest in reporting scientific discoveries and inventions in an apparent effort to fill the political news vacuum.[92]

Moreover, while the *Moniteur* contained numerous disquisitions from the other classes of the Institut on literature, poetry, history, and the fine arts, the natural sciences clearly had a special place in its pages. News about the sciences almost effortlessly projected the image of stable, controlled *progress*, founded in an epistemology of appeal to the sole authority of the facts of nature rather than the enlightened public opinion Napoleon so distrusted. And of course science was the intellectual pursuit most dear to Napoleon personally: he was even a member of the Première Classe himself. The *Moniteur* proudly noted how, even a mere five days after 18 Brumaire, and "amidst the great matters with which he is occupied," he had not neglected to attend its meetings.[93] The flow of authoritative judgements from the Institut outwards into the official press rather naturally mirrored the centralized structure of the regime as a whole, and brought the cultural authority of science into extremely close alignment with the political authority of the state. During the first year of Napoleon's rule, one newspaper, the *Ami des Lois*, was even shut down for daring to attack the Institut National with (as the *Moniteur* complained) "ridicule and sarcasm."[94] There could scarcely be a sharper contrast with the irreverent, humorous, and vociferously independent British press.

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patriotic addresses to Napoleon from representatives of the French provincial governments, dedicated to extolling French military glory and railing against the malevolence of England. For many readers Biot's piece was probably the most engaging and fresh item of domestic news in the paper.

[92] In particular, the *Journal de Paris* had a regular "Nouvelles des Sciences, des Lettres et des arts" section, which often reprinted scientific news from the *Moniteur* and other sources. The *Journal de l'Empire* and the *Gazette de France* chose to focus more on theatre reviews, literature, and the fine arts, but did offer some in-depth scientific content, such as a series of long reports on Franz Joseph Gall's Phrenology lectures in 1808 (first report *Journal de l'Empire*, 27 January 1808).

[93] *Moniteur*, 24 Brumaire an 8 (15 November 1799), p. 209.

[94] *Ami des Lois*, 7 Prairial an 8 (27 May 1800), pp. 2-3, which included attacks on Laplace and Cuvier for acting as tools of the government. Decree of suppression in *Bulletin des lois de la République Française*, 3e série, vol. 1

But if scientific articles in the *Moniteur* functioned as Napoleonic propaganda, they were not *merely* propaganda; on the contrary, they performed a crucial role in the wider international system of scientific information. Publication was exceptionally fast, sometimes occurring the day after material was read at the Institut, and printing a text in the paper was a very reliable way to ensure it travelled quickly and widely both within France and beyond its borders, including, as we shall we in Chapter 4, to Britain. Moreover, the arrangement granted the scientific members of the Institut an unusually high level of control over the public image of science in the papers, even down at the level of the text itself, which the members wrote themselves and which Maret promised to replicate with especial "care and exactness." The Institut complained how, in the past, "journalists have by themselves published articles concerning the work of the Institut which have been badly rendered, whether through negligence or through a lack of knowledge of the material which is treated." They were pleased that they "would not have this inconvenience to fear with the *Moniteur*."[95]

And so the members of the Institut were virtually handed the authority to decide what scientific information was worthy to be printed in the French papers and what was not, a choice which in Britain lay squarely with the non-scientific editors and paragraph-men of the newspapers who had embraced Aldini. On occasion, the Institut used this power to print material that quite frankly must have been completely incomprehensible to most of the *Moniteur*'s readership. Biot's article would at least have been relatively accessible to someone with only a little acquaintance with galvanism, but it seems unlikely that many French men and women who perused the paper on 30 June 1800 would have got much out of the daunting extract on the third

(Paris: Imprimerie de la République, 1800), no. 28, pp. 8-9 (Arrêté du 9 Prairial an 8); *Moniteur*, 10 Prairial an 8 (30 May 1800).

[95] Report to the Classe by Tessier and Monge, in *Pochette* for 6 Ventôse an 8 (25 February 1800), Archives of Acadèmie des Sciences, Paris.

page from a recent memoir by Laplace concerning nutation in the orbit of the moon.[96] In cases like this the readership divided into those few scientific initiates who were equipped to make sense of the content (and who would benefit from the *Moniteur* bringing it to them reliably and quickly), and everyone else. For readers not capable of understanding, the articles would at least contribute to a general sense of grand scientific progress and French intellectual preeminence, and it was perhaps characteristic of both the Institut and the Napoleonic press regime that nobody seems much to have cared whether scientific news would be generally accessible as long as it filled this function.

Biot's article, however, was certainly written and published in part as an address to scientifically-interested general readers. It was an attempt to reassert what Biot termed "the language of truth" against the excessive enthusiasm surrounding galvanic experiments, especially those concerned with life.[97] "The imagination," Biot complained, "has seen in these purely mechanical convulsions the momentary reestablishment of life, and in the fluid which causes them, the principle of our very existence." But novelty, the stock-in-trade of Aldini and other "false heroes who are always boasting of their exploits," ought to be distrusted by "the true friends of science." In the Napoleonic spirit, popular fashion and opinion ought to be subordinated to judgement by centralized scientific authority. "Novel facts pique our curiosity by the marvels which accompany them," he cautioned, but by rational, controlled investigation, "we seize the chain which unites them, and what seemed boundless reduces to one or two new facts." In this case, the facts reduced to Volta's contact theory of the voltaic pile, they had no special

[96] *Moniteur*, 11 Messidor an 8 (30 June 1800), p. 1135. Another example is the article containing Gay-Lussac and Thenard's experiments with potassium and ammonia discussed in Chapter 4, whose details would probably have made sense to only a handful of people in the world, all adept chemists.
[97] Biot also cautioned against galvanism's uncontrolled application to medicine, which he thought useless and probably dangerous in some cases.

relationship to life, and for Biot this was the final word on the matter of galvanism. He hoped everyone would soon see it this way.

But Biot's attack was fuelled by more than a dislike of uncontrolled enthusiasm for scientific novelties. In fact, Aldini's galvanism posed a particular problem because it had actually been receiving a measure of the same kinds of official endorsements as the work of the Institut—including being printed in the *Moniteur*. As we saw, Aldini had not had much success at the Institut, but he had managed to consolidate an alternative base of support in Paris at the new Société Galvanique.[98] The membership of the Société overlapped with that of the Institut in a few cases, and even included Napoleon's brothers Lucien and Joseph Bonaparte. Its focus was squarely on investigating the connections between galvanism and the vital force, a project in which Napoleon himself was deeply interested.[99] From its first meetings, in an apparent mirroring of the Institut's special arrangement with the press, bulletins of its activities appeared in the *Moniteur*—including detailed reports of Aldini's experiments. For Biot, it clearly presented a challenge to the authority of the Institut as the first and final court of scientific judgement in France. Hence the virulence of his opposition: he was not so much ridiculing an outside enemy as denouncing what he saw as a scientific fifth column within the Napoleonic system. His clarion call to reason was not immediately successful, but the Société Galvanique's

[98] In the early stages of the Société's formation, two prominent members wrote to the Minister of the Interior (Chaptal) advertising galvanism's possibilities as a new medical therapy and requesting official government support for the Société's endeavours. Archives Nationales, Paris, F/8/2 (Dossier VII), 25 Pluviôse an 11 (14 February 1803).
[99] Blondel, "Animal Electricity in Paris," 195-97. Napoleon remained fascinated by the relationship between galvanism and the force of life. In exile on St. Helena in 1817, he remarked to his companion Gaspard Gourgaud that he thought electricity, galvanism, and magnetism contained "the great secret of Nature," and that "Galvanism works in silence. For myself, I believe that man is the product of these fluids and of the atmosphere, that the brain pumps in these fluids and gives life, that the soul is composed of these fluids, and that after death they return into the ether, from where they are pumped by other brains." Entry for 6 January 1817, in Gaspard Gourgaud, *Sainte-Hélène: Journal Inèdit de 1815 à 1818* (Paris: Flammarion, 1899) 1:386.

appearances in the *Moniteur* did become much less frequent. It eventually faded away around 1809.[100]

But such minor divisions of scientific sovereignty aside, the privileged access to the press which the Institut enjoyed was clearly overwhelmingly useful in cementing its authority and projecting an image of a controlled, stable, productive, and state-backed scientific enterprise both within France and abroad. In London, the *Moniteur* was not only assiduously read by men of science for information on French work; it also formed the source for most of the Continental scientific news published in the "intelligence" sections of the British monthly scientific journals, *Nicholson's Journal* and the *Philosophical Magazine*.[101] In translation, these reports of work at the Institut reproduced the authoritative precision of the *Moniteur* for British readers.[102] The attitudes of British men of science to the French Institut are hard to characterize, being compounded of an incongruous mixture of emotions: rivalry, admiration, anxiety over possible British inferiority, collegiate solidarity with fellow scientific investigators, distaste for the Institut's close association with an enemy regime, and envy.[103] The proportions naturally varied between individuals and through the different phases of the Napoleonic conflict. But no serious man of science in Britain could ever afford to ignore the latest news from Paris, and more than a few of them were a little mesmerized by the Institut's projections of scientific power that filtered into Britain in French journals and in copies of the *Moniteur*.

This is nicely captured in another episode in the newspapers at the time of the Peace of Amiens. Near the end of 1801, Sir Joseph Banks was elected a foreign associate of the Institut

[100] The last reference to it in the press appears to be *Moniteur*, 16 August 1809.
[101] It was almost always uncredited as a source, however.
[102] In contrast, reports of Royal Society meetings in the British journals could sometimes prove inaccurate, causing confusion in Continental journals where they were reprinted.
[103] The British attitude to Napoleonic France and Napoleon himself was obviously in large part antagonistic during this period, but in fact encompassed a multiplicity of registers, ranging from admiration (for example, Charles Blagden during the Peace of Amiens) to virulent hatred. See Stuart Semmel, *Napoleon and the British* (New Haven: Yale University Press, 2004).

National, and he accordingly wrote them a letter of thanks in January 1802. In a singularly poor choice of phrase for a President of the Royal Society, Banks expressed his pleasure at being chosen an associate of "the First Literary [i.e. learned] Society in the world."[104] In the eighteenth-century Enlightened Republic of Letters in which Banks had risen to prominence, this might have been an insignificant and private bit of polite flattery. Unfortunately for him, he had reckoned without the Institut's new closeness to the French press. Banks's letter was clearly an excellent piece of propaganda for Napoleonic science, and accordingly it was soon printed in translation in the *Moniteur*.[105] One of the copies of that paper then completed the circuit and brought Banks's ill-advised words back to London four days later, whereupon they were reprinted in a paragraph in the *Times*.[106] This publicity was bad enough, but Banks soon got much worse from the radically anti-French journalist William Cobbett, who translated Banks's entire letter and reprinted it in his *Political Register* newspaper with a scathing commentary on "this load of filthy adulation."[107] Cobbett continued the barrage in his next weekly issue, and the scandal resurfaced six months later with a letter from an anonymous "Fellow of the Royal Society" calling on Banks to be denied re-election as President.[108] Banks weathered the storm, but the whole sordid little incident was clearly bad for the Royal Society and indeed the public image of science as a whole in Britain. In contrast, it seems fair to say that the glittering French scientific elite at the Institut derived nothing but benefits from the cowed, insipid, and

[104] See the draft letter of 21 January 1802 in *Banks Corr.*, 5:124. Banks was elected on 17 November 1801: *PVSA*, 2:435, 26 Brumaire an 10 (17 November 1801).
[105] *Moniteur*, 27 Ventôse An 11 (18 March 1802); *PVSA*, 2:460, 11 Pluviôse an 10 (31 January 1802).
[106] *Times*, 22 March 1802, p.2.
[107] The commentary is signed "Misogullus"; it is unclear if it was written by Cobbett himself or sent to him by someone else. It combined attacks on French science with an ad-hominem broadside against Banks, lambasting him for "sentiments of servility, disloyalty, and falsehood, which ought never to be conceived by an English heart, never written by an English hand, and least of all by yours," and for bringing "discredit on your country, and discredit on the Royal Society." *Cobbett's Weekly Political Register*, 27 March 1802.
[108] *Cobbett's Weekly Political Register*, 3 April 1802; 6 November 1802. The letter in November noted that Banks had responded only by maintaining a "cautious and gloomy silence on the subject."

propagandized press under Napoleon, who was their natural ally in his approach to the news as in much else.

Patriotic and Unpatriotic Chemistry: Humphry Davy in the Press

In the spring of 1811 Humphry Davy had many reasons to feel pleased. He was newly famous, widely hailed in Romantic terms as a man of "genius," and in the midst of delivering a course of lectures to adoring crowds of up to 1,000 people at the Royal Institution on "chemical philosophy" and on electrochemistry, the new science he had effectively created out of galvanism. But he had been reading about himself in the newspapers, and he was not happy. What irritated him was a new phenomenon: the regular press coverage of his lectures. He had hoped for good things, but on seeing the newspapers' reports, he thought them inaccurate, sloppy, misinforming to the public, and potentially damaging for his scientific reputation. "I am made to say things which I never thought of saying and which are absolutely absurd," he complained to a correspondent, remarking that he had been quoted as saying that "Platina gives off Hydrogen in vacuo" and that carbonic oxide [carbon monoxide] was a compound of carbonic acid and oxygen.[109] Not only his opinions but even his basic chemical facts were being misrepresented. Davy's public image in fact benefited a great deal from the British newspapers, and in the case of his lectures especially he was very fortunate in their coverage.[110] But, frustratingly for Davy, it was clear that the press was making use of him, and not the other way

[109] Davy to Thomas Allan, 1 April 1811, RI HD/26/D/4b (transcript of lost original, made by John Davy).
[110] For example, the beginning of Davy's years of fame after his discovery of potassium and sodium in 1807 was marked by the circulation of a laudatory item in the press entitled "New Discovery in Chemistry," proclaiming that "Mr. Davy's name, in consequence of these discoveries, will be always recorded in the Annals of Science amongst those of the most illustrious philosophers of his time. His country with reason will be proud of him." *Morning Chronicle*, 26 January 1808.

around. He was master of what he said in the lecture theatre, but outside it he could exercise very little control.[111]

The reports on Davy's lectures in papers like the *Observer* and the *Morning Chronicle* constitute one of the most detailed and serious engagements with science by the British newspapers during this period.[112] On the face of it, they furnish an illuminating contrast with the press's treatment of Aldini: lurid experiments, grotesque corpses, and jokes giving way to the ordered pronouncements of an expert man of science. Davy indeed disliked Aldini's brand of galvanism, but his actual relationship to that kind of scientific display was a little more complicated. To understand how, we need to return to the winter of the Peace of Amiens, when, at the height of the craze of interest in Aldini's galvanism, Davy made his debut in the newspapers.

"The novel science of Galvanism appears to have awakened a general spirit of attention and curiosity," declared one London newspaper in February 1803. It followed this with a paragraph describing a recent lecture by the young Davy on galvanism at the Royal Institution, which had drawn "the most numerous audience that has been known there since the

[111] My treatment of Davy here and elsewhere is indebted to the discussion of him in Jan Golinski, *Science as Public Culture: Chemistry and Enlightenment in Britain, 1760-1820* (Cambridge: Cambridge University Press, 1992), 188-235; see also Jan Golinski, "Humphry Davy and the Lever of Experiment," in *Experimental Inquiries: Historical, Philosophical, and Social Studies of Experimentation in Science*, ed. H. E. LeGrand (Dordrecht: Kluwer, 1990), 99-136. My main point of departure from Golinski's insightful account concerns the agency granted Davy in the crafting of his public image. While Golinski's Davy is depicted as effortlessly manipulating audiences at the RI to compel assent to his experimental truths (the audience formed, in Golinski's phrase, his "lever of experiment"), I am more interested in how Davy's public image emerged out of the ways he was *used by* the newspaper press, and by the aristocratic and fashionable RI audience whose interests and tastes shaped the style and content of his lectures. In this view, Davy's public image was as much the creation of his audience and of the newspapers as it was a consequence of his own self-fashioning.

[112] Previously it was thought that serious press reports of RI lectures began with Davy's successor Michael Faraday: Frank James, "Reporting Royal Institution Lectures, 1826-1867," in *Science Serialized: Representations of the Sciences in Nineteenth-Century Periodicals*, ed. Geoffrey Cantor and Sally Shuttleworth (Cambridge, MA: MIT Press, 2004), 67-79.

commencement of that establishment."[113] Noting that Davy "did not exhibit any experiments on *muscular contractions*"—perhaps because they would not have been "sufficiently delicate" in front of the RI's mixed-gender audience—the newspaper went on to describe how he had made up for this by showcasing some remarkable phenomena that involved the chemical effects of galvanism, especially "the beautiful experiments of the combustion of the metals" and "the ignition of charcoal under water." Another report of the lecture in the *British Press* newspaper noted that Davy had already "enriched" galvanism with "several new facts and discoveries" of his own.[114]

The newspapers' interest in these lectures—and therefore in Davy—was clearly a side effect of Aldini's visit. Both items were short single paragraphs, with scientific details limited to cursory descriptions of a few spectacular experiments ("A few years ago it would not have been believed that copper, silver, and gold, could be set on fire by means of the Galvanic spark"). Although they contain no experiments with animals and therefore leave a much more sanitized and orderly impression than Aldini's headless dogs, in other respects they are not very dissimilar to some of the paragraphs describing the Italian's demonstrations. There is even interest in the fashionable aristocratic makeup of the audience: the *British Press* thought the lecture showed how "a taste for scientific pursuits is rapidly diffusing itself among that class of society, whose chief distinction has hitherto consisted in title or fortune."

Here is the key to Davy's relationship towards Aldini. Davy made plenty of declarations against excessively showy and fashionable science in his lectures, but these were prompted not so much by his obvious *differences* from Aldini (which was Biot's position) as by a fear of being perceived as *too similar* to him. Davy had a difficult balancing act to perform at the Royal

[113] This report is quite clearly from a London newspaper printed around 17 February 1803, but I have not been able to trace the original. It is reprinted in *Ipswitch Journal*, 19 February 1803.
[114] *British Press*, 25 February 1803.

Institution.[115] His lectures had to be sufficiently appealing to draw large audiences (they were in fact keeping the Institution's finances afloat), but at the same time Davy was painfully aware of a criticism regularly levelled at the RI: it was putting science too much in thrall to the "fashionable world." Serious-minded observers like Henry Brougham denounced its atmosphere as a "thick medium of fashionable philosophy," and so ingrained did the association become that one of Davy's lectures on galvanism there furnished the subject for an entire chapter of humor in a now-forgotten novel satirizing fashionable Society, *A Winter in London*.[116] The challenge was how to be popular without slipping into Aldini's dubious and chaotic register. Davy worked hard to keep just the right balance in his lectures: there would be plenty of spectacular crowd-pleasing demonstrations, but they would all be for a serious end. The true experimenter, he declaimed to his audience in 1810, should not stoop to making himself "subservient to transient amusement," or become a "magician playing off tricks to create astonishment and wonder." And the ideal audience member should attend "not merely from curiosity, or the hope of being amused, but from the genuine love of knowledge."[117]

By 1811, when the newspapers began to offer serialized reports of entire courses of his lectures, Davy was a master at this balancing act. The key development had been the beginning of the major discoveries that made him both famous across the scientific world and a public figure in Britain, starting in 1807 with the isolation of potassium and sodium by galvanism. More

[115] The foundation of the Royal Institution in 1799 has long been seen as marking a transitional moment in the history of science in British public life, with Davy's tenure there establishing its dual role as a site for both popular lectures and private research that it was to hold under Faraday and throughout the nineteenth century; see Morris Berman, *Social Change and Scientific Organization: The Royal Institution, 1799-1844* (Ithaca: Cornell University Press, 1978); David Knight, *Humphry Davy: Science and Power* (Cambridge: Cambridge University Press, 1992); Golinski, *Science as Public Culture*, 188-235. Davy lectured there from 1801-1812, and by 1803 he was already beginning to define the RI's public face; see Robert Siegfried, "Davy's 'Intellectual Delight' and his Lectures at the Royal Institution," in *Science and the Sons of Genius: Studies on Humphry Davy*, ed. Sophie Forgan (London: Science Reviews, 1980), 177-200; George Foote, "Sir Humphry Davy and his Audience at the Royal Institution," *Isis* 43 (1952): 6-12.

[116] Henry Brougham, *Edinburgh Review* 11 (January 1808): 390. Thomas Skinner Surr, *A Winter in London, Or, Sketches of Fashion* (London: Phillips, 1806), 2:172-86.

[117] RI MS HD/3/B/6; MS HD/2/D/1.

new elements followed, along with bold ideas on everything from the elemental nature of chlorine to the causes of volcanic eruptions. After 1807, Davy's lectures changed. He used them to delineate his new science of electrochemistry, and to make novel scientific arguments with his audience as participant. It was the ideal way to bury accusations of pandering to fashion. Rather unexpectedly, the RI had suddenly become a key site for the announcement of scientific developments—anyone who attended when Davy was speaking could expect to hear something new, and even eminent men of science could be spotted in the audience.[118] It was this quality that made Davy's lectures so attractive to the press, and brought his aims into rough alignment with theirs.

Let's take a closer look. On 23 February 1811 Davy delivered the sixth lecture in his spring course on chemical philosophy and electrochemistry. The next morning, a detailed report of the lecture was printed in the *Observer* newspaper, as part of a series of reports that would eventually take in all fourteen lectures in Davy's course.[119] The lecture was on galvanism and the action of the voltaic pile, and the *Observer*'s report reveals how Davy's performance was made into popular news for the press that reflected developing events within the scientific world. A week or two before, a copy of the *Moniteur* had arrived in London containing part of a report read by Cuvier at the Institut National on the Institut's work during the year 1810.[120] The *Observer* had printed translations of some of the key sections in its previous week's issue, including a new claim by French chemists Gay-Lussac and Thenard about the power of galvanic

[118] For example, Davy noted their presence in a lecture of 1808. "I see with a mixed sensation of pleasure & uneasiness seated on these benches Proficients in Science much more capable of instructing me than I am of enlightening them." RI MS HD/2/D/3.
[119] *Observer*, 24 February 1811. As Davy's lectures for this course took place on Saturdays, the *Observer*, a weekly Sunday newspaper, had an advantage over much of the competition which published six days a week.
[120] *Moniteur*, 15 January 1811, p. 58.

batteries.[121] They had announced experiments purporting to show that the "force" of a battery was not proportional to the number of the metal plates used, but in fact varied as the cube root of the number of plates.

And now, in this week's lecture, Davy had announced from the floor of the Royal Institution that they were wrong. As the *Observer* put it, "Dr. Davy said that he had made a variety of experiments during the present week, to ascertain the fact, and he had found that the statement of these philosophers was not correct."[122] Although he offered no law of proportion of his own to replace the one proposed by the French, Davy showed his audience a series of trials on the number of plates of a certain area needed to burn different lengths of iron wire, which he said proved that battery power could not possibly increase as slowly as the French proposed. He put the blame squarely on the "imperfect construction" of French batteries. Having dispensed with his rivals, Davy moved onto other related subjects, which the newspaper also reported in detail. He demonstrated experiments with the action of the battery on different metals, took a detour into the history of galvanism, and castigated a popular galvanic medical therapy, Perkins's "metallic tractors."[123] The lecture ended with a rousing peroration on "the connection which subsisted between the progress of science and political freedom," a model example of the vigorously patriotic scientific discourse which Davy framed in direct opposition to the dominant position of French chemistry. Drawing a comparison with ancient Greece, Davy described (said the newspaper) how in Britain "the mind was roused to public energy by the cheering voice of public freedom and popular applause." Napoleonic science might show a glittering face, but its

[121] *Observer*, 17 February 1811. Here, the *Observer* was scooping the monthly scientific journals, which went to press at the end of each month.
[122] "Dr." Davy was a recent addition which the *Observer* had picked up on: Trinity College Dublin had conferred an honorary LLD on Davy in November 1810.
[123] On the craze for Perkins's Tractors see James Delbourgo, "Common Sense, Useful Knowledge, and Matters of Fact in the Late Enlightenment: The Transatlantic Career of Perkins's Tractors," *William and Mary Quarterly* 61 (2004): 643-84.

feet were clay. “The patronage of a tyrant might for a short period give a sickly encouragement to science,” he admitted, but in the end, “it could only strike root with vigour in a land where men enjoyed the power of free discussion.” It was a sentiment well-calculated to resonate with the wartime British press.

In the *Observer* reports of his lectures, Davy was cast in a powerful, commanding role, endowed with a new kind of voice for a British man of science in the newspapers.[124] What he said at the RI was invested with importance, and became the next day’s news. Readers were invited to a rare glimpse of a master quarryman at work right at the coalface of scientific progress, chipping away at the unknown. The *Observer*’s report of Davy’s first lecture drew those readers in by relating Davy’s comment that on subsequent occasions he would “have the satisfaction of announcing some new and striking experiments, which he believed would throw considerable light on many of the obscure parts of chemical philosophy.”[125] There was a palpable expectation that new discoveries were on the program. “You are the first persons in this island who have witnessed this novel creation of experimental science,” Davy had told his audience as he introduced them to his new element “boracium” in a lecture in 1809.[126] Davy’s own field of electrochemistry furnished a particularly rich trove of novelty, because there was simply nowhere else the public could read about the subject: Davy had published no synthesis that would consolidate his fragmentary and technical *Philosophical Transactions* papers or

[124] The sporadic reports of a few of Thomas Garnett’s early lectures at the RI that we encountered in the last chapter have some similarity to the later reports of Davy’s lectures, but these arose more from the large interest in the recent foundation of the RI rather than from a focus on the person of the lecturer (they formed part of a series which included news on other aspects of the new Institution). Garnett was forced out of the RI not long afterwards (Davy was his replacement), and he never had the opportunity to build up the kind of fame or public scientific authority that Davy did.

[125] “Few events in the history of science,” trumpeted the same newspaper, “have excited more curiosity and expectation than the discoveries which he has recently made.” *Observer* 3 February 1811.

[126] RI MS HD/3/A/7. “Boracium” [boron] was the subject of another Anglo-French battle, as both Davy and Gay-Lussac and Thenard claimed credit for its discovery.

incorporate responses to French criticisms.[127] "The subject you will feel is in a great measure unexplored," he told his audience in 1808. "Little or no assistance in the investigation can be derived from Books."[128] Now the press was offering what books could not.

These reports of Davy's lectures are closer to the Institut's productions in the *Moniteur* than they are to the chaotic paragraphs on Aldini's experiments from a few years earlier.[129] Davy's regular weekly lectures at the RI turned out to be an excellent venue for making respectable but stimulating scientific news. The newspaper reports were not directly authored by Davy, but it is largely his voice we receive, taken down and broadcast through print. If we were to look about for an established mode of British news which this mirrors, it would be Parliamentary reporting—and in fact it seems reasonably plausible that the individuals whom the *Observer* and later the *Morning Chronicle* deputized to take down the Davy's words were the same people sent to record debates at Westminster.[130] Newspapers were the natural publications to cover the lectures; with one lecture per week, they had an obvious speed advantage over the monthly periodical press, and the lectures' weekly serialization created expectation and established a sense of continuity in the systematic exploration of a scientific field. Men and women not lucky enough to hold RI subscription tickets could still experience the best of what Davy had said only one or two days afterwards; those who were attending in person but happened to miss a lecture had a ready-made way of catching up before the next Saturday arrived.

The newspaper reports combined summaries of Davy's experiments and ideas with his more eloquent excursions into philosophical, historical, or political territory. Besides Davy's

[127] Davy planned to publish an integrated account of this work, but never did; see John Davy's statement in *Davy Works*, 5:vii.
[128] RI MS HD/2/D/1.
[129] The length of the items is unusual, as is the level of detail in particular experiments.
[130] These people were the only "reporters" employed by newspapers in this period.

criticisms of the French, the *Observer*'s series for the 1811 course also contained his views on scientific method—his two pillars were reverence for Baconian experimental facts and a distrust of excessive reliance on theories or "systems"—as well as natural-theological digressions presenting the sciences as a means of revealing and venerating the divine authorship of Nature.[131] The message: science was theologically and politically safe, and drawing materialist conclusions from science was an error. The mystery of the deep nature of organic life, in particular, was never going to be amenable to materialist explanations: "in vain had it been attempted," the newspaper reported him saying, "to explain the cause of life by changes produced, depending on chemical or mechanical principles."[132] It was from this direction that Davy launched his attack on Aldini's brand of galvanism, once again setting himself apart from that kind of public experimentation. "After the discovery of animal electricity," read the *Observer*'s account of his words, "an attempt was made to explain by it the powers of life." This had been a failure. "The immortal spark was itself a governor controuling the agencies of material elements," and associating it with the material phenomena of galvanism was "seeking the living amongst the tombs."

The *Observer*'s reports of the 1811 chemistry lectures were reprinted outside London from Edinburgh down to Davy's native Cornwall.[133] When Davy began a short course on geology soon afterwards, the *Morning Chronicle*, one of the leading London dailies, made use of the opportunity to begin publishing its own rival reports, and when Davy began to lecture on chemical philosophy and electrochemistry again in 1812 the *Observer* and the *Chronicle* continued to print competing accounts. "The high and universal interest which Mr. Davy's discoveries in science have excited, and the crowded assemblies which his Lectures attract to the

[131] *Observer*, 17 March 1811; 5 May 1811.
[132] *Observer*, 3 February 1811.
[133] *Caledonian Mercury*, 9 February–29 April 1811; *Royal Cornwall Gazette*, 16 March–11 May 1811.

Royal Institution, manifest a spirit of inquiry and a thirst for knowledge, truly honourable to the age," the *Chronicle* proudly announced, noting Davy's oratorical power over his audience, and how "he incessantly presents to them new and luminous views of the grandest operations of Nature, conveyed in the most simple style of explanation, but illustrated at times by images the most beautiful, and by applications to the Great First Cause the most edifying and sublime."[134]

A man of science could hardly have asked for a better write-up. Yet as we saw earlier, Davy was on the whole far from satisfied with the newspapers' coverage. The primary target of his irritation was the *Observer*, apparently because of its "absolutely absurd" rendering of some of his statements in the 1811 lecture course. In February 1811, Thomas Allan, a Scottish banker, printer, and devotee of mineralogy who owned the Edinburgh newspaper the *Caledonian Mercury,* wrote to Davy to ask his blessing on the *Mercury* publishing reports of his lectures. Davy replied saying he would have "no objection," but grumbling about the quality of the *Observer*'s renderings.[135] A few weeks later he was even more critical. "The accounts given in the Observer of my Lectures have been so extremely incorrect that I hope no report of the same kind has appeared at Edinburgh," he told Allan. This was in fact a veiled admonition, as Davy must have been well aware that Allan's items in the *Mercury* were in fact almost exact reprints of the *Observer*'s flawed reports.[136] The widespread reprinting in the periodical commons particularly worried Davy, as it spread erroneous versions of his ideas far and wide in a completely unpredictable manner that might well be doing damage to his reputation. When the *Morning Chronicl*e entered the field, Davy told another correspondent that he was happy its

[134] *Morning Chronicle*, April 30 1811. This report of one of Davy's 1811 geology lectures was reproduced as far afield as Washington, DC: *National Intelligencer*, 29 June 1811.
[135] Davy to Thomas Allan, February [1811], National Library of Scotland MS.581, no. 508. The letter has "14 February 1810" written at the bottom—but the year clearly should be 1811, as the Observer was not printing reports of Davy's lectures before that year.
[136] Davy to Thomas Allan, 1 April 1811, RI HD/26/D/4b (transcript of lost original, made by John Davy).

reports were "made with more care," noting that he had "protested against any republications of the reports in the Observer."[137] In turn, after being taken to task by Davy for recycling the *Observer*'s dubious versions, Allan launched his own alternative series of reports for the 1811 geology lectures in the *Caledonian Mercury*, now advertising them as "much more full and accurate" than "those in other papers."[138]

The newspapers' enthusiastic reports of his lectures may have been inaccurate, but they were of course overwhelmingly favorable to Davy, giving an added luster to his celebrity status.[139] But on other occasions, the press did not hesitate from giving him some of the savage treatment which it was famous for meting out to public figures. This was particularly so when Davy was perceived as being too friendly to French science or to France—a fact which perhaps explains the frequency of some of his public denunciations of French work at the RI.[140] In January 1808, the day after news of the award of Napoleon's Prix du Galvanisme to Davy appeared in the London papers, the *Times* printed a barbed paragraph declaring that Davy ought not to "gaze with stupid admiration on successful villainy" but rather be of "too lofty a mind to accept of any patronage" from Bonaparte, and sarcastically suggesting that Davy ought to donate the prize money to the widow of Johann Palm, a Nuremberg bookseller whose execution on Napoleon's orders had become a *cause célèbre* in the British press.[141] Davy was stung by these

[137] Davy to Dr. E. D. Clarke, 23 May 1811. Transcription from the Davy Letters Project. Original in unknown private collection; project uses transcription made by Warren Dawson, in the late June Fullmer's notes.
[138] *Caledonian Mercury*, 11 May 1811.
[139] But even the laudatory reporting of Davy's lectures in the *Observer* could provoke unexpected attacks, as when the paper printed a long letter from the once-respected but then rather marginalized savant Jean-André Deluc attacking the connections between electricity, chemical affinity, and the operation of the voltaic pile that Davy had argued for in his last lecture, and insinuating that Davy and the Royal Society Council had blocked publication of Deluc's own work. *Observer*, 3 March 1811.
[140] Chapter 4 shows how Davy in fact privately maintained good scientific intelligence links with France.
[141] *Times*, 21 January 1808. Palm had been executed for printing a pamphlet attacking Napoleon, and the case was latched onto by the British newspapers as an example of Napoleon's suppression of the liberty of the press; see Semmel, *Napoleon and the British*, 20, 121. Davy also suffered at the hands of the papers in 1824 after a failed attempt to improve the copper-bottoming of ships' hulls: *Morning Chronicle*, 11, 18, 28 October 1824. For more public attacks on Davy see June Fullmer, "Humphry Davy's Adversaries," *Chymia* 8 (1962): 147-64.

"foolish paragraphs"—and decided to accept the prize anyway.[142] When he actually went to France in 1813 (and collected his prize medal), the *Times* sent him on his way with a furious denunciation for his acceptance of safe passage from the enemy. "We should not be sorry," it thundered, "if Buonaparte sent him to keep company with the first victims of Corsican treachery at Verdun [prison]," before lambasting him as mere "*dirty-finger* gentry," (because of his laboratory vocation), whose only achievement had been "making women and children troublesome, by the affectation and babble of knowledge."[143] The *Times* was simultaneously besmirching Davy, denouncing his lectures as fashionable froth, and tarring the social status of chemists. This was too much for the radical journalist Leigh Hunt, who leapt to the defense of the "dignity of chemistry" in his weekly *Examiner*, lambasting the *Times*'s paragraph-writer for his "sturdy vulgarity of ignorance."[144] But Hunt still left room to criticize Davy for his tacit endorsement of Napoleon's patronage and politicization of science, accusing him of thoughtlessly using chemistry as a weak screen for his love of celebrity and desire "to see and be seen" in Paris as he had been in London. The episode must have seemed redolent of the scandal of Banks's fawning letter to the Institut ten years before. Davy's public image benefited a great deal from the British press, but even when the press was being favorable he found he could not

[142] According to Davy's friend Thomas Poole, after receiving the official letter from the Institut in 1808 Davy supposedly remarked that: "Some people say I ought not to accept this prize; and there have been foolish paragraphs in the papers to that effect; but if the two countries or governments are at war, the men of science are not. That would, indeed, be a civil war of the worst description: we should rather, through the instrumentality of men of science, soften the asperities of national hostility." John Aryton Paris, *The Life of Sir Humphry Davy* (London: Colburn and Bentley, 1831), 168. The periodical the *Monthly Repository* came out in Davy's defense against "our wise and patriotic public prints": *Monthly Repository* 3 (March 1808): 167.
[143] *Times*, 19 October 1813.
[144] *Examiner*, 24 October 1813. Hunt was not necessarily the ideal person to have on one's side, however, as he was editing his paper from a prison cell following conviction for seditious libel.

control it.[145] And he was now fair game for the papers' attacks. The newspapers had helped make him a public figure; here was the price.

Conclusion

Davy resigned from his position at the Royal Institution in April 1812, just when his lectures were becoming a definite fixture in the London newspapers. His marriage to a wealthy Society widow, Jane Apreece, finally obtained him the position he coveted as an independent gentleman of science, and he was freed from the slightly demeaning position of having to rely on lecturing for an income.[146] His successor, William Brande, was not known for any brilliant discoveries and did not carry the same attraction for the newspapers. Expressing regret at Davy's departure, the *Caledonian Mercury* told its readers that it now would no longer "give a regular account of these lectures" in the same manner, but would merely "confine ourselves to such facts as may be new, or not generally known." If Aldini stands as a prime example of the British press making news out of a scientific figure in its own way (and not doing that scientific figure much good in the process), Davy's experience was more double-edged: the press's treatment of his lectures burnished his image and represented a new degree of seriousness and detailed engagement with scientific subjects, but he had plenty of frustrations with the newspapers as well. But in the end, Davy's very public elaboration of electrochemistry that echoed through his lecturing activity,

[145] Were Whig "opposition" papers like the *Morning Chronicle* more favorable and more regular in their coverage of Davy and scientific matters in general than "ministry" papers like the *Times*? It seems possible, though I am skeptical of ascribing this to some deep hostility on the part of ministry newspapers to any widely-perceived "radical" associations of the sciences in Britain at this time, as I believe these have been over-stressed. A better explanation might be that Whig-inclined papers would simply be more likely to latch onto Davy because of a general orientation towards subjects concerned with rational enlightened progress, which his lectures fitted rather well.

[146] Davy's career choice serves as yet another instance of the now commonplace observation in the historiography that "professionalization" in the sciences happened relatively late, especially in Britain: see Martin Rudwick, *The Great Devonian Controversy: The Shaping of Scientific Knowledge among Gentlemanly Specialists* (Chicago: University of Chicago Press, 1985); Jim Endersby, *Imperial Nature: Joseph Hooker and the Practices of Victorian Science* (Chicago: University of Chicago Press, 2008).

and his presence in the magazines and the newspapers, opened up new avenues for experimenting with different ways of being a public scientific figure. For the rising generation of men of science in the 1820s and after, his career presented points to emulate—as well as pitfalls to avoid. Nineteenth-century Britain's "public men" of science, from Michael Faraday to Thomas Henry Huxley, were to some extent his heirs in that he showed some possible ways forward, though it was by no means clear where these would lead in 1812. And the generation that followed would still be preoccupied, as Davy was, with where they stood in relation to the powerful, centralized machine of French science, which outlasted Napoleon, its supreme patron. After Napoleon's fall the coverage of science in the *Moniteur* naturally declined as the press liberalized to some extent and began to write about other things. But, with French science still possessing a natural centralized source of news in the activities of the Institut (now rebranded as the Académie Royale des Sciences), the French press retained its serious engagement with scientific matters through the Restoration and on into the July Monarchy.[147]

Scientific practitioners perhaps never again had quite the degree of direct control over the newspapers' message about science as the members of the Institut did under Napoleon. Throughout the nineteenth century they would have to seek out other ways of engaging with the press and asserting their point of view. That century belonged to the newspapers: as yet unchallenged by radio, television, and later by blogs and Twitter, ever-more respectable, and with booming circulations, they were the masters of daily information, and it was no longer tenable to dismiss newspapers as basically irrelevant to the enterprise of science, as Joseph Banks would have done. Now the point was not to keep the newspapers at bay but to get them on board.

[147] Including in the famous 1830 Cuvier-Geoffrey debate; see Toby Appel, *The Cuvier-Geoffrey Debate: French Biology in the Decades before Darwin* (Oxford: Oxford University Press, 1987), 155.

The historical moment I have sought to capture in this chapter stands only at the beginning of a 200-year history of interactions between science and the press that encompasses developments barely even on the horizon at the beginning of the nineteenth century, such as the evolution of specialized science correspondents, organized scientific press releases, and the rise of "popular science" writing. Much of this history is basically unknown to us and remains to be written. But we can expect that running through it all will be the inescapable tension epitomized by the newspapers of Britain on the one hand and Napoleonic France on the other: freedom for the press to do as it likes, versus oversight or control over scientific news by the small intellectual elite who are the experts in the subject under discussion and who can lay claim to be best equipped to decide on what counts as accurate scientific information. This problem is one manifestation of a very deep social tension between expert science and popular democracy which would appear to be a characteristic feature of the modern world.[148] It still faintly haunts scientific practitioners concerned with the public face of what they do. In 2011, writing for the *Guardian* newspaper, the online editor of the leading scientific journal *Nature* criticized a common privilege granted to scientists of checking the copy of news stories related to their work before publication. In response, three scientists mounted a vigorous defense of the practice, suggesting "news stories about science are different from those about politics or business" because of the peer-review process involved in scientific research, and concluding that "public trust in science, and in science reporting, is harmed far more by inaccuracy than by non-independence." The *Nature* editor then drew attention to "nine ways scientists demonstrate they don't understand journalism," and was forcing into stating the obvious: the goals of journalists are not the same as scientists and the aim of science journalism is not simply to communicate suitably-

[148] This tension is a central theme of Daniel Kevles, *The Physicists: The History of a Scientific Community in Modern America* (Cambridge, MA: Harvard University Press, 1995).

vernacularized copies of scientists' public statements.[149] Scientists wish that newspapers would report science better and defer to their expert authority; journalists wish that science would understand their craft and respect their independence. At the start of the nineteenth century, gentlemen of science like Davy carving out new roles on the public stage had to face an earlier incarnation of the tension between elite scientific authority and the free circulation of information in the press. Then, as today, there was no self-evident solution.

[149] http://www.guardian.co.uk/science/blog/2011/sep/29/scientists-copy-check-stories; http://www.guardian.co.uk/science/blog/2011/oct/11/scientists-check-stories-before-publication; http://www.guardian.co.uk/science/blog/2012/jan/17/scientists-journalism.

Chapter 3. Pacchiani's Salt: Experiment and the European Journal System

London at the beginning of August, 1805. Appearances suggest quiescence in the British scientific world. It is summer; the Royal Society will not meet until November; the Royal Institution's fashionable patrons are at their country houses; Humphry Davy is collecting minerals in Ireland; and Napoleon is at Boulogne, less than a hundred miles away, with an invasion flotilla and 200,000 men. Nonetheless, some interesting things are unfolding in one of the local scientific journals, Alexander Tilloch's *Philosophical Magazine*. Wonderful new results are emerging from the conjunction of galvanism and chemistry effected by Volta's pile. Tilloch has just announced that two previously-unknown investigators have performed experiments with the battery that hint at the solution to one of chemistry's great riddles: the composition of muriatic acid.[1] The two discoverers, Pacchiani and Peel, are at opposite ends of Europe –at Pisa, and at Cambridge. They have been laboring in complete ignorance of one another, and their work is printed together for the first time in the latest issue of Tilloch's journal. The editor addresses his readers: "We are on the verge of perhaps more than one important discovery in chemistry. At such a crisis, in particular, every phaenomenon that by even the most remote analogies may appear connected with this inquiry, should be communicated by chemists for the benefit of science. For ourselves, we can only say we shall faithfully discharge the duty which devolves on us, to lay them carefully and correctly before the public."[2]

Whether or not they read Tilloch's appeal, scientific enquirers across Britain, France, Germany, and Italy followed its spirit. Over the next two years, a mass of experimental work and debate on the new discoveries appeared in Europe's nascent scientific journals. "The

[1] Known to us as hydrochloric acid.
[2] *PM* 22 (July 1805): 186.

fermentation which had been thus excited in the chemical world," recalled the man of science Andrew Ure in 1821, "was extreme."[3] The debates turned on a fundamental question: how did galvanic electricity act to transform matter? An answer would have profound implications, not only for galvanism, but for the entire science of chemistry.[4] In this chapter I use the ferment of controversies in galvanism during 1805-06 to investigate the new fast-paced scientific journals that appeared around 1800: publications like Tilloch's *Philosophical Magazine*, the *Annales de Chimie*, and the *Annalen der Physik*. How did these publications work? What kinds of roles did they and their editors fill in the scientific world? How did they interact with other kinds of media? And how did they operate together, as a transnational information system, to shape the large-scale contours of scientific communications in a world divided by war?

In our early-twenty-first-century digital media landscape, scientific journals survive as gatekeepers to accreditation and prestige. Publishing a paper in a journal (print or online) cannot surpass simply uploading it to a preprint server in speed, ease of access, or the *potential* size of the audience.[5] Rather, scientists publish in top journals like *Nature* or *Science* because doing so confers career status, increased visibility (and so a larger *actual* audience), and community endorsement that their work represents original, important, and (provisionally) error-free research. Communication is still part of what journals do, but it is not their primary function.

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[3] [Andrew Ure], review of 6th ed. of Thomas Thomson's *System of Chemistry*, *Quarterly Journal of Science, Literature, and the Arts* 11 (April 1821): 139; for Ure's authorship see Thomson's reply in *Annals of Philosophy*, 19 (April 1822): 241.

[4] Beginning with early surveys like John Bostock's *An Account of the History and Present State of Galvanism* (London: Baldwin, 1818), the muriatic acid controversy has usually been narrated rather cursorily as a kind of misdirected prelude to Davy's famous mature electrochemical work. In his magisterial history of electrochemistry published in 1896, Wilhelm Ostwald treated the episode in some detail, but, secure in the knowledge of how the controversy ended, he did so rather dismissively and condescendingly, as part of a chapter on "Galvanic Phantasies" ("Galvanische Phantasieen"). To someone with a knowledge of modern electrochemistry, some of the positions adopted by participants in the muriatic acid controversy do indeed look strange; the reason, however, is that the field of electrochemistry was the eventual *outcome*, rather than the guiding framework, of these debates. Wilhelm Ostwald, *Elektrochemie: Ihre Geschichte und Lehre* (Leipzig: Veit, 1896), 220-30; translated as Wilhelm Ostwald, *Electrochemistry: History and Theory*, 2 vols. (New Delhi: Amerind Publishing, 1980), 1:213-23.

[5] Notably, proponents of open access publishing criticize subscription-based journals on the grounds that they *prevent* a vast potential audience—the general public—from having access to the latest research.

Around 1800, I argue, this situation was exactly reversed. Journals were not privileged venues for accrediting new science. They were technologies of communication. Forged as commercial ventures within the wider periodical commons, they were intended to solve perceived problems to do with the transmission of information in the scientific world, while simultaneously acquiring influence and income for the editors who ran them. Close cousins of the newspapers, they were improvised, flexible tools for combating the effects of distance and selling access to news and information, often reprinted from elsewhere. When matters of accreditation and trust did arise, they appeared in rather surprising and accidental ways, shaped by how journals happened to be put to use by scientific practitioners as tools of information. Far from being a *solution* to the problem of how to secure good scientific knowledge, the Napoleonic-era journals—so successful as communication technologies—dragged the scientific community into the chaotic and disreputable world of periodical print, re-posing the problem of accreditation in a new venue, sometimes in alarming ways.

Historicizing scientific journals requires some careful thinking about chronology. Periodical print is an old technology, invented by news-sellers around the beginning of the seventeenth century.[6] And it has been linked to the pursuit of the sciences for a long time—famously, ever since the foundation of the *Journal des Sçavans* and the *Philosophical Transactions* in the 1660s. But only in the nineteenth century, as Alex Csiszar has shown, did scientific practitioners fully embrace periodicals as *core* components of the scientific enterprise, so that journals supplanted societies and academies as the primary venues for the formal

[6] It was developed by two constituencies in the European news market: sellers of manuscript newssheets looking to widen their customer base, and purveyors of one-off printed newsbooks and broadsides who discovered that serial or periodical publication could create a cadre of regular purchasers from whom a reliable profit could be generated. Andrew Pettegree, *The Invention of News: How the World Came to Know About Itself* (New Haven: Yale University Press, 2014), 182-207; see also C. John Sommervile, *The News Revolution in England: Cultural Dynamics of Daily Information* (Oxford: Oxford University Press, 1996).

accreditation of new knowledge.[7] And only from about the end of the Second World War to the close of the twentieth century did a certain special kind of printed periodical hold a virtual monopoly on the communication of new science: the modern peer-reviewed journal.[8] Historical accounts written during that period tended to project twentieth-century beliefs about the uses of periodicals in science far back into the past.[9] This seemed convincing because the first learned journals like the *Philosophical Transactions* had appeared at the culmination of the "Scientific Revolution" in the mid-seventeenth century. Since journals appeared indispensable to modern science, and Henry Oldenburg had founded the *Transactions* precisely when modern science itself was thought to have been created, then surely he must have invented "the" scientific journal, in some essential way.[10] Paradoxically, the very fact that periodical publications had been around in the sciences for centuries was used to deny that they had much of a real history at all.[11]

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[7] Alex Csiszar, "Broken pieces of fact: the scientific periodical and the politics of search in nineteenth-century France and Britain" (PhD diss., Harvard University, 2010).

[8] The year 2000, by which time innovations of the early 1990s such as the arXiv.org preprint system and the Web had made a clear impact on scientific communications, marks a symbolic end point for this period.

[9] However, now that this period is (maybe) over, current debates over the digital future of scientific communications and publishing are fostering an increased awareness even among scientists themselves that some features once considered essential parts of scientific communication are of quite recent invention. For example: although proclamations are still commonplace that peer review has been integral to science for centuries, other scientists will now cite the gold-standard historical example of Albert Einstein to show that blind peer review in journals has not been ubiquitous for very long. (Used to German journals like the *Annalen der Physik*, where powerful editors like Max Planck made quick decisions on which papers to publish, in 1936 Einstein objected to the editors of the American *Physical Review* disclosing his work to a shadowy "anonymous expert" without his permission.) See Daniel Kennefick, "Einstein versus the *Physical Review*," *Physics Today* 58 (2005): 43-48, cited as a historical example by http://michaelnielsen.org/blog/three-myths-about-scientific-peer-review/.

[10] For example: "The scientific journal appeared in the seventeenth century, soon after the beginnings of modern science itself, and simultaneously with the origin of the scientific society. The development of science since then has continually been intertwined with the history of the printed journal." Douglas McKie, "The scientific periodical from 1665 to 1798," in *Natural Philosophy through the 18th Century*, ed. Allan Ferguson (London: Taylor and Francis, 1948), 122–32. McKie's survey, published in this commemorative collection marking the 150th anniversary of the *Philosophical Magazine*, is actually quite sensitive to changes in forms and practices of scientific publication across the period he covers, but it sets a pattern in insisting that there exists a coherent history of "the scientific journal" from the 1660s until the twentieth century. See also A.A. Manten, "Development of European Scientific Journal Publishing Before 1850," in *Development of Scientific Publishing in Europe*, ed. A.J. Meadows (Amsterdam: Elsevier, 1980), 1-22.

[11] By the twentieth century, when journals had long been an established part of science, it was natural for such publications to put forward an image of long-term continuity which elided the transformations and messy

New accounts of the rise of authoritative and professionalized scientific journals during the nineteenth century have cut through this mirage of long-term continuity.[12] But this work raises new questions, because it reveals we lack a good understanding of what kinds of things periodicals were *for* in science before they were embraced as trustworthy gatekeepers of scientific knowledge. As a historiographical compass for navigating this unmapped terrain, I borrow a powerful argument from the history of technology, which asserts that the historical role of a technology is best understood not by zooming in on heroic first moments of innovation, but by following the technology's actual uses and consumption by people across time.[13] This steers us away from the trap of an "innovation-centric" history of scientific print, which would, for example, ascribe great importance to Henry Oldenburg's foundation of the *Philosophical Transactions* in 1665 in trying to understand how specialist periodicals and the rapid distribution of printed intelligence became integrated into the pursuit of science.[14] The early *Transactions* did specialize in natural knowledge, it did circulate printed intelligence, and it came out monthly. But it is questionable how important this package of features was in the day-to-day practice of seventeenth-century fields like astronomy or experimental philosophy. Even more damningly for

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contingencies of their past. For example, the *Philosophical Transactions*—a publication with a particularly complex history—simply became, according to its own description, "the world's first scientific journal," which "established the important principles of scientific priority and peer review, which have become the central foundations of scientific journals ever since." (http://rstl.royalsocietypublishing.org/, accessed 28 May 2015.) In fact, during its first half-century the *Transactions* was a shaky operation whose role in the enterprise of natural knowledge was not at all clear. It also folded on several occasions. See Adrian Johns, "Miscellaneous Methods: Authors, Societies and Journals in Early Modern England," *British Journal for the History of Science* 33 (2000): 159-86.

[12] Csiszar, *Broken Pieces of Fact*; Melinda Baldwin, *Making Nature: The History of a Scientific Journal* (Chicago: University of Chicago Press, 2015); Melinda Baldwin, "The shifting ground of Nature: Establishing an organ of scientific communication in Britain, 1869-1900," *History of Science* 50 (2012): 125-54.

[13] See especially David Edgerton, *The Shock of the Old: Technology and Global History since 1900* (London: Profile, 2007), which argues for a wholesale "rethinking of our notion of technological time" (p. xi) by replacing "innovation-based timelines" with histories of use.

[14] This mythic teleology emerges most strongly in popular histories that ascribe to Oldenburg's 1665 journal "the origin of the practice of 'peer review,'" and the invention of "the first modern scientific journal, a platform for international scientific collaboration that defined the template that has been used ever since." Tom Standage, *Writing on the Wall: Social Media—The First 2,000 Years* (New York: Bloomsbury, 2013), 117-23. In reality, looking at Oldenburg's *Philosophical Transactions* explains the long history of periodicals and science about as much as looking only at the first steam locomotives would explain the social effects of railways in the modern world.

an innovation-centered history of journals, the model did not catch on.[15] In 1765, for example, there was no periodical of any real importance combining these features anywhere in the world.[16]

Focusing on the *uses* of periodical print reveals that fast-paced specialist scientific journals began to have a long-term impact on the practice of science and its community structure around the end of the eighteenth century. Around 1800, a cluster of newly-founded independent monthly journals in France, Britain, and Germany, self-consciously linked to and drawing on one another, emerged as a transnational system dedicated to sharing new scientific information. Crucially, these journals grew up in a symbiotic relationship with the experimental practice of the period's fastest-moving scientific fields, galvanism and chemistry. This symbiosis was in the *kinds* of new things people had to communicate (experiments), the *ways* in which those results were communicated (relatively short descriptions), the *speed* at which men of science felt this communication should happen (as fast as possible), the intended audience (a community of experimenters across Europe), and the subsequent *uses* which were made of these communications once printed (especially harnessing them in cumulative fashion to make more experiments). As dealers in this information, journals claimed to represent a month-by-month picture of the cutting-edge of scientific progress. They even started to shape the idea of scientific progress in their own image, so that progress was increasingly defined by the evolving narrative

[15] This is only really surprising if we think that because journals are now extremely important in science, they must have been important right from the moment that the first periodicals dealing with science were invented. There is no reason that this should be true. "In use-centred history," writes David Edgerton, "technologies do not only appear, they also disappear and reappear, and mix and match across the centuries." Edgerton, *Shock of the Old*, xii.

[16] As explained in Chapter 1, the predominant eighteenth-century serial format was the academic memoir collection, like the post-1752 *Philosophical Transactions* or the *Mémoires* of the French Académie des Sciences, and these were not periodicals, but serials issued slowly and irregularly and containing no news or intelligence. In addition, there were the various learned journals of the Republic of Letters; these functioned as important sources of news for people investigating natural knowledge, but they dealt in intelligence about the learned world as a whole—especially news or reviews of books—rather than specialist scientific information. There were, of course, many other periodicals published during the eighteenth century that contained varying amounts of content related to scientific, technical, and medical subjects, but the importance of any given title for the larger community of practitioners of science appears to have been small; for these publications see David Kronick, *A History of Scientific and Technical Periodicals: The Origins and Development of the Scientific and Technical Press, 1665-1790* (Metuchen, NJ: Scarecrow Press, 1976).

of discovery the journals presented. If you wanted to be a part of certain sciences, galvanism and chemistry above all, you really *had* to read journals. Not to have regular access was to retreat from the active community of experimental research. For some kinds of science, almost to everyone's surprise, journals became seemingly indispensable.

Nonetheless, the ongoing war frequently disrupted this nascent information-sharing system. War created fractures in the European scientific news world precisely at the moment when the journals were assuming important new roles in the day-to-day lives of men of science. But in fact this actually raised the status of journals and the demand for them within the scientific world: because travel between certain countries was difficult or impossible, mail routes uncertain, and foreign journals (and books) so hard to obtain, for many people a scientific journal became an essential window onto developments abroad. The wars of 1792-1815 presented many problems and frustrations for journal editors. But by looking through their eyes at the kinds of work they did, we will see how war was also their opportunity to make themselves useful.

Rozier's Dream: Spreading the News of Discovery

What publications counted as scientific journals? The question is a slippery one.[17] Across the eighteenth century, a diverse range of periodicals were connected in various ways with the sciences and other areas of intellectual life, from learned journals of the Republic of Letters like

[17] It has troubled bibliographers who had a professional need to sort and classify science-related publications from the seventeenth and eighteenth centuries. The solution adopted was to sweep all kinds of serials or periodicals with some appreciable level of "scientific" content—from the *Mémoires* of the Académie des Sciences to obscure and short-lived agricultural magazines—into a catch-all category of "scientific and technical periodicals," or just "scientific journals." See especially David Kronick, "Studies of the Early Scientific Journal: The Basic Source Lists," in *"Devant Le Deluge" and Other Essays on Early Modern Scientific Communication* (Metuchen, NJ: Scarecrow Press, 2004), 27-39; also Kronick, *A History of Scientific and Technical Periodicals*. From a bibliographic perspective, this made sense, since, as Kronick puts it, the aim was "to collect as many of the various species of communications formats as possible." Unfortunately, such useful bibliographic research appears to have resulted in hugely inflated estimates of the number of scientific journals in existence around 1800. One survey makes the extraordinary claim that 750(!) scientific journals existed in 1800—a figure that would have baffled men of science living at the time: Manten, "Development of European Scientific Journal Publishing Before 1850," 1.

Pierre Bayle's *Nouvelles des Republique des Lettres* or the early *Philosophical Transactions*, to general-readership publications like the British *Gentleman's Magazine*, to (especially in Germany) various special-interest periodicals dealing with subjects like medicine and agriculture.[18] The ways these publications operated and were used by people involved with the sciences varied widely. Additionally, as we saw in Chapter 1, there were the memoirs of academies and societies (after 1752, the *Philosophical Transactions* joined this class), which were not periodicals (or journals) at all, but serials, appearing infrequently and often irregularly. Looking at this variegated European print landscape during (say) the 1760s and demanding to know which titles were scientific journals would be like trying to identify who was a communist during the French Revolution, or who was a biologist in the seventeenth century. The question is freighted with anachronism because it forces a category that only hardened later onto the culture of the past.

But by 1805 the question could have been meaningfully asked and answered. Strikingly, the first use of the term "scientific journal" in English—in print at least—dates from 1797.[19] While making too much of this fact in isolation would be excessively nominalist (and Anglocentric), it is indicative of a broad shift in the uses of periodical print in the sciences

[18] See Anne Goldgar, *Impolite Learning: Conduct and Community in the Republic of Letters, 1680–1750* (New Haven: Yale University Press, 1995), 54-114; Thomas Broman, "Periodical literature," in *Books and the Sciences in History*, ed. Maria Frasca-Spada and Nick Jardine (Cambridge: Cambridge University Press, 2000), 225–38.

[19] A search of Google Books gives the first occurrence as a review mentioning "the German scientific journals" in the *Monthly Review* 22 (1797): 572; this is also the first use cited by the *OED*. *Nicholson's Journal* has been founded in London earlier the same year, and the singular form, "scientific journal" appears first in a review of Nicholson's publication in *Monthly Review* 29 (July 1799): 304. Notably, this is around the same time the words "scientific" and "science" were starting to be used in an exclusive sense, to refer particularly to the natural sciences—in phrases like "man of science," "scientific discovery," and so on; this raises the suggestion that the notion of a *scientific* journal only crystalized with Cunningham and Williams's "invention of science" around 1800: Andrew Cunningham and Perry Williams, "De-Centring the 'Big Picture': 'The Origins of Modern Science' and the Modern Origins of Science," *British Journal for the History of Science* 26 (1993): 407–32. Of course, the word "journal" had long been applied to learned periodicals in the seventeenth and eighteenth centuries, including Oldenberg's *Philosophical Transactions* and, naturally, the *Journal des Sçavans*. In this context it carried the connotation of "newssheet," a sense which it retained through the Napoleonic period and into the early nineteenth century.

around the end of the eighteenth century. Although the category remained a little fuzzy for years afterwards, by 1805 there was a small core group of publications—around ten in number—that the European scientific community would have identified as "the journals" of science.[20] These publications were young: with one exception (the *Journal de Physique*) all founded after or during 1789. They were commercial ventures, generally run by independent-minded editors looking to increase their standing in the scientific world.[21] They were monthly publications, and unlike the eighteenth-century learned journals, they were concerned specifically with the natural sciences rather than knowledge in general.[22] Their editors, whose names were known across the European scientific world, saw themselves as forming an international group carrying on similar projects. Their common purpose was to spread information about new discoveries.[23] The journals were fashioned as rough-and-ready working responses to the problem of communication, designed to transmit new science to people who needed it, and who would pay for it. They operated as flexible, customizable information technologies to mitigate the tyranny of distance and dispersal that was inherent to the pursuit of science in the Napoleonic world.

[20] When, for example, Humphry Davy recorded during his Continental Tour that he had "seen no scientific journals" since crossing into Italy, he clearly had in mind a specific and limited set of monthly publications that he was missing out on. Davy to Pictet, 26 May 1814, *Pictet Corr.* 3:167. Here is one possible list someone like Davy might have compiled in 1805 if asked to name "the scientific journals." France: *Annales de Chimie*, *Journal de Physique*, *Bibliothèque Britannique*, *Bulletin des Sciences*. Britain: *Nicholson's Journal*, *Philosophical Magazine*. Germany: *Annalen der Physik*, *Neues Allgemeines Journal der Chemie*. We might also add Jean-Baptiste Van Mons's *Journal de Chimie* (Brussels), and Luigi Brugnatelli's *Annali di Chimica*, although the former ceased publication in 1805 and the latter issued only an isolated volume in that year (having not published since 1802).

[21] The *Annales de Chimie* is the major exception to the independent editor model: it was run by a committee of the Institut's chemists.

[22] And, in some cases, the technological "arts" as well.

[23] On the role of the new British journals as "international anthologies of scientific discovery" see Jonathan Topham,"Anthologizing the Book of Nature: the circulation of knowledge and the origins of the scientific journal in late Georgian Britain," in *The Circulation of Knowledge between Britain, India, and China*, ed. Bernard Lightman and Gordon McOuat (Leiden: Brill, 2013), 119-52. While Topham emphasizes the common anthologizing function of a range of British publications, from the *Philosophical Magazine* to annual "retrospects" of discovery, here I stress commonalities between the leading *monthly* journals in Britain, France, and Germany, and in particular the temporal aspects of the ways they handled information, with quick turnarounds, reprints from one another, and a mission to be first into print with important scientific news.

In 1771 the Abbé François Rozier had launched the first of the new monthly journals, the *Observations sur la Physique* (subsequently the *Journal de Physique*).[24] It was addressed to "true men of science" ["*les vrais Savans*"], to solve problems of communication between them. He declared in 1773: "The purpose of this Work is to announce the discoveries made daily in the sciences." The rapid expansion in scientific investigations, particularly in "experiment and observation," meant that discoveries were happening all the time—new chemical substances and processes, new manufacturing techniques, new electrical experiments. But, Rozier thought, these discoveries were not reaching other scientific enquirers quickly enough. The eighteenth century had seen a dramatic growth in science through the foundation of academies and learned societies, yet while new discoveries could be quickly announced at academy meetings, information about them was only distributed further afield much later, in the printed collections of "Memoirs" or "Transactions." "The majority of these academic collections," Rozier lamented, "are published in their national language, and printed several years after the Memoirs have been read. During this time we remain ignorant of facts which could be of the greatest utility for the sciences." Academies were excellent at promoting science in particular places, but it appeared they did so at a cost to the international flow of the facts of discovery. "It seems," Rozier wrote, "that as the number of savants has increased, the correspondence between those of different countries has decreased."[25] Distressingly, researchers in different parts of Europe might be working on the same questions and yet be unaware of each other's work. "I therefore sought to bring the

[24] James McClellan, "The Scientific Press in Transition: Rozier's Journal and the Scientific Societies in the 1770s," *Annals of Science* 36 (1979): 425-49. Although Rozier began publication of the *Observations* in 1771, his project as described here really dates from the new series of the journal he started in 1773.
[25] "Avis," *Observations sur la Physique* 1 (1773), iii-vi. Rozier was nonetheless concerned to run his journal alongside rather than in competition with existing institutions; he was particularly careful to keep up good relations with the powerful Académie des Sciences.

sciences together in a common center," he told one correspondent, "a general depository, where everyone should have the opportunity to take credit for his work and make himself known."[26]

"This journal has no resemblance to the periodical publications that are so widespread in France and in foreign countries," Rozier declared.[27] Of course, his publication deployed techniques honed in a century's worth of periodical print, including the journals of the Republic of Letters, and it recapitulated aspects of defunct projects such as Oldenburg's original monthly *Philosophical Transactions*. But what matters is not the originality of Rozier's model but how it was taken up and used by the scientific world. His vision of a transnational "common center" for the exchange of specialist reports of discoveries, located in the realm of print rather than rooted in any physical location, informed later ventures both in France and abroad. Subsequent journal editors were the inheritors of Rozier's dream of perfect information in science.

Consequently, they voiced complaints very similar to his about the slowness and failures of information transmission.[28] Of course, it was natural to proclaim defects in scientific communications in need of remedy when launching a new periodical, but these statements did reflect a real and chronic anxiety that the movement of information was not keeping pace with an expansion in new discoveries. And it was the *experimental* sciences, and above all chemistry, the great scientific success of the late eighteenth century, which formed the new journals' focus and

[26] Rozier to P. Wargentin, 8 August 1775, quoted and translated in McClellan, "Rozier's Journal," 445.
[27] "Avis," *Observations sur la Physique* 1 (1773), iii.
[28] For example, the *Avertissement* to the first volume of the *Annales de Chimie* (1789) declared that, although chemistry had never been pursued more actively, "it will progress at a still greater pace" when "the savants of several countries have a greater facility for somehow making a reciprocal exchange of their discoveries." William Nicholson in 1797 announced his new journal would mitigate problems of communication, speaking of how "all the expanded sources of philosophical intelligence" were confined to an "extreme few," and complaining of "the very limited circulation of academical Transactions." A "want of such general communication," he claimed, had caused "a numerous catalog of errors retained in the works of authors of the first eminence." Preface, *NJ* 1 (April 1797). Launching the *Philosophical Magazine* in 1798, Alexander Tilloch lamented that despite "the present enlarged state of Science, the Public in general have very little opportunity of being early acquainted with the improvements which are daily made in the different branches of human knowledge." Prospectus in *Star*, 29 June 1798, 1.

fuelled their growth.[29] A science with devotees scatted all over Europe—from the Edinburgh medical man, to the German professor, to the Swedish mining official, to the French provincial apothecary—chemistry provided the journals with a broad base of readers, making them more viable business ventures. "Chemistry embraces so vast a field, and is cultivated by so numerous a tribe of votaries," remarked *Annals of Philosophy* editor Thomas Thomson in 1815, "that its annual progress is exceedingly striking and rapid."[30] This tribe of chemical votaries produced a large number of new results to communicate, and these experimentally-derived and fact-based discoveries were usually of a kind easily and compactly summarized in journals. Chemistry and periodical print suited one another.

Several new journals were devoted entirely to chemistry. One early imitator of Rozier was Lorenz Crell, who founded his pioneering German chemical journal, the *Chemische Annalen*, in 1778. Crell aimed to mitigate the decentralized geography of the German scientific world. At his base in Helmstedt in the Duchy of Brunswick, he received information coming in from Central Europe's scattered chemists, processed it into print, and sent the cumulative package out to them again each month. As Karl Hufbauer has shown, this was a crucial step in binding German chemists into a disciplinary community, so that they addressed their work to fellow chemists rather than the larger Republic of Letters.[31] A decade later, in 1789, the *Annales de Chimie* was founded by Lavoisier and other French chemists—in part to counteract the influence of *Rozier's Journal*, now edited by an opponent of Lavoisier's new oxygen chemistry,

[29] Before founding their own *Annales de Chimie*, Lavoisier and other major figures of late-eighteenth-century French chemistry used Rozier's journal as a quick way to make reports of their work available, and reprinting in Rozier's publication was also important for disseminating the results of British pneumatic chemists in France; see Henry Guerlac, *Lavoisier: The Crucial Year* (Ithaca: Cornell University Press, 1961), esp. 59-71.
[30] *Annals of Philosophy* 5 (January 1815): 2. Thomson had earlier noted how "like all other journals of the present day, our *Annals* must contain a greater proportion of Chemistry, which is making a rapid progress, than of those sciences which are in a great measure stationary." "Advertisement" to *Annals of Philosophy* 1 (1813), iv.
[31] Karl Hufbauer, *The Formation of the German Chemical Community, 1720-1795* (Berkeley: University of California Press, 1982), esp. 62-82.

Jean-Claude Delamétherie.[32] Other journals had a broader purview but a strong chemical focus, as with *Nicholson's Journal*, founded in 1797 with *Rozier's Journal* as a clear model.

Both galvanism and chemistry therefore featured prominently in the new monthly journals, with the conjunction of the two fields that followed the advent of the battery renewing the excitement of chemical discovery just when the novelty of pneumatic chemistry and the French overthrow of phlogiston was fading.[33] In the wake of Volta's remarkable discovery, there had never been greater demand for media that aspired to give readers, as Rozier put it, "the current picture of scientific knowledge."[34] Since science was considered to be cumulative—discoveries bred further discoveries—the idea was that journals would not only make information more readily available, but actually serve to hasten the progress of human knowledge. And gradually, with men of science making more and more use of them as readers and writers, the movement of information in these new journals did indeed begin to alter the rhythm and tempo of scientific life.

The Elusive Muriatic Acid

In May 1805 an Italian named Francesco Pacchiani announced he had made a profound discovery using the galvanic battery.[35] The riddle he claimed to have solved was the chemical composition of common salt. In a sense, of course, chemists *knew* what salt was. It was muriate

[32] Maurice Crosland, *In the Shadow of Lavoisier: The Annales de Chimie and the Establishment of a New Science*, (British Society for the History of Science, 1994). Rozier had retired from editorial duties in 1780—he was later killed during the siege of Lyon in 1793—leaving the journal to his nephew, Jean-André Mongez. Mongez left Delamétherie in charge of the journal when he sailed on Lapérouse's doomed round-the-world expedition in 1785; after the expedition vanished somewhere in the Pacific, Delamétherie acquired the journal by default. Escaping his predecessors' propensity to meet sudden and violent ends, he ran it until his death in 1817 at the age of 73.

[33] "Chemistry," reflected Nicholson in 1813, had "a second time, within our own observation, become a new science by the discovery of the effects of galvanism on its processes, and the development of elementary substances of higher simplicity than were before known." *NJ* 36 (1813): 388.

[34] "Avis," *Observations sur la Physique* 1 (1773), iv.

[35] For another view of the muriatic acid controversy, concentrating especially on the Italian reception of Pacchiani's work, see Ferdinando Abbri, "Il Misterioso 'Spiritus Salis': Le Ricerche di electrochimica nella Toscana Napoleonica," *Nunicus* 2 (1987): 55-88.

of soda: neutralize muriatic acid with the alkaline caustic soda, evaporate, and you would find salt crystals. The mystery lay with the composition of the acid and the alkali (we would now call them hydrochloric acid and sodium hydroxide).[36] The question of what these substances were made of had been left unanswered in Lavoisier's new system of chemistry, articulated in his *Traité Élémentaire de Chimie* of 1789. Lavoisier had asserted that both were compounds, but was able to give no direct proof.[37] Muriatic acid was a particular embarrassment: the French theoretical system *insisted* it must be a compound containing oxygen (Lavoisier's "principle of acidity" as well as the supporter of combustion), but no one had convincingly managed to break down the acid to prove or disprove this.[38] Both as a test of Lavoisier's ideas and because the acid was an essential laboratory reagent with important uses in manufactures, decomposing muriatic acid would be a great achievement: as Cuvier put it, it was "undoubtedly one of the most important questions that still remain to be resolved in chemistry."[39]

Pacchiani, the new Professor of *Física Teórica* ("natural philosophy") at Pisa, was as yet a complete unknown on the European scientific stage.[40] Meeting him in Italy fifteen years later, the young Mary Shelley cloaked him in a mantle of gothic glamour: "Il Diavolo Pacchiani," as the Pisans supposedly called him, with "the highest mind, a profound genius, and an eloquence

[36] This is a translation into our terminology, not an exact equivalence. "Soda" for chemists in 1805 was not a precise term—it might mean "caustic soda" or "carbonate of soda"—and because solid sodium hydroxide readily absorbs water from the air, even an early-nineteenth-century chemist's "caustic soda" would probably not have been quite the same as a modern laboratory's pure and dry sodium hydroxide.
[37] Their identification as compounds was based on the strength of analogy with other acids and alkalis which *had* been decomposed. Lavoisier's table of simple substances listed the "muriatic radical" in muriatic acid as an "unknown" simple substance, and implied soda was a compound by not listing it in the table. On the problem of muriatic acid see H.E. LeGrand, "Ideas on the composition of muriatic acid and their relevance to the oxygen theory of acidity," *Annals of Science* 31 (1974): 213-25.
[38] The oxidation product of muriatic acid was termed "oxymuriatic acid," i.e., denoting a compound of muriatic acid with oxygen. But in 1811 Davy asserted that oxymuriatic acid (more strictly, oxymuriatic acid gas) was not a compound at all, but an elemental "simple body." We now know it under the new name he coined, chlorine.
[39] *Moniteur*, 6 Messidor an 13 (25 June 1805).
[40] Originally from Prato, Francesco Giuseppe Maria Pacchiani (1771-1835) had been ordained a priest at the age of 20; after studying at Pisa he was appointed to the university's chair of logic and metaphysics in 1801 before later transferring to the chair of natural philosophy. For a brief biography, see the *Dizionario Biografico degli Italiani*.

that transports" (she later revised this to an opinion much less favorable).[41] Cynical, world-weary, and having long since withdrawn from scientific pursuits when Shelley encountered him, in 1805 the 34-year old Pacchiani was confident he was on the verge of winning scientific fame, and Napoleon's Prix du Galvanisme.[42] His claim was simple: using galvanic electricity, he had managed to synthesize muriatic acid from pure water.

Many of the people who read about Pacchiani's experiments in the scientific journals had trouble making sense of the precise details of what he had done. There were several reasons for this: the announcement of the experiments in three separate parts, their uneven propagation in the journals, the lack of a precise language for describing galvanic manipulations, and (most seriously) Pacchiani's own rather opaque mode of expression. But his key experiment went like this.[43] He placed a glass tube, stopped with wax at the top end, upright in a trough filled with distilled water. A gold wire, connected to the positive side of a galvanic pile (in our terms, an electrode) passed through the wax into the distilled water in the tube. Pacchiani closed the lower end of the tube with a piece of linen or taffeta, forming a barrier between the water in the tube and the water in the trough. This barrier was a key feature of Pacchiani's experimental arrangement, which was otherwise a variation on the standard setup for the decomposition of water. The barrier divided the circuit in two—another way of manifesting the strange

[41] Mary Shelley to Leigh Hunt, 3 December 1820, in *The Letters of Mary Wollstonecraft Shelley*, ed. Betty Bennett (Baltimore: Johns Hopkins University Press, 1980), 1:165-166. The character of Benedetto Pepi in Mary Shelley's novel *Valperga* (1823) was probably inspired by Pacchiani. Channeling contemporary English ethnic stereotypes of sinister Italians, another member of the Shelleys' travelling party, Thomas Medwin, described him as "somewhat above the common height, with a figure boney and angular, and covered with no more superfluous flesh than a prizefighter. His face was dark as that of a Moor, his features marked and regular, his eyes black and gloomy." Medwin further commented, in a reference to the villain in the Gothic novel *The Italian*, that he "might have sate to Anne Radclyffe for a Schedoni," rounding off the portrait by proclaiming that Pacchiani was "admirably calculated for a spy." Pacchiani seems to have had a scandalous reputation; Medwin described a fondness for "midnight orgies, which he was in the habit of celebrating with some of the most dissolute of the students." Thomas Medwin, *Life of Percy Bysshe Shelley* (London: Thomas Cautley Newby, 1847), 2:54-60.

[42] Pacchiani freely acknowledged the Prix du Galvanisme as a motivation in printed accounts of his work.

[43] I base my description especially on Pacchiani's first and third *opuscoli* (cited below).

phenomenon of the separation of the two wires during the decomposition that had troubled Nicholson and Carlisle.

To complete the circuit a strip of moistened paper connected the negative side of the pile with the water in the trough. Pacchiani reported the following observations. 1) Oxygen given off at the gold wire, and collecting at the top of the sealed tube. 2) A smell of oxymuriatic acid (in our terms, chlorine). 3) The liquid in the tube acquired a golden color. 4) This liquid gave a cloudy precipitate when tested with nitrate of silver (the traditional test for muriatic acid). His logic: since the only substance in the reaction tube had been distilled water, and since oxygen was given off at the gold wire in the tube, and muriatic acid and oxymuriatic acid were formed (together with some sort of gold compound, presumably muriate of gold), then some of the water must have *lost oxygen and transformed into muriatic acid and oxymuriatic acid*. Conclusion: both muriatic acid and oxymuriatic acid must be oxides of hydrogen! As he put it: "Hydrogen does not have a single degree of oxygenation, but many. One of these is water, below that is oxygenated muriatic acid, and below oxygenated muriatic acid is another, which is muriatic acid."[44]

Pacchiani put his claims into circulation in a way which may appear rather archaic: a series of three printed pamphlets in Italian ("*opuscoli"*), framed as letters to senior learned colleagues.[45] Had he been working in (say) Manchester, Jena, or Paris, he might have chosen to announce his discovery in one of the monthly scientific journals. But at this time there were no

[44] *Lettera del dottor Francesco Pacchiani professor di fisica nell'Università di Pisa al chiarissimo sig. auditore Lorenzo Pignotti* (Pisa: 9 May 1805), 7.

[45] *Lettera del dottor Francesco Pacchiani professor di fisica nell'Università di Pisa al chiarissimo sig. auditore Lorenzo Pignotti* (Pisa: 9 May 1805); *Lettera del dottor Francesco Pacchiani, professore di fisica, nell'universita di Pisa all'illustriss. sig. Giovanni Fabbroni* (Pisa: 10 June 1805); *Lettera del dottor Francesco Pacchiani, professore di fisica nell'Università di Pisa, all'illustriss. sig. Giovanni Fabbroni* ([Florence?]: 9 July 1805). Only the first and third of these were reprinted in English, in the *Philosophical Magazine*: *PM* 22 (July 1805): 178-83; *PM* 24 (April 1806): 176-80. Copies of the original *opuscoli* and other Italian pamphlets published in response to Pacchiani's work are very rare. The Othmer Library of the Chemical Heritage Foundation, Philadelphia, probably contains the best collection outside Italy, with nine pamphlets related to the episode bound together.

such publications south of the Alps, and their specialist culture of scientific information was still rather alien to the Tuscan learned community.[46] In Pacchiani's world, the sciences and humanistic letters were pursued alongside one another in the broader model of intellectual life characteristic of the Enlightenment Republic of Letters; the addressee of his first pamphlet, his mentor Lorenzo Pignotti, had previously held Pacchiani's chair in natural philosophy, but had switched jobs to become court "historiographer."

The first pamphlet announcing the discovery was printed at Pisa around 9 May. Two more followed in June and July, containing further experimental details and arguments. They were soon reprinted in a learned periodical run by the Pisan men of letters, the *Nuovo Giornale dei Letterati,* and their claims were hotly debated in Italian learned circles.[47] But if Pacchiani's work was to make any impact beyond Italy, someone needed to go to work transmitting the news. Pisa in 1805 was part of the Kingdom of Etruria, one of Napoleon's short-lived experiments in European statehood. Rearranging minor sovereigns like pieces on a chessboard, Napoleon had conjured Etruria into existence in 1801 out of the former Grand Duchy of Tuscany, packing off the ex-Grand Duke to become Elector of Salzburg so he could give the Spanish Bourbons compensation for deposing a family member, the Duke of Parma. Factious Etrurian court politics kept the Kingdom officially neutral, and so the Tuscans were rather exceptional in the Italian peninsula in having open trade and postal connections with both France and, through the important port of Livorno, Britain.[48]

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[46] Luigi Brugnatelli's *Annali di Chimica*, published at Pavia, was a specialist publication—founded in 1790, it was clearly inspired by the *Annales de Chimie*—but the last volume had come out in 1802. Later in 1805, Brugnatelli published an isolated volume which did contain reprints of Pacchiani's *opuscoli*: *Annali di chimica* 22 (1805): 125-56. In 1808, he began the new bi-monthly *Giornale di fisica, chimica e storia naturale.*

[47] See Abbri, "Il Misterioso 'Spiritus Salis'"; *Nuovo Giornale dei Letterati* 2 (1805): 368-75, 416-25; 3 (1805) : 194-99.

[48] The first Etrurian King, the son of the ex-Duke of Parma, had reigned only two years before promptly dying at thirty and leaving the throne to his three-year-old son. His wife, Maria Luisa of Spain, presided as Regent over a moderately reactionary court characterized by struggles between pro- and anti-French factions. She aimed at a rather

To spread the news, Pacchiani turned to an influential mentor with a European reputation, Giovanni Fabbroni, one of Italy's most eminent savants.[49] Fabbroni was quickly convinced by Pacchiani's "beautiful and useful discovery"—which he actively promoted in Italy.[50] His first move, however, was to transmit copies of Pacchiani's first pamphlet abroad. In Florence on 15 May, with the pamphlet just off the press, he sat down to write several letters.

Information Aggregators

Seven hundred and fifty miles away and two months later, Alexander Tilloch was assembling material for the next issue of his scientific journal, the London *Philosophical Magazine*. It was a familiar process, one which he and fellow English, French, and German journal editors like Nicholson, Delamétherie, Pictet, Gilbert, and Gehlen performed month after month. Tilloch had been doing it for seven years already; he would continue for another twenty. Making up the *Magazine* and getting it to press was as much a part of the rhythm of life as the change of the moon or the tides on the Thames. Come winter or summer, come war or peace, come a change of government or Napoleon's legions at Dover or anything short of the worst of all evils, bankruptcy, the *Philosophical Magazine* would still be ready for sale, 96 fresh-printed pages primed with "as early an Account as possible of everything new or curious in the Scientific World, both at Home and on the Continent," on the last day of every month.[51]

By Tilloch's standards the pace of assembling the *Magazine* was relaxed—a slow-motion version of something he did every day. Alone among his fellow scientific journal editors, he was

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perilous balancing act between the two, trying to placate France while remaining neutral in the war and carrying on trade with Britain. The continuation of ties to Britain led to the extinction of her Kingdom in 1807, when Napoleon finally lost patience and annexed it to France. See Michael Broers, *The Napoleonic Empire in Italy, 1796-1814: Cultural Imperialism in a European Context?* (New York: Palgrave, 2005), 77-80.

[49] Fabbroni was broadly pro-French, as Pacchiani seems to have been, and worked actively in the Napoleonic regime after the annexation of Etruria.

[50] Fabbroni to Count Niccolò da Rio, 18 May 1805, printed in *Giornale de Italiana Letteratura* 9 (April 1805): 97.

[51] Preface, *PM* 1 (1798): iii. For "the last day of every month" see advertising in *Star*, 30 June 1798, p. 2.

also in charge of a daily newspaper, having become editor and part owner of a major evening title, *The Star*, in 1789, soon after moving to London from his native Glasgow.[52] All editors of journals had to be able to manage the business and technology of print effectively, but Tilloch was a print-shop native, having spent several years in the 1780s pioneering stereotype printing.[53] Denied membership of the Royal Society because he edited a scientific journal (Banks no doubt fearing he would print any intelligence heard at meetings), Tilloch socialized with other men of science in alternative venues like the Askesian Society, a City of London group with a largely Dissenting membership that met regularly for experiments and discussion.[54] Journalist and tradesman, he was not the sort of man welcomed in Joseph Banks's library, and his interactions with the resident intelligence broker there, Sir Charles Blagden, appear to have been virtually nonexistent.[55]

Scientific journals in 1805 were composite copy-paste productions, put together out of a mass of texts gathered from near and far, some receiving their first printing in the journal, others reprinted from elsewhere. Each issue was the latest in an ongoing serialized process: readers were aware there had been an issue a month before and that there would be one the next month as well. Like the newspapers, but at a slower tempo, issues were freeze-frame snapshots of the state of information about the world gathered at a particular place and time. Thus the impact and meaning of information was relative: the key was not how new or original particular texts or

[52] Biographical details on Tilloch from *DNB*, and an obituary in *Gentleman's Magazine* 95 (1825): 276–81; see also W.H. Brock and A.J. Meadows, *The lamp of learning: Taylor and Francis and the development of science publishing* (London: Taylor and Francis, 1998), 89-109.
[53] In line with Tilloch's practical background, the *Philosophical Magazine* (like *Nicholson's Journal*) included numerous "practical articles, which may be serviceable to Artists [i.e. artisans], Manufacturers, &c.," as well as "new Machines and new Mechanical Inventions." Prospectus, *Star*, 29 June 1798, 1.
[54] It is unclear when precisely Tilloch's failed bid for Royal Society membership occurred; it seems his editorship of a daily newspaper (a very disreputable occupation) was also a factor in his rejection. On the Askesian Society milieu see Ian Inkster, "Science and Society in the Metropolis: A Preliminary Examination of the Social and Institutional Context of the Askesian Society of London, 1796-1807," *Annals of Science* 34 (1977): 1-32.
[55] Tilloch is conspicuous by his absence in the pages of Blagden's diary. Blagden did, however, read the *Philosophical Magazine*.

pieces of intelligence or texts were in absolute terms, but whether they were new to the journal's readers. Every issue represented, in the editor's judgment, the most novel and interesting scientific information—"a variety of matter collected at great expense, and with much labour"—available to him during the past month at his particular location; it was a projection into print of his personal perspective on the scientific world.[56] The business therefore depended on editors having good access to foreign and domestic scientific information.[57] That invention of the newspapers, periodicity, was used to sell a service: a journal connected readers to a common channel of intelligence, guaranteeing a monthly influx of fresh scientific reports, date-stamped bulletins of progress. On this channel, new experiments, observations, or discoveries from far away (or close to home) could be received. Through letters back to the editor, they could also be transmitted.

In mid-July 1805 Tilloch was busy assembling material for the eighty-sixth issue of his *Magazine*, which would need to finish printing a day or two before the end of the month.[58] A key "source of information" (his term), was the Continental journals—especially French.[59] But he seems to have had difficulty obtaining these during the first half of 1805, because in July the latest he had available was a six-month-old issue of the *Journal de Physique* dated the end of December 1804 (he had received this back in April; next month he would receive a much more recent *Journal de Physique* dated May 1805). For his July issue, he also lifted material—on oxides of mercury, and a compound for silvering mirrors—from earlier issues of the *Journal de*

[56] Quote from Tilloch's Prospectus, *Star*, 29 June 1798, 1.
[57] Fortunately, being a well-known journal editor meant that people who wanted to broadcast information would send it to you.
[58] This section is based on a reading of the *Philosophical Magazine* for July 1805 (vol. 22), and the surrounding months. The 96-page July 1805 issue consists of six sheets of letterpress, printed on both sides and folded in octavo; it has 11 articles, with an average length of 6.5 pages, followed by a further 20 pages of shorter pieces of news and intelligence.
[59] Prospectus, *Star*, 29 June 1798, 1.

Physique and the *Annales de Chimie*.[60] Much more up-to-date was Tilloch's copy of the *Moniteur* of 25 June, which contained a long report by Cuvier on the activities of the Institut National for the past year. Other material in the journal was culled from the wider periodical commons, Tilloch using whatever came to hand containing something new and interesting. This month it was a letter sent from the remote Kamchatka peninsula in the Russian Far East, reporting on Captain Kruzenstern's expedition in the Pacific and originally printed (in German) in a St Petersburg periodical. Not for nothing did the *Magazine* carry a Latin epigram from the sixteenth-century humanist Justus Lipsius on the virtues of compilation: "As the spider's web [Latin: textus] is no better because it spins its own thread, our own [web/text] is none the worse because, like bees, we draw our material from others."[61]

But some of the information in Tilloch's journal was also sent to him directly, in letters from people wishing to announce work through the *Magazine*. This month he had received something particularly exciting. It was a follow-up to a previous communication; back in April, Tilloch had received a short letter from a "W. Peel" writing from Cambridge. Peel had announced an experiment in which muriate of soda was formed by the action of galvanism on pure water—later interpreted as similar to Pacchiani's formation of muriatic acid, with the additional formation of the alkali soda at the positive wire. "The salt could not have been contained in the water before the experiment," Peel asserted, "because I used every precaution to have it free from impurities." Peel was a person entirely unknown to Tilloch, but that did not matter; many contributors to the *Philosophical Magazine* were unknown. Tilloch had printed the

[60] Of course, it is possible that Tilloch did have a later issue of one of the French journals but chose not to reprint from it because he did not think the contents particularly novel or interesting; this seems unlikely, however.
[61] "Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." The implication is that bees draw nectar from many flowers to make honey; this was a classical trope, drawn from Seneca, on the merits of compilation. Though Tilloch's background was in newspaper journalism, which had its own quite separate traditions of text-aggregation, he was clearly keen to link his project to the old humanist encyclopedic tradition as well.

letter in the next issue, writing in a footnote that the "interesting experiments" might "lead to some knowledge of the composition of soda, and of the base of muriatic acid, discoveries which could not fail to prove highly useful."[62] Now, a second letter from Peel had just arrived, reporting the galvanic production of muriate of potash—a strange variation on the first result.

Peel's work suddenly seemed even more interesting because Tilloch had just received word of Pacchiani's experiments, via Cuvier's report in the 25 June *Moniteur*. The news was given very briefly, with no experimental details, but it was enough to show a clear connection with Peel's results. Tilloch wrote an editorial commentary to Peel's second letter. "It seems probable," he declared, "that some of those substances which in the present state of our knowledge we are obliged to consider as the most simple elements, such as oxygen, hydrogen, and azote, are, in fact, compounds." Galvanism, it appeared, held out the promise of transforming matter at a deeper level then any power hitherto known.[63]

The question in Tilloch's mind: what exactly had Pacchiani, this unknown Italian far away, actually done? And it was soon answered—partly. Later in the month, when the earlier sections of the issue, including Peel's letter, were already with his printer, Tilloch received a copy of the *Edinburgh Medical and Surgical Journal,* recently published in the Scottish capital.[64] To his surprise, it contained an English translation of Pacchiani's account of his experiments. What had happened was this: one of the people to whom Giovanni Fabbroni had sent Pacchiani's first pamphlet was Andrew Duncan, the *Surgical Journal'*s editor (Duncan had become friends with Fabbroni while studying in Florence). Fabbroni asked him to communicate the discovery

[62] Slightly skeptical of Peel's claims, Tilloch had also suggested that Peel try the experiment with water composed from its elements by exploding hydrogen with oxygen. *PM* 22 (April 1805): 279-80.
[63] *PM* 22 (July 1805): 155. If this seems far-fetched, it is worth noting that Humphry Davy later made similar suggestions, and even claimed to have successfully decomposed nitrogen, sulphur, phosphorus, and carbon (see next chapter).
[64] The timing of events discussed here is clear from Tilloch's editorial comments.

"to the philosophers of your country through the medium of your Journal."[65] Duncan quickly had Pacchiani's pamphlet translated, and appended a reprint as a postscript to his journal (already in press) to give his readers "the earliest possible notice of the discovery." Tilloch continued the chain, reprinting the *Surgical Journal'*s translation in London, alongside a letter he received *even later* from the Manchester Chemist William Henry—who, having just read the *Surgical Journal*'s account, had successfully obtained muriatic acid in his own version of the experiment using distilled water and platinum wires.[66] Tilloch had lamented the lack of "information concerning the arrangement of professor Pacchioni's apparatus"; Henry's work provided clear instructions for producing the acid.[67] The end of the month now only days away, Tilloch padded out the remaining few pages of the journal with miscellaneous scientific news, the press-men ran off over a thousand copies of the sixth and last sheet of the journal, the sheets were folded and assembled, stitched with thread, and—finally—sent out for delivery to subscribers or for sale in London or packed up for shipment to the provinces.[68] The issue was done.

Journal editors and their publications were information aggregators—month by month, they gathered together a variety of texts and intelligence on science from different sources, processed them, and packaged them up to form the next month's issue. Once printed, copies were distributed to hundreds of readers across a broad geographical area. Journals were thus technologies of distance in two senses: they drew together scientific information from a variety of places, and their issues were distributed to a variety of places. Some texts, like the contents of

[65] *Edinburgh Medical and Surgical Journal* 1 (July 1805): 393-96. During his nine-month stay in Florence Duncan supposedly "lived on habits of intimacy with the celebrated Fontana and Fabroni": *A biographical dictionary of eminent Scotsmen* (Glasgow: Blackie, 1856), 2:176.
[66] This version did not feature the linen barrier between the two halves of the circuit that Pacchiani had used. Henry's letter is dated Manchester, 23 July; Henry had studied medicine Edinburgh and retained connections there, which probably explains why he subscribed to the *Surgical Journal*.
[67] *PM* 22 (July 1805): 155. A printer's error in the *Edinburgh Medical and Surgical Journal* led to Pacchiani's name frequently being misspelt "Pacchioni" by British men of science.
[68] A print run of 1,250 was reported in *Monthly Magazine* 26 (1806): 163.

the foreign journals, the editor specifically sought to obtain. Others arrived unexpectedly, as with Pacchiani's experiments in the *Edinburgh Medical and Surgical Journal*. Still others, like the *Moniteur* newspaper, were brought by forces outside the world of science entirely. And a key feature of the new specialist journals was that information was increasingly sent to the editors directly, as with Davy's first experiments with the battery printed in *Nicholson's Journal*, or Peel's letters to Tilloch for the *Philosophical Magazine*.

But although journals were, in a way, the newspapers of science, they clearly functioned very differently from the daily press. The basic division was not the paragraph but the article, and this could be long and heavy with technical detail. Not everything in journals was news—a new discovery or a report of the recent activities of the Institut certainly were, but when Tilloch reprinted a memoir from the *Philosophical Transactions*, for example, he was dealing with information of a less urgent kind, which he included to widen access to the contents of an expensive publication. Finally and most importantly, the audience was supposed to *participate* in the process of doing science. The idea was that if information on new discoveries was promptly and cheaply furnished to large numbers of investigators, then these people might be spurred to make new discoveries themselves.[69] "The most prompt publication of new discoveries," Rozier had asserted, "will also accelerate the progress of the sciences, which are nothing more than the sum of these discoveries."[70] This was the kind of snowballing process of investigation that had flourished in *Nicholson's Journal* in the wake of the announcement of the battery. To become a reader of a journal was to be invited to participate in the process of discovery itself.[71]

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[69] Journals were thus specialist in terms of their restriction to scientific content but not in terms of their audience. That is, their audience was not exclusively or even primarily specialist expert elites; they targeted a mixed community of scientific enthusiasts of various levels of accomplishment which was imagined to intersect with a broader "public."

[70] "Avis," *Observations sur la Physique* 1 (1773), iv-v.

[71] For an earlier articulation of this point see Samuel Lilley, "Nicholson's Journal, 1797-1813," *Annals of Science* 6 (1948): 78-101.

This raised thorny questions. By complementing or displacing older models of scientific communication, journals were bringing the technology of periodical print into the heart of the process of doing experimental science. What level of trust could be extended to their contents? How to maintain and guard a sure road to scientific knowledge through the disreputable periodical commons? One way journals were made more respectable was with a new persona of the scientific editor. Unlike the editors of most other periodicals, he advertised his name on the title page, putting his public scientific reputation up as a guarantee of the information contained within. Hence scientific journals were frequently identified by the names of the men who ran them: "*Gilbert's Annalen*," "*Gehlen's Journal*," "*Nicholson's Journal*," and so on.[72] With editors as guardians and guarantors of their contents, scientific journals in 1805 were certainly less disreputable and untrustworthy than newspapers or magazines. But precisely for that reason—because they were good enough to be useful to men of science but still foreign arrivals from the land of commercial news—the journals represented a new approach to information and knowledge in the sciences. They promised (or threatened) to transform scientific practice and community, and in galvanism and chemistry they already appeared to be doing so.

Responses to this situation varied. In Britain, journals seemed especially problematic because they destabilized a system in which scientific information was centralized—in Joseph Banks's library and Royal Society meetings—but only weakly controlled. This, combined with a distinctively-British denigration of print news, is why Banks and Blagden distrusted Tilloch and Nicholson.[73] Elsewhere the situation was different. Secure in their centralized mastery of French science, supported by a patron who, astonishingly, had managed to muzzle the frenzied Revolutionary newspaper press and bring it to heel, the members of Napoleon's Institut were far

[72] This was perhaps also because the official titles were commonly rather similar-sounding permutations of the terms "journal," "annals," "philosophy/physique/physik," and "chemistry."
[73] For Banks's clashes with Nicholson see Watts, "'We Want No Authors.'"

more willing to get involved with journals like the *Annales de Chimie* or the *Bulletin des Sciences*. And men of science in Germany, where scientific information was neither centralized nor controlled, and where periodicals were already a respectable part of learned Enlightenment culture, embraced journals wholeheartedly and with little fuss as solutions to the problem of communication. In short, while the techniques of periodical print were broadly similar in Britain, France, and Germany, the ways they were used, and the consequences of their use, were quite different. Yet all across Europe, journals were on the rise.

Printing up a Storm

During 1805 and 1806, the European journals carried a mass of information about Pacchiani and Peel's controversial galvanic experiments with muriatic acid. Different opinions, different versions of the facts, different experimental setups and results, different claims as to what the right experiments *were* to settle the question, criss-crossed the scientific world in their pages.[74] The controversy utterly defies explanation in terms of a linear step-by-step narrative. It had many participants in many places, who took a variety of positions (and sometimes changed their minds), who all received information about one another's experiments at different times and by different routes and in different forms, and who of course grafted their own interpretations onto what they received. We are not listening to two well-defined sides trading speeches across the floor of a debating chamber, but walking around a large echoing conference hall in which different conversational groups form, exchange heated arguments, and disperse, and individuals cross and re-cross one another's paths at different times, relate garbled versions of the work of third parties, and shout at people across the room who remain oblivious to what they are

[74] The references below represent only a sample of the mass of writings on the issue in English, French, German, and Italian.

saying.[75] Even more than the spread of Volta's battery, which was interpreted in diverse ways but universally *accepted* as an important discovery, the muriatic acid controversy illustrates the fragmentary and partial viewpoints from which individuals experienced scientific information and witnessed scientific events in the Napoleonic age.[76] That had always been true, of course, but the journals changed what the view was like. By bringing large numbers of participants into the controversy and by providing each person with an unpredictable monthly influx of information on the work of others far away, they delocalized scientific debate to a new degree, fostering a greater sense of interconnectedness among diverse groups without forging the kinds of personal trusted connections that exchanges of letters provided. Previously, in the age of the Academies and the Republic of Letters, the question of who had standing to stake scientific claims, and how to adjudicate those claims, had been mostly a local problem, whose solution could then be exported through webs of personal trust.[77] Now it was being posed on a grander scale.

Only twenty years before, a technical scientific controversy could not have taken place in periodical print in this kind of sprawling multi-national fashion, because most of the specialist publications that supported it did not exist. The experience was therefore rather new to everyone involved—editors, writers, and readers. As it turned out, the journals proved a good mechanism for fostering creative disagreement. Closing the debate would be another matter.

Fabbroni's letter-writing campaign transmitted the text of Pacchiani's claims beyond Italy, but only to a few individuals in key centers. He chose Georges Cuvier as his target in

[75] Compare with Martin Rudwick, *The Great Devonian Controversy: The Shaping of Scientific Knowledge Among Gentlemanly Specialists* (Chicago: University of Chicago Press, 1985), in which the controversy was multi-stranded but the major participants did *in fact* meet in person for spirited but civil debate across the floor of the Geological Society, in the style of the British House of Commons.
[76] Nonetheless, we will see that some of these viewpoints were more equal than others.
[77] See Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England* (Chicago: University of Chicago Press, 1994).

Paris.[78] The letter was two weeks in transit from Florence, with Cuvier announcing its arrival to a meeting of the Institut on 3 June.[79] The Parisian chemical elite took an immediate interest in the discovery, with a short notice appearing in the next issue of their *Annales de Chimie*. "We can only hope for the confirmation of such an important fact," the editors noted enthusiastically, which "would be fertile in its consequences, and throw new light on the theory of the French chemists."[80] The next month they were ready with a full translation of Pacchiani's pamphlet "to satisfy the justifiable impatience of our subscribers."[81] Soon afterwards, the same process of translation and reprinting brought Pacchiani's work to the German scientific community, in the pages of Gehlen's *Journal der Chemie* published at Berlin.[82] In a few months, the journals ensured that the information in Pacchiani's obscure *opuscoli* was broadcast across the European scientific world—now in English, French, and German rather than the original Italian.

The response to Pacchiani's experiments in Britain was framed by the extraordinary coincidence of the news crossing paths with Peel's letters in the *Philosophical Magazine*. "A great degree of attention was excited by these experiments," recalled the chronicler of galvanism John Bostock in 1818, "to which the more credit was attached, because they proceeded from

[78] On the connection see Dorinda Ourtram, "Storia naturale e politica nella corrispondenza tra Georges Cuvier e Giovanni Fabbroni," *Ricerche storiche* 12 (1982): 185-235.
[79] *PVSA* 3:221, 14 Prairial an 13 (3 June 1805).
[80] *AC* 54 (30 Prarial an 13; 19 June 1805): 332.
[81] *AC* 54 (30 Messidor an 13; 19 July 1805): 15-21.
[82] *Neues Allgemeines Journal der Chemie* 5 (1805) : 242-44. This probably appeared around the beginning of August. Pacchiani's work may have arrived in the Prussian capital after being forwarded by a former patron of Fabbroni's in Tuscany, Federigo Manfredini, then Minister of State in the Electorate of Salzburg (the short-lived secularized successor to the Archbishopric of Salzburg that had been given to Fabbroni's former patron, the Habsburg ex-Grand Duke of Tuscany). The earliest translation of Pacchiani's work into German was in fact printed at Salzburg, in the local medical gazette the *Medicinisch-Chirurgische Zeitung*, on 6 June (vol. 2, pp. 347-52). Fabbroni seems to have used Manfredini as a conduit for forwarding information on Pacchiani's work into the German lands; the learned news gazette the *Göttingische Gelehrte Anzeigen* for 24 June (pp. 999-1000) printed a summary of Pacchiani's pamphlet that had been forwarded to Göttingen by Manfredini. Fabbroni also sent a letter to St. Petersburg to the Foreign Minister of Russia, Prince Adam Jerzy Czartoryski, who was asked to forward Pacchiani's pamphlet to the Imperial Academy of Sciences; see Abbri, "Il Misterioso 'Spiritus Salis,'" 82.

sources entirely independent of each other."[83] Subsequent attempts to repeat and interpret them prompted much discussion among British chemists. Back in July, at about the same time that the *Edinburgh Medical and Surgical Journal* arrived in London, Charles Blagden had received his own private intelligence on Pacchiani's experiments from a contact in Paris.[84] "Employed in reading journals," he noted in his diary at the beginning of September. A week later, he was in Joseph Banks's library energetically talking over the response to Peel and Pacchiani's experiments with fellow chemist Charles Hatchett. He learned from Hatchett that Humphry Davy, for one, was skeptical: he thought the alkali in Peel's experiments "might come from the glass" (there seemed to be no serious doubts, as yet, about Pacchiani's work).[85]

On the Continent, others had been at work trying to replicate and verify Pacchiani's discovery. Travelling through Italy with the young French chemist Gay-Lussac and fellow German scientific traveller Christian Leopold von Buch, Alexander von Humboldt first heard the news at Rome. "The two Memoirs of M. Pacchiani struck us with such force that we are immediately building ourselves a galvanic pile to examine the action on water," he wrote to Fabbroni on 2 July. "Doubtless this will be one of the most brilliant and the most keenly-anticipated of discoveries."[86] Later, when Humboldt's party arrived at Milan, they joined forces with Alessandro Volta and fellow Italian *fisici* Configliachi and Moscati to try the experiments at Volta's house. With success: muriatic acid was formed just as Pacchiani had said. Von Buch was

[83] Bostock, *An Account of the History and Present State of Galvanism*, 62. Peel's experiments seem to have attracted comparatively little attention on the Continent, perhaps because by the time news of them arrived they were seen as simply a variation on Pacchiani's work; they were, however, known to Pacchiani's Tuscan circle by late 1805: *Gaetano Cioni to Fabbroni*, 30 November 1805, reproduced in Francesco Bonaini, *Elogio del dottor Gaetano Cioni* (Florence: Galileiana, 1852), 38; this collection contains a number of letters from Cioni (a supporter of Pacchiani's) to Fabbroni, documenting the Italian side of the controversy. In Britain, however, Pacchiani's work was naturally read by chemists like William Henry as having "confirmed" Peel's "very important discovery." *PM* 22 (July 1805): 183.
[84] *Blagden Diary*, 18 July 1805; the letter was from Benjamin Delessert (on this connection see Chapter 4).
[85] *Blagden Diary*, 4 September 1805, 12 September 1805.
[86] Humboldt to Fabbroni, 2 July 1805, Fabbroni Papers, American Philosophical Society, Philadelphia, MSS.B.F113.

the one to break the news north of the Alps on behalf of the group, and he knew how to do it: write to journal editors. On 6 October he sent letters to both Pictet of the *Bibliothèque Britannique* and Gilbert of the *Annalen der Physik*. "There is already no lack of theories relating to this wonderful transformation of water and the formation of muriatic acid," he enthused to Gilbert, "giving electricity a leading role in the phenomena of Nature!" Since Pacchiani's descriptions of the experiments were, as he put it, "far from clear," von Buch also provided careful details of the experimental setup that had succeeded at Milan.[87]

The sanction of Volta's eminent name was a strong mark in Pacchiani's favor. But the Italians (and the rest of the scientific world) still awaited the word from Paris.[88] On receiving Pacchaini's report of his discovery, the Institut had, as usual, appointed a committee of members to investigate and prepare a definitive report. The members chosen were Biot and the young chemist Louis Jacques Thenard. Despite (so they said) following Pacchiani's procedure with "the must scrupulous exactness," they failed to produce muriatic acid. But then news of Peel's discoveries arrived from England, and a letter from Humboldt reporting the successful trials at Volta's house arrived from Milan. These prompted "a complete revision in our researches." They now repeated the experiments "with extreme care," taking a variety of precautions to guard against possible contamination (which they suspected had led Pacchiani astray).[89] Their final result was that they did observe the formation of small quantities of an acid, but when tested, it was not muriatic acid! An alkali was also formed, and similarly resisted definite identification—though the chemists wondered if it might be coming from the decomposition of the glass used in

[87] *BB* 30 (November 1805), 247-63; *Annalen der Physik* 21 (1805), 129-35. Gilbert did not print the name of the writer of the letter he received, but since it is dated from Milan the same day as von Buch's letter to Pictet, and contains the same news, it seems safe to assume von Buch wrote it; furthermore, the letter is included in von Buch's *Gesammelte Schriften* (Berlin: Reimer, 1867), 1:549-552.
[88] The importance of the Institut's judgment to the Italians is clear from the letters from Cioni to Fabbroni reproduced in *Elogio del dottor Gaetano Cioni*, 35-41.
[89] *Moniteur*, 10 Brumaire an 14 (1 November 1805), 151-52.

the experiments. Their results were printed in the *Moniteur* of 1 November. Although their report failed to clear up the difficulties, it did not reflect well on Pacchiani's skills as an experimenter.

The Paris Société Galvanique performed its own parallel investigations, reporting them in the French journals; they failed to produce the acid. However, their experimenters had placed both the electrodes in the same tube of water—an arrangement which Pacchiani at the same time vehemently insisted was invalid.[90] In London, another letter from Peel appeared in the *Philosophical Magazine*—"I feel no little satisfaction at the interest my experiments have excited," he commented—while in Italy Pacchiani's immortality seemed already assured, with newspapers now trumpeting "the Celebrated discovery of Professor Pacchiani."[91] But in Berlin, Gehlen, editor of the *Journal der Chemie*, appended to his translation of Pacchiani's work a damning letter from his friend Professor Paul Erman, dismissing the experiments as "neither new nor important."[92] Erman asserted that Pacchiani's "supposedly new experiments" had in fact been anticipated by the work of others, particularly another Berliner, Paul Louis Simon ("to whom rather belongs the merit of having discovered this anomaly"). He observed that Pacchiani had clearly missed Simon's work because of poor communications due to a lack of scientific journals in Italy![93] Thankfully, Erman told the editor, "you protect us against any similarly unpleasant consequences of isolation and of ignorance of what is done in science in other

[90] *JP* 61 (Vendemiaire an 14; October 1805), 281-85; *AC* (30 Brumaire an 14; 21 November 1805), 152-60. Pacchiani's third *opuscolo* (originally dated 9 July), in which he had insisted on the separation of the electrodes, was reprinted in the same issue of the *AC* (pp. 111-18).

[91] *PM* 23 (December 1805): 257-59. Peel's third letter mostly detailed experiments to prove the purity of the water he had used; curiously, he did not mention Pacchiani's work. The principal Florentine newspaper, the *Gazzetta Universale*, printed a series of items on Pacchiani's discovery: 20 August 1805, 67:533-34; 24 August 1805, 68:543-44; 12 November 1805, 91:742; quote from 28 December 1805, 104:846.

[92] *Neues Allgemeines Journal der Chemie* 5 (1805): 244-48. The Prussian Erman later won the Prix du Galvanisme in December 1806 for experiments on the classes of galvanic conductors, subsequently printed in the *Journal de Physique*: *PVSA* 3:473, 29 December 1806; *JP* 64 (February 1807): 121-49. Politically, the award was timely; only two months before, having crushed Prussia on the battlefield, Napoleon had captured Berlin. "The prize has been awarded to M. Erman, a Prussian," the Minister of the Interior later wrote to Napoleon. "Thus the Prussians under the laws of your Majesty are treated as your subjects, and Prussia, conquered by your arms, also enjoys the benefits spread in your name." Champagny to Napoleon, 25 April 1807, reproduced in *La Revue Scientifique* 28 (1881): 329.

[93] Simon had declared the acid to be the result of contamination with organic substances.

countries, as does Gilbert [editor of the *Annalen der Physik*], and those few other faithful editors [*Annalisten*] who labor with such diligence and zeal." When Gehlen printed the letter, he added a sprawling two-page footnote with a raft of references to previous experiments that had appeared in the German journals.

But in Italy others were busy refuting this. Luigi Brugnatelli, Italy's leading chemist, insisted Pacchiani's experiments were *not* explained away by Simon's previous work (of which Brugnatelli was aware). In a text reprinted by journals in France, Germany, and Britain, he reported experiments in which he had produced muriatic acid even after taking care to exclude from his apparatus any of the "vegetable or animal substances" that critics pointed to as a possible source of contamination.[94] Pacchiani's partisans Gaetano Cioni and Pietro Petrini, who also knew of Simon's work (from a reprint in the *Annales de Chimie*), declared that Simon had been on the verge of Pacchiani's "luminous and fruitful" discovery—but had failed to pursue it precisely because he had dismissed the acid he found as a product of contamination. Their pamphlet published at Pisa was subsequently reprinted in the *Annales de Chimie* and, from there, by Tilloch in London.[95] With so many verifications, refutations, and counter-refutations propagating back and forth through the specialist journals, outside observers had trouble picking sides. "The composition of Muriatic Acid is still a subject of dispute," commented the London

[94] He insisted a wire of very pure gold was necessary to produce the acid. Brugnatelli, Professor of Chemistry at Pavia, originally communicated his experiments to the Instituto Nazionale d'Italia, the Napoleonic Kingdom of Italy's version of the Institut National; on this body see Luigi Pepe, "Volta, the Istituto Nazionale and Scientific Communication in Early Nineteenth-Century Italy," in *Nuova Voltiana: Studies on Volta and his times*, vol. 4, ed. Fabio Bevilacqua and Lucio Fregonese (Milan: Hoepli, 2002), 101-116. Subsequently they were printed in his own *Annali di chimica* and reprinted in the *Bibliothèque Britannique* and from there, by the *Philosophical Magazine*, and in the *Journal de Physique*, which was itself reprinted by Gehlen's *Journal für die Chemie und Physik* (Gehlen apologizing for presenting a "translation of a translation") and the *Annalen der Physik*. *Annali di chimica* 22 (1805): 257-304; *BB* 31 (January 1806): 43-60; *PM* 25 (June 1806): 57-66; *JP* 62 (April 1806): 298-318; *Journal für die Chemie und Physik* 1 (1806): 54-88; *Annalen der Physik* 23 (1806): 177-219.

[95] *Lettera di G. Cioni e P. Petrini al chiarissimo signore dot. Francesco Pacchiani* (Pisa: 4 September 1805), 4. *AC* 56 (30 Frimaire an 14; 21 December 1805): 269-78; *PM* 24 (March 1806): 167-70. Cioni and Petrini also attempted a quantitative analysis of the products of Pacchiani's experiment.

Monthly Magazine, "there are in our own, and in foreign countries advocates for both sides of the question, and each of the parties rests his pretensions on the evidence of experiment."[96]

So had Pacchiani and Peel made a discovery, or not? On this question, men of science were faced with the problem of choosing between conflicting opinions and divergent pieces of intelligence. These ranged from the enthusiasm of early converts like von Buch, to strongly negative views, exemplified by Erman and also by the caustic commentary of Jean-Baptiste Van Mons, editor of the Brussels-based *Journal de Chimie*.[97] In sardonic footnotes to his translation of excerpts from Pacchiani's reports, Van Mons openly mocked Pacchiani's skills as a chemist, picking up on the Italian's confusion over a series of tests for the possible presence of nitric acid. He added that he had tried the experiment himself and found no trace of muriatic acid.[98]

That issue of Von Mons's journal was not kindly received in Italy, but its criticisms were softened by the simultaneous news of Volta's support. In October 1805 an anonymous writer penned a letter from Florence to the Pisan *Nuovo Giornale dei Letterati*. The writer complained about the "great commotion" over the news that Volta and Humboldt had repeated the experiments successfully. The "marvelous transformation" of water into muriatic acid had already been proved beyond doubt by Pacchiani and accepted by Fabbroni and others, which ought to be enough even for "many Italians, not experimenters, who are loath to believe a thing until someone from north of the Alps says it is true." The positive result at Milan seemed like just another distant echo, cancelling out negative reports from Paris, Turin, and "the Journal of Van Mons" at Brussels. "What of it?" the writer waspishly observed, "My own senses have

[96] *Monthly Magazine* 21 (May 1806): 347.
[97] Van Mons had been involved with the *Annales de Chimie,* and launched his publication as a supplement to the Parisian chemists' journal.
[98] *Journal de Chimie et de Physique* 6 (1805): 402-06.

assured me of the reality of the transformation, and I am rather inclined to believe the evidence of my senses, than to believe what the journals may say about it."[99]

An Imperfect System

A lot of work went into sustaining this two-year controversy, and much of the burden was shouldered by journal editors: sourcing rare material from abroad, translating, receiving letters from correspondents, trying to adjudicate with editorial commentaries—all the while making sure their issues went to press each month. Whatever their private opinions on the validity of the discoveries, editors tended to print any new reports they could lay hands on, fostering the sense of debate (and, perhaps, confusion). In this way the small core set of European journals formed the venue for the controversy, in several languages and among multiple localized communities. These publications were not only similar but interconnected, in a transnational system in which information was translated, reprinted, and disseminated among different audiences in a densely-woven subdomain of the periodical commons. Their editors formed a self-conscious group, relying on one another's journals for fresh information to reprint—although the difficulties of wartime communication often made these interchanges rather sporadic.

The power of the journals was that they facilitated a division of labor in scientific intelligence. Editors did much of the work of communications in the scientific world, and sold access to that work to their readers. The hard graft of Tilloch and Nicholson in London, Pictet in Geneva, Gehlen in Berlin, or Gilbert in Halle in managing information turned their journals into the go-to sources of news about science in their respective locales. This certainly did not spell an end to the kinds of private exchanges of letters that carried information back and forth between places like Joseph Banks's library and the Institut National. But with the techniques of periodical

[99] *Nuovo Giornale dei Letterati* 3 (1805): 297-99; letter dated Florence, 20 October 1805.

news now actually being used to *create* a virtual space in print in which controversies like the muriatic acid experiments could play out, month by month, the dynamics of those older systems of communication would change too.

Tilloch and his fellow editors put a great deal of effort into maintaining their publications as go-to sources of the very latest scientific information. The up-to-date quality of information was key, and a large piece of what the editors were selling. But since this up-to-dateness was relative to locality—old news in Paris might be fresh and unknown intelligence in London—the job of an editor was not only to get information to press quickly but also to obtain it from distant places. Tilloch was proud that the muriatic acid discoveries had made their debut in his journal, with Peel's first letter printed at the end of April 1805, and he worked hard to keep the *Philosophical Magazine* at the forefront of the latest developments, gathering, translating and repackaging the latest haul of scientific texts from abroad. Over his monthly issues in 1805 and 1806, Tilloch reproduced everything he could find related to the muriatic acid experiments in Continental sources—Pacchiani's later *opuscoli* from the *Annales de Chimie*, reports of the Société Galvanique from the *Moniteur*, Brugnatelli's investigations from the *Annali di Chimica* via the *Bibliothèque Britannique*, and much more.

Editors wanted any text that might contain fresh news or scientific discoveries, from manuscript letters, to *Transactions* and *Memoirs* of scientific societies, the *Moniteur* newspaper, medical periodicals, literary-scientific magazines like the Paris *Magazin Enclycopédique*, and new books. But the most useful source, of course, was the foreign journals. Because each foreign journal issue was already a package of the latest scientific information in a particular place, captured at the time it was printed, editors could use foreign journals when preparing their own publications to gain extra traction on the progress of discovery. Getting some issues of the

Journal de Physique and the *Annales de Chimie*, for example, would bring a London editor like Tilloch up-to-date on the state of French science—information he could pass onto his readings by reprinting.[100] Collectively, monthly scientific journals had now started to emerge as their own European news system: just as the foreign news in newspapers was mostly reprints from other newspapers, so monthly scientific journals had reached a new stage where they were able to draw the bulk of their foreign information from each other.[101] This meant they exerted a powerful influence on how scientific information moved around. Send a letter describing an important chemical or galvanic experiment to the *Journal de Physique* and a few months later it would probably appear in translation in English in the London journals and in German at Halle or Berlin.

Translation was a key part of the process of making the information from foreign journals available to new groups of investigators. This chimed with the mission, prominent in the British journals especially, to bring scientific and technical discoveries to broad non-elite audiences: not only was the "practical man" of early industrial Britain not going to be invited to Joseph Banks's library or the Royal Society; he was also unlikely to read French or German. Consequently editors tended to have well-developed language skills. William Nicholson had built his reputation in part by producing English translations of French scientific books, while Gehlen of the *Journal der Chemie* was said to have a working knowledge of eight modern languages.[102]

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[100] Unlike other denizens of the periodical commons, scientific journals often (but not always) cited the sources of material they reprinted—at least when that material was taken from other journals and from academic memoir collections (the *Moniteur* was often reprinted in British journals, but almost never cited as the source).

[101] Thus French journal editors welcomed the advent of the first new British scientific journals in 1797-98, with the *Annales de Chimie* praising William Nicholson for his new publication because it was "…particularly devoted to circulating, periodically and swiftly, the results of the studies and work of all persons in various countries who are occupied with the natural sciences. This communication is the most certain method of hastening scientific progress, and as the author rightly says in a preface, one could make a long list of errors perpetuated by men of the first rank, due to the want of this general communication." *AC* 23 (12 Fructidor an 5; 31 August 1797): 173-74.

[102] See the obituary of Adolph Ferdinand Gehlen in *Repertorium für die Pharmacie* 1 (1815): 435-46, 473; for Nicholson, see Watts, "'We Want No Authors,'" and *DNB*.

Some of the labor was out-sourced; in London one intrepid grub street hack, William Johnston, was said to labor "from 5 in the morning till 11 at night" producing translations for the *Philosophical Magazine* and other publications.[103]

But first the editors had to get their own copies of the foreign journals. This was a difficult logistical problem. Even in the peaceful Europe just before the French Revolution, sending anything bulkier than a letter across international borders was far from straightforward. Lorenz Crell, trying to send copies of his *Chemische Annalen* in the 1780s to the Royal Society from Helmstedt in the Duchy of Brunswick—a principality actually bordering George III of Britain's Electorate of Hannover—experienced a string of problems with obstructive postal officials and packages delayed, or sometimes lost altogether.[104] By 1805, journal editors were more practiced in the logistics of moving print than Crell had been, but in wartime they also faced more serious obstacles. The most consistently-troublesome bottleneck was naturally the movement of journals between Britain and Continental Europe.

Editors used a variety of ad-hoc methods to get hold of each other's publications. To begin with, they tried to maintain reciprocal relationships with each other.[105] In 1803 Tilloch informed his readers of on-going connections with Pictet, Van Mons, Delamétherie, and Brugnatelli (so covering France and the low countries and northern Italy but not Germany), and in addition promised "new sources of information" from "arrangements he has lately made with his friends in different parts of Europe."[106] Advertising these sources of intelligence was clearly

[103] Richard Taylor to John Taylor, 17 March 1803, quoted in Brock and Meadows, *Lamp of Learning*, 27. Original in Taylor Papers, St. Bride Library, London.
[104] Crell to Banks, 17 December 1785, BL Add. MS 8096, ff. 284-285; 1 May 1786, BL Add. MS 8096, f. 291; 29 September 1786, BL Add. MS 8096, ff. 292-293; 7 January 1787, *Banks Corr*. 3:243-245; Crell suggested to Banks that he might be permitted to use the services of the "King's Messengers" who travelled between Hannover and London—a suggestion not apparently welcomed by Banks.
[105] Unfortunately very little manuscript correspondence between editors survives for the Napoleonic period.
[106] Preface to *PM* 17 (1803): 4. Pictet in turn advertised the connection with Tilloch (whom he had met during a trip to England) in his own journal, noting how "The Philosophical Magazine, of which Mr. Tilloch is editor, and the

a selling point. So too was the promise of being first to announce foreign discoveries, with the *Bibliothèque Britannique*, as the preeminent source in France for British scientific work, advertising special measures taken by its editors to "outpace the usual channels."[107] Editors also relied on intermediaries with expert knowledge of how to obtain foreign publications. One focal point was the shop of bookseller Joseph de Boffe in Soho, London, where English Journals leaving Britain for the Continent crossed paths with arriving French Journals. De Boffe was the principal importer of French publications in London, and he seems to have been involved in obtaining the *Journal de Physique* and presumably other publications for the London scientific community.[108] He was also part of the supply chain for getting British journals to Pictet's *Bibliothèque Britannique* in Geneva.[109]

In reality, and especially after Napoleon's Continental System blockade began in 1806, this was all less a regularized, predictable system than a series of jury-rigged ad-hoc measures that had to be adjusted as the geopolitical winds shifted, as armies marched back and forth, as ports were annexed, or governments relaxed or increased blockade restrictions. Sometimes the arrangements worked, but on other occasions supplies of foreign journals dried up entirely. One particularly difficult period for London was the winter and spring of 1808-09, in which the prohibitions of the Continental System were enforced very stringently and Britain was almost entirely cut off from the rest of Europe. In 1808 one short-lived periodical, the quarterly

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Journal which is edited by Mr. Nicholson, are the registers of all that is published of importance in England in the natural sciences; and the editor of the former of these compilations puts a particular emphasis on keeping his readers up-to-date [*au courant*] with the scientific news of the Continent." *BB* 18 (December 1801): 390-91.

[107] Preface, *BB* 1 (1796): 8.

[108] De Boffe was listed as a London agent on the title page of the *Journal de Physique* until 1806 (the year the Continental System began), and also seems to have been importing the *Annales de Chimie* in the 1790s. When war resumed in 1803 after the breakdown of the Peace of Amiens, the main commercial link between Britain the Continent was Hamburg and the other North Sea ports—until Napoleon's annexation in 1806—and Continental journals were probably obtained this way (the *Journal de Physique* also lists a Hamburg bookseller as agent).

[109] David Bickerton, *Marc-Auguste and Charles Pictet: The Bibliothèque Britannique (1796-1815) and the Dissemination of British Literature and Science on the Continent* (Geneva: Slatkine Reprints, 1986), esp. 332-69.

Retrospect of Philosophical, Mechanical, Chemical, and Agricultural Discoveries, lamented that "the increased difficulty of communication with the Continent has rendered the insertion of foreign papers, very irregular, and we are apprehensive will for some time to come deprive us of that interesting source of information."[110] At the end of this dark period, in June 1809 a relieved William Nicholson was finally able to inform his readers that "I have just received some of the French Journals, that have been so long in arrear; and am informed, that the rest are on their way from Paris."[111] Two months later they had arrived, so that Nicholson was finally able to give readers translations of important new material, such as an article by Gay-Lussac and Thenard on the newly-discovered Boracic acid [boron] printed in the *Journal de Physique* a full nine months before.[112]

During the Continental System, the interchange of journals between Britain and Germany seems to have been minimal; instead, the French journals provided a crucial bridge, with German journals re-translating French translations of the British journals and vice versa. "It is a great misfortune to me, that for some years I have not seen your admirable Journal," lamented Humphry Davy on one rare occasion in 1810 when he was able to send a letter (via Sweden) to Gilbert of the *Annalen der Physik*.[113] In an editorial on the progress of science during 1813 (a

[110] Preface, *Retrospect of Philosophical, Mechanical, Chemical, and Agricultural Discoveries* 3 (1808), ii; on this publication see Topham, "Anthologizing the Book of Nature," 147-49. During the same period, Tilloch also lamented to his readers that "in the present interrupted state of communication between this country and the Continent, we find it extremely difficult to procure the foreign journals. May we add, that our friends who happen to obtain any of them cannot confer on us a greater favour than by allowing us the use of them for a few days?" *PM* 32 (October 1808): 89.

[111] *NJ* 23 (June 1809): 158.

[112] *NJ* 23 (August 1809): 260-63.

[113] *Annalen der Physik* 38 (1811): 464-66 (the letter, received early in 1811 and presumably sent sometime in 1810, was written in English but is printed in German translation, though the quote I give here is in fact rendered in English in the *Annalen*). Davy complained in the letter of the inaccuracy of translations of his work in "French Journals" (apparently the *Journal de Physique*)—a matter of consequence to Gilbert who had been relying on these translations as a source for his own journal. Davy also complained of the French journals having reprinted "mutilated excerpts from reports in [British] journals to which I had not given my authority," as well as extracts from private letters "in which I have been greatly misrepresented." (Second quote printed in the *Annalen* in English.) Clearly the reprinting of British scientific information abroad could be frustrating as well as beneficial for someone

particularly turbulent year for Central Europe), Thomas Thomson informed readers of his *Annals of Philosophy* he could only speak about Britain and France, as war had completely disrupted supplies of the German journals.[114] The European journals did constitute a coherent system during the Napoleonic Wars, but sometimes their interconnection was more ideal than reality. Only after the wars ended could editors share information with anything approaching the speed and reliability they desired.

While the scientific journals did form an identifiable group, reprinting and borrowing from one another, they were not a self-contained system; they shared information with the wider periodical commons as well. We have already seen how the journals would reprint information from the *Moniteur*, medical journals, and numerous other periodicals. In their turn, journals were used as sources of information in their own right. Because they tended to print both foreign and domestic scientific intelligence before other publications, and in considerable detail, editors of general periodicals with a wider audience increasingly made the journals their go-to sources for information on the world of science. If they were looking for some interesting scientific news to make up a paragraph or to pique readers' curiosity, all they had to do was to flip through the latest *Philosophical Magazine* or *Journal de Physique* to be sure of finding what they wanted. In Britain, newspapers (prominent among them the Sunday *Observer*) sometimes copied from the

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like Davy who was eager to cultivate a European reputation. This is also a striking echo of Davy's criticism of inaccurate reports of his lectures in British newspapers the same year (see previous Chapter), where he also complained of being "misrepresented" in print. An example Davy probably had in mind was a reprint in the *Journal de Physique* of a somewhat garbled report of his discovery of potassium and sodium announced in his second Bakerian lecture at the Royal Society. This news had been printed in *Nicholson's Journal* without Davy's authorization: see *NJ* 19 (January 1808): 78-79, reprinted by *JP* 66 (March 1808): 259-60; the errors were corrected in a letter from "A Chemist" (quite possibly Davy himself) printed in Nicholson's next issue, who complained that "The extensive circulation of your excellent Journal both at home and abroad makes it more desirable, that it should not be the means of propagating any incorrect statements of scientific facts." *NJ* 19 (February 1808): 147-48.

[114] In his January 1814 editorial Thomson commented how "Germany has been the theatre of the most bloody war that Europe has yet seen [the War of the Sixth Coalition], and cannot therefore be expected to furnish much materials for our historical sketch; especially as Saxony, the country in which some of the most important scientific journals are published, has been occupied by the French army, and of consequence none of these Journals could make their way to London." Thomson also noted how "the state of Italy has for some years nearly interrupted all scientific communication between that country and Britain." *Annals of Philosophy* 4 (January 1814): 1-2.

journals, but the link to a wider readership was primarily through the magazines, monthly "miscellany" periodicals dedicated to the diffusion of knowledge of all kinds amongst an interested public.[115]

Representative among them was the *Monthly Magazine*, which in 1813 informed its readers how much "the progress of science and philosophy in England," was "proved by the subsequent establishment and extensive circulation of monthly journals devoted to those objects." When the *Magazine* had been founded in 1795, it commented, in Britain "there existed no journal appropriated to objects of science."[116] After listing the three British scientific journals then in operation as well as the London *Medical and Physical Journal*, the *Monthly* reminded its readers that they would always "enjoy the benefit of all these works" in the abstracting and reprinting of "whatever is new" in the *Magazine*'s own pages. During the muriatic acid controversy, the *Monthly Magazine* indeed made use of the journals for news, reprinting material from the *Philosophical Magazine* on Pacchiani's experiments.[117] Long before the invention of popular science journalism, reprinting in the periodical commons allowed the specialist journals to provide the ongoing narrative of scientific progress for wider audiences as well as for their own readers.

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[115] These publications—the *Monthly Magazine*, *Universal Magazine*, *Gentleman's Magazine*, *Scot's Magazine*, *European Magazine*, *Monthly Repository*, and others—had come to prominence in the second half of the eighteenth century (the oldest, the *Gentleman's Magazine*, was founded in 1731). Their model, including reader-sourced contributions and debates, reprinted articles, and "literary and philosophical intelligence," in fact shared many features with the scientific journals, and had been a source of inspiration for the first scientific journal editors in Britain; see Watts, "'We Want No Authors.'" After 1800, however, these publications increasingly turned to the journals as sources for scientific material rather than gather it themselves.

[116] Except perhaps, the *Magazine* noted, the *Repertory of Arts*, "a work chiefly engaged in the useful purpose of detailing the specifications of new Patents." *Monthly Magazine* 36 (November 1813): 342-43.

[117] In fact, the *Monthly* reproduced both Pacchiani's report and William Henry's letter in their entirety from the *Philosophical Magazine* for July 1805, and even appeared to claim that it had sourced the texts itself, printing them under the heading "*For the Monthly Magazine*." *Monthly Magazine* 20 (November 1805): 308-11. But on occasion the *Monthly* also appears to have made its own translations from copies of the French journals; *Monthly Magazine* 21 (May 1806): 330-31 translates a report on Pacchiani's work by the Société Galvanique printed in the *Annales de Chimie*—a different translation from the one that appeared in the *Philosophical Magazine* one month before.

Being a Journal User I: Charles Sylvester of Sheffield

Though the information published in their pages flowed out among a wider public, the core audiences for scientific journals were not passive readers; they were active users. Let's meet one of them: Charles Sylvester of Sheffield, Yorkshire, avid consumer of *Nicholson's Journal* and the *Philosophical Magazine*. Sylvester is a perfect example of the class of "practical men" who formed a large component of scientific journals' audiences in Britain and whose interest in the sciences grew out of concerns with the "mechanical arts," industrial and manufacturing processes, and applied chemistry.[118] Sylvester had come of age in 1790s Sheffield—a great hotbed of British radical politics, with one of the most active "Corresponding Societies" advocating for democratic reform and even a Revolution on the French model.[119] Then an "unwashed artificer," working as a silversmith, he had been closely involved with this milleu of militant politics, adding some heterodox religious opinions—"he was the only man I ever heard profess himself an atheist," remembered one contemporary.[120] It was this awakening to political consciousness in the Revolutionary dawn of the early 1790s that spurred Sylvester into a relentless program of self-education. He had reached the age of 18 without ever learning to read or write; this late start apparently only increased his hunger for knowledge.[121] He learned chemistry and metallurgy, published a book of radical poetry and songs, and began to think of making a living out of technical inventions, engineering projects, and public scientific lecturing.

Then the news of Volta's battery came to Sheffield. It must have been a revelation for the young metalworker and silversmith: the power to split water into its elements, drawn from

[118] For biographical information see Ian Inkster and Maureen Bryson, *Industrial Man: The Life and Works of Charles Sylvester* (Las Vegas, NV, 1999).
[119] E.P. Thompson, *The Making of the English Working Class* (New York: Vintage, 1966), 149-51.
[120] "Biographical Notices" by Joseph Hunter, BL Add. MS 36527, f. 24. The description of Sylvester as a "lean, unwashed artificer" was also due to Hunter (a Unitarian Minister): Inkster and Bryson, *Industrial Man*, 8. (Hunter was quoting Shakespeare's *King John*, Act IV, Scene 2.)
[121] Obituary of Sylvester in *Liverpool Mercury*, 25 January 1828.

nothing more than a stack of discs of silver and zinc. By 1805, riding the craze for galvanic demonstrations started by Aldini's visit to London, Sylvester was supporting himself mostly on income from lecturing on galvanism in the towns of the north of England.[122] Reading *Nicholson's Journal* and the *Philosophical Magazine* allowed him to keep up with the cutting edge in galvanism and other sciences; the journals were his window on the scientific world beyond Yorkshire, a link to people far away he had little chance of ever meeting.[123] He was able to read the latest experiments not only from British but also—through reprints—from French, German, and Italian researchers, both eminent and obscure. In the pages of the journals he could watch experimental research in the making, played out on a European stage, and imagine himself participating in it. In short, he made journals his own pedagogical tool.[124] "To the active and ingenious mind in early life this mode of information is invaluable," opined one reader of *Nicholson's Journal* in 1805. "Besides furnishing new ideas to the young student, they point out the precise state of the different branches of human knowledge; they teach him the necessary caution for conducting experiments with vigour and accuracy, instead of drawing conclusions

[122] In December 1804 he was in Nottingham giving galvanic lectures at a tavern called the Spread Eagle, showing "the Deflagration of Metals" and "the astonishing Effects of Galvanism upon Dead Animals." Broadside advertising lectures, BL Shelfmark 1888.c.18/1(45). In March 1805, he was in York giving lectures at "The Exhibition Room, Little Blake Street," promising "an immense" galvanic battery "exposing a metallic surface of more than 8000 square inches," and the ever-popular "Influence of this powerful stimulus upon animals some time after death," in which "the Head and Body of a Sheep will perform all the Motions attendant on Winking, Chewing, Breathing, Kicking, &c. after they have for some time been separated from each other." *York Herald*, 23, 30 March 1805. He also sold his own "galvanic apparatus, upon an improved plan" on his lecture tours. *Hull Packet*, 19 February 1805.

[123] The galvanism Sylvester learned from the journals fed back not only into public lecturing activity but also into his practical work: in 1805 he obtained a patent for a method of zinc-sheathing ships and houses to reduce corrosion that applied his knowledge of the different galvanic powers of metals. *Repertory of Arts* 9 (1806), 251-52.

[124] This was just as Nicholson and Tilloch had intended: their journals were supposed to be *both* vehicles of communication for proficient men of science and a tool of real-time, month-by-month self-education in the ongoing progress of science for those who needed instruction. "And though the greatest care will be taken that the Journal shall maintain its character for the accuracy, fidelity, and importance of its contents," Nicholson wrote in 1801, "great attention will nevertheless be paid to render it popular and instructive to the numerous Cultivators of Natural Philosophy who may not yet have arrived at the higher stations of acquirement." "Philosophical Journal: New Series," bound with *NJ* 5 (1802): 2. Tilloch had commented in his Prospectus how his *Magazine* would be useful to "young persons of every description, who wish to improve themselves in the Philosophical Sciences," who would "thus easily acquire information respecting the newest and most important discoveries either in Science or the Arts." *Star*, 29 June 1798, 1.

from a few insulated analyses, or imagining that his data are sufficiently perfect for establishing new systems. By reading these publications it is that he will enlarge his general conceptions, and will learn to emulate the various illustrious characters of all the enlightened countries of the world."[125] Sylvester was cut off from London's metropolitan scientific elite by geographical and especially social distance. His artisanal background and radical politics debarred him from elite gentlemanly scientific circles; someone like him could hardly expect to become a Fellow of the Royal Society, or be invited to sip tea in Sir Joseph Banks's library. But the journals were a more open and level information exchange, in which the hard currency was new and striking experiments. He didn't have to go to Banks's soirees; he didn't need to cultivate a scientific patron to smooth his way; he didn't even have to leave Yorkshire. All he had to do was write.[126]

Sylvester had finally summoned the courage to send his first letter to *Nicholson's Journal* in 1804; it had been on the action of the voltaic battery on water.[127] Since then he had been following the developing discussions over Pacchiani and Peel's experiments, and in May 1806 he was ready to write again. The initial spur was a series of letters in *Nicholson's Journal* in the first half of 1806 from a pseudonymous personage called "H.B.K.," advancing an anti-Lavoisierian explanation of the formation of acids in galvanic experiments. "H.B.K." had referred to a report by the Société Galvanique on Pacchiani's experiments which Nicholson had

[125] *NJ* 13 (January 1806): 72-73.

[126] Sylvester did, however, travel to London in August 1806 (his last letter to Nicholson on the controversy is dated there), and he doubtless at some point forged connections with commercial- and trade-oriented scientific circles in the capital. He eventually moved there in 1821, by which time income from industrial and engineering projects had made him a comfortably-wealthy man.

[127] *NJ* 9 (November 1804): 179-82. The letter-to-the-editor form clearly allowed Sylvester to frame his claims as a modest, unvarnished, and open-ended offering to other readers of the journals—thus avoiding the need to assume the more ostentatious and risky pose of independent publication. "I have for some time read and admired your valuable publication," Sylvester wrote to Nicholson, "but have not before this time ventured a communication on any subject. If you think the following thoughts and experiments worthy the attention of your readers, I shall feel myself honoured if you give them a place in your Journal." Sylvester's first letter was one of many attempts to grapple with the intractable "distance problem" of galvanism. "I observe that some of your correspondents are inclined to doubt the truth of water being a compound body," he noted, "from observing in its decomposition that the oxigen and hidrogen are given out at so great a distance from each other." Sylvester then proceeded to offer experimental arguments against this idea that water might be a simple substance.

just reprinted from the *Journal de Physique*.[128] "The Society then are of opinion," read Nicholson's translation, "that M. Pachiani [*sic*] was deceived as to the nature of the acid which he announced that he obtained, and that this acid might have been produced by some animal or vegetable substance employed in the apparatus."[129] (Curiously, some months later, the Société revised its opinion and announced successful replication of Pacchiani's work.)[130]

Sylvester intervened with a letter in *Nicholson's Journal* at the beginning of June 1806. He first attacked H.B.K's "very singular" anti-Lavoisierian conclusions, before bringing forward some elegant new experiments of his own to demonstrate the involvement of galvanism when one metal precipitates another from solution.[131] In a long postscript, he reported that he had been repeating Pacchiani's experiments himself, using a tube sealed at one end by a bladder to form the porous barrier, and wires of the inert metal platinum. He had been able to obtain muriatic acid—and an unknown alkali, which he thought might be ammonia or possibly (here he referred to Peel's experiments) soda. Sylvester ended on a lyrical flourish. "What an important field will be opened to the chemist and the naturalist! If a galvanic trough of moderate size be capable of generating muriate of soda from water… the water used in the economy of life and in other situations, must be frequently undergoing this change. The muriate of soda, thus generated, is carried by the rivers into the ocean, from whence it cannot return by evaporation: need we then wonder at the saltness of the sea?"

Nicholson printed Sylvester's letter with a marginal query attached asking whether the bladder might in fact have introduced contamination. Eight weeks later, Sylvester opened a new

[128] H.B.K.'s letters are *NJ* 13 (Janurary 1806): 40-42; *NJ* 13 (March 1806): 223-28; *NJ* 14 (May 1806): 50-56.
[129] *NJ* 13 (February 1806):137-41; reprinting *JP* 61 (Vendémiaire an 14; October 1805): 281-85. The report had been drawn up by Jean Riffault, *Administrateur* of Gunpowder and Saltpeter at Paris, and a member of the Société.
[130] *JP* 63 (August 1806): 165-66. In their earlier experiments, the Société Galvanique had left out the crucial porous barrier of separation between the two halves of the electrical circuit.
[131] *NJ* 14 (June 1806): 94-98. "I have no objection to his convincing the world of the absurdity of modern chemistry," Sylvester wrote, "but I would recommend him to bring forward facts for that purpose, without which every opinion must fall into nothing."

issue of *Nicholson's Journal* to find a letter from the surgeon and electrical therapist Charles Wilkinson, his occasional rival in galvanic lecturing, announcing he had not produced muriatic acid after a variety of precautions (including isolating the experimental vessel in a different room from his galvanic battery).[132] But Wilkinson had not used a porous barrier to separate the two halves of the circuit in his experiments, and so his result in fact confirmed Sylvester in his opinion that this separation was necessary to produce muriatic acid. The key was to make it happen without using any suspicious organically-derived substances. Sylvester also considered the possibility that the alkali might be coming from the breakdown of the glass vessel, and decided to try to exclude glass as well. Less than three weeks later, he sent a new experiment to Nicholson to throw light on what he was now calling "this dubious research." He had a tube made of porous pipe-clay, closed at one end, which was filled with distilled water and placed in a platinum cup. With all these precautions in place, the acid and the alkali were still produced. "From the above facts," Sylvester concluded (echoing and expanding on Pacchiani) "it would appear that the nitric and muriatic acids, as well as the three alcalis are oxydes of hydrogen." He signed off with an appeal to the *Journal's* "ingenious correspondents" to repeat his experiments.[133]

Sylvester's experimental practice shows quite clearly the tightly-interconnected character of the texts circulating in the European journal system. He was building on and responding to a range of scientific writers—among them Pacchiani, Peel, the Paris Société Galvanique, Wilkinson, and Nicholson's own editorial commentary. The experimental information he used

[132] *NJ* 14 (August 1806): 342-43. Sylvester had previously sparred with Wilkinson in *Nicholson's Journal* over the decomposition of water: *NJ* 10 (February 1805): 106-107. Wilkinson did his experiments while in Ireland lecturing on one of his self-proclaimed "galvanic tours"; he was normally based in London. For the "galvanic tour" see *NJ* 10 (January 1805): 56-57; on Wilkinson see John Thornton, "Charles Hunnings Wilkinson (1763 or 64-1850)," *Annals of Science* 23 (1967): 277-86.
[133] *NJ* 15 (September 1806): 50-52.

had originated in Paris, Pisa, Cambridge, Ireland—but he had received it all through the same channel: the two English scientific journals, printed in London, that he read every month. Sylvester, we can presume, knew neither French nor Italian—and in any case he almost certainly never had access to any foreign scientific journals in Sheffield—yet he was able to get this information because having access to one journal offered a connection to the whole journal system, and made him an indirect reader of foreign publications. Someone like him was precisely the audience editors Nicholson and Tilloch were selling to, and the use of journals was built into how he devised, carried out, and reported experiments. Over the next months, the journal system worked in the reverse direction to bring Sylvester's experiments to a European scientific audience: Sylvester (though he presumably did not know it) was reprinted in the *Bibliothèque Britannique* and then in *Gehlen's Journal* at Berlin and the *Annalen der Physik.*[134] His work even reached Pacchiani in Italy.[135] The Italian Professor and the Sheffield silversmith would never meet and never swap letters, but they were able to exchange experimental information, and it was the journals that made this possible.

Being a Journal User II: Humphry Davy

The most famous contribution to the debates over the galvanic production of muriatic acid was not native to the journals. It is Humphry Davy's Bakerian Lecture, "On some chemical agencies of electricity," read at the Royal Society on 20 November 1806. The lecture is an extended academic memoir, a more formal, much longer, and more closely-argued text than Sylvester's

[134] *BB* 33 (December 1806): 324-37; *Journal für die Chemie, Physik und Mineralogie* 2 (1806): 152-58; *Annalen der Physik* 25 (1807): 107-112.
[135] Or so we can assume, since details of Sylvester's experiments were given in a letter written by Gatteschi, a fellow Professor at Pisa, printed in a periodical with which Pacchiani was involved, the *Giornale pisano di letteratura scienze ed arti* (the new title of the *Nuovo Giornale dei Letterati*), 6 (Jan/Feb 1807), 114-24.

letter-based reports of experiments (or any other contribution to the muriatic acid debates).[136] When Tilloch reprinted it in the *Philosophical Magazine* starting in the beginning of July 1807, he had to spread its forty-six pages over three consecutive issues.[137] In the words of Henry Brougham, writing in the *Edinburgh Review*, Davy had constructed a "chain of experiments by which he removes all the difficulties and obscurity that hitherto hung over the changes produced in water by Galvanic action." Brougham further remarked how it was "surpassed by no inquiry of the kind, in modern times, for closeness, copiousness, and minute accuracy."[138] Looking back some twelve years later, the great Swedish chemist Jöns Jacob Berzelius agreed, dubbing it "among the best memoirs which have ever enriched the theory of chemistry."[139]

Davy's memoir, and particularly the ideas of electrochemical action which it contains, has been put under the microscope many times by historians of science.[140] Rather than repeat their analyses, I want to read it as a demonstration of Davy's access to and masterly use of experimental information—what the *Edinburgh Review* called "the felicity with which he seizes and follows up the loose hints thrown out by other inquirers."[141] The text is a work of synthesis as well as a publication of new results, and it is Janus-faced, meant to tidy up past debates, and to open up a future experimented program. First, Davy meant to close the controversy started by Peel and Pacchiani over the production of acids and alkalis by sorting and clearing away its

[136] Printed version in *Philosophical Transactions* 97 (1807): 1-56.
[137] *PM* 28 (June 1807): 3-18; *PM* 28 (July 1807): 104-119; *PM* 28 (August 1807): 220-33. This kind of multi-issue serialization of long and important papers was of course a sales tactic as well.
[138] *Edinburgh Review* 11 (January 1808): 398.
[139] Jöns Jacob Berzelius, *Traite de Chimie* (Paris: Didot, 1829), 1:164. (First Swedish edition published in 1818).
[140] J.R. Partington, *A History of Chemistry*, vol. 4 (London: Macmillan, 1964), 40-45; Harold Hartley, *Humphry Davy* (London: Nelson, 1966), 50-54; Colin Russell, "The Electrochemical Theory of Sir Humphry Davy," *Annals of Science* 15 (1959): 1-25; Richard Ziemacki, "Humphrey Davy and the conflict of traditions in early nineteenth-century British chemistry" (PhD diss., Cambridge University, 1974), 235-45; June Fullmer, "Humphry Davy, Electrochemist," in *Selected Topics in the History of Electrochemistry*, ed. George Dubpernell and J.H. Westbrook (Princeton: Electrochemical Society, 1978), 88-99; David Knight, *Humphry Davy: Science and Power* (Cambridge: Cambridge University Press, 1992), 62-64; Golinski, *Science as Public Culture*, 203-213. As Golinski notes, many of these historians read the lecture for the electrochemical "theory" it supposedly contains, glossing over the closure of the muriatic acid controversy which constituted the real importance of the work for Davy's contemporaries.
[141] *Edinburgh Review* 11 (January 1808): 398.

tangle of different experimental assertions. Second, having established how the voltaic battery acted, Davy wanted to show the way to its use as an instrument of chemical research.[142] The lecture is the production of a young chemist already ensconced at the heart of the London scientific community, connected to the web of information centered on Joseph Banks's library in Soho Square. Through Banks and other contacts, Davy had access to a unique accumulation of scientific print (including that great luxury, original copies of the French journals), as well as to private news received in letters by the principal communicators with French men of science, Sir Charles Blagden and Alexander Marcet. He was as well placed as any chemist in Britain could be to access and exploit the insights of others. This certainly does not explain Davy's success, but it does help to put it in perspective.

Davy's memoir contains a mass of experiments, but his two main conclusions can be quickly stated. First, galvanism acting on pure water decomposes it into hydrogen and oxygen only, and any appearance of acid or alkali is always the result of contamination. Second, galvanic electricity acting on a solution draws one class of substances to the negative wire ("hydrogene, the alkaline substances, the metals, and certain metallic oxides") and another class to the positive wire ("oxygene and acid substances"). This galvanic attraction is "sufficiently energetic to destroy or suspend the usual operation of elective affinity"—that is, it effects chemical change, decomposing substances or separating out the different components of a solution.[143]

[142] Drawing on previous sociological studies of experiment, Jan Golinski has called this the "black-boxing" of the Voltaic battery, in which it was "transformed from a subject of experimental research into an unproblematic tool for further research." Golinski, *Science as Public Culture*, 206; for the original application of the black-box concept to experimental work see Trevor Pinch, *Confronting Nature: The Sociology of Solar-Neutrino Detection* (Dordrecht: Reidel, 1986), 212-14. I would agree with this insight provided it is understood that it is not the battery *itself* being black-boxed, but rather its action on matter in aqueous solution. Debates continued over how the internals of the battery functioned, but after the acceptance of Davy's work chemists generally agreed on the essentials of what electricity *from* the battery would do to water and substances dissolved in it.

[143] *Philosophical Transactions* 97 (1807): 29.

After a few works of introduction, Davy rushes into a mass of citations of the journals, from *Nicholson's*, the *Philosophical Magazine*, and the *Annales de Chimie*. He reviews their main items on the generation of acids and alkalis, noting that the production of acids, at least, was nothing new—they had been observed in England soon after the voltaic battery had first been used on water. Arriving at the *Philosophical Magazine*'s reports of Pacchiani's experiments, and Peel's experiments with muriate of soda, he describes them as "recent, and the discussions with regard to them still alive."[144] As opinions against Pacchiani and Peel he cites the experiments of Biot and Thenard, and the Société Galvanique (of whose subsequent reversal of opinion he was unaware). He then devotes a paragraph to Charles Sylvester's recent work from *Nicholson's Journal*, which, because of Sylvester's removal of glass and organic substances from the experimental arrangement using pipe-clay and platinum, he sets up as the cleanest positive claim for the production of acids and alkalis (and therefore the one he most needs to refute). "I have no doubt of the correctness of his results," Davy says, "but the conclusion appears objectionable" (he pointed out that pipe-clay might decompose to give an alkali).

But Davy also displayed access to information that would not have been available to a reader who, like Charles Sylvester, only had access to the British scientific journals. He made use of Biot and Thenard's important report from the *Moniteur*, which, for reasons which remain obscure, was never reprinted in Britain.[145] For experiments later in the paper on the galvanic decomposition of various salts and the separation of their constituents at the positive or negative

[144] Davy also claimed to have privately performed experiments similar to Pacchiani's "as early as 1800" and produced muriatic acid, but (he said), he had reached the conclusion that "the muriatic acid owed its appearance to the animal or vegetable matters employed." (Ibid., 2)

[145] The natural explanation for this would be that Tilloch or Nicholson never got hold of a copy of the *Moniteur* issue containing the report, since if they had they would almost certainly have reprinted its contents. This is supported by Nicholson's mentioning Biot and Thenard's failure to confirm Pacchiani as "scientific news" in the issue of his journal published on 1 December 1806(!), just *after* Davy had mentioned this fact in the first part of his Bakerian lecture. This probably indicates reluctance on the part of Davy, Blagden, or others in Banks's circle to share material received from France with the journal editors, with whom Banks was not on good terms.

poles—crucial to his ideas of the positive and negative electrical nature of different substances—he had access to a memoir by the Swedes Hissinger and Berzelius, of which extracts had appeared in the *Annales de Chimie*, but which was, as he later put it, "never translated into English, nor at all attended to."[146] Finally, Davy had access to private information from fellow London chemist William Hyde Wollaston, who never made any public statement on the controversy, but had come up with an arrangement for separating out the two halves of the circuit using two tubes connected with moistened fibres of amianthus (very pure asbestos).[147] This finally appeared to solve the problem of creating a porous barrier that did not introduce contaminants.

Davy moved on to his own chain of experiments, which was constructed so as to reproduce the contamination and then gradually remove it.[148] In two agate cups connected by a piece of amianthus, he ran the galvanic decomposition with distilled water and platinum wires for 48 hours. He obtained traces of muriatic acid and an alkali. A similar arrangement using glass tubes as reaction vessels also yielded acid and alkali. He repeated the experiment with agate cups four times, finding that acid and alkali were produced in smaller quantities each time, but could not be quite eliminated.[149] Learning from Biot and Thenard's report, he was sure to note his precautionary techniques of repeatedly washing the apparatus with distilled water, and never

[146] Bakerian Lecture for 1828, *Philosophical Transactions* 116 (1826): 388; however, in this late lecture Davy claimed that had had not drawn any major ideas from Hissinger and Berzelius in 1806, and had only seen their paper after his "fundamental experiments were finished." Davy had used the *Annales de Chimie*'s extracts from the memoir, which had been taken from an 1803 German version (itself translated from a Swedish Manuscript) in *Gehlen's Journal*. *AC* 51 (30 Thermidor an 12; 18 August 1804): 167-74; *Neues allgemeines Journal der Chemie* 1 (1803): 115-49.

[147] *Philosophical Transactions* 97 (1807): 3.

[148] The description I give here is not of course necessarily what Davy *actually* did during the course of his investigations, but rather the way he represented his chain of experimentation to the outside world in his Bakerian Lecture. It is possible that some additional insight into how Davy used information from others could be gleaned from detailed attention to Davy's laboratory notebooks, but the payoff from this work would probably be fairly limited as Davy's recording of his experiments tended to be sporadic and messy.

[149] On the fourth attempt Davy managed to obtain a solution in which muriatic acid was undetectable, but he still obtained what appeared to be "extremely dilute nitrous acid."

touching any of it with his hands during the experiments. Then he began to strike out on his own. What exactly was "pure" water? "The only substance which I could now conceive capable of furnishing the fixed alkali," he informed his readers, "was the *water itself*."[150] There followed a series of experiments trying the decomposition of water in hollow gold cones. Davy found more acids and alkalis produced, and decided they might originate from "saline matter carried over in distillation." Careful evaporation of the distilled water in a silver still allowed him to obtain a small sample of the impurities, which when reintroduced into the gold cones did indeed increase the alkaline matter formed under galvanism. Using purer distilled water obtained with the silver still, he repeated the experiment again in the gold cones. He still found some minute quantities of nitrous acid and ammonia, which he decided were formed from nitrogen dissolved in the water. The final version of the experiment was run inside the receiver of an air pump that had been repeatedly exhausted and then filled with hydrogen to drive all nitrogen from the water. After leaving the battery connected for 24 hours, he found oxygen and hydrogen produced, and the remaining water chemically unchanged. At last, he had been able to bring galvanic electricity to act on pure water to give oxygen and hydrogen, and nothing else.

Drawing on the accumulated experiences of a range of researchers—gathered from months of reading similar experiments of varying quality in the journals—Davy had pared down the decomposition of water to its fundamentals: finally, here was chemically pure water, decomposed to hydrogen and oxygen, without any of Pacchiani or Peel's strange acids or alkalis. His careful analysis also furnished an important corollary: galvanic action had the power of drawing acid substances in solution to the positive wire, and alkaline substances to the negative wire. The later sections of the paper proceeded to build up a program of experimental investigations based on these insights, and into which, almost as an aside (the *Edinburgh Review*

[150] Emphasis in the original.

praised him for "applying himself almost exclusively to the multiplication of facts, and repeatedly admitting that the time for theorizing is not yet come"), he tossed his famous hypothesis of the equivalence of "electrical energy" with chemical affinity.[151]

Davy's work became public through a gradual, staggered process. Only Fellows of the Royal Society, and a few guests, heard what he had said when he actually read his lecture—which had to be spread over four of the Society's Thursday-evening meetings.[152] Neither of the British scientific journal editors were present; they were not Fellows and were *personae non gratae* at meetings due to fears they would use the opportunity to publish what they heard.[153] Nonetheless, the editors were able to gather information on what had been said, thought the experimental details were sketchy. "In this interesting lecture," noted Tilloch in a veiled piece of self-advertising for his own journal, "the author particularly noticed the important discoveries of Mr. Peel, and of Pacchiani, relative to the formation of alkali and acid, by the electric influence, and which have assisted him in exploring a new and untrodden region of chemical science."[154]

Following a common practice, Davy's memoir was eventually circulated among Fellows as printed "separate copies," but the text was only published in Britain in the *Philosophical Transactions* in June 1807, some seven months after Davy announced his results.[155] Rozier's ghost might not have approved, but it is unlikely that Davy worried much. As a rising young Fellow and man of science building a career in the metropolis, the Royal Society was his clear choice of venue to announce such important experiments, and that meant publication in the

[151] Davy did not, of course, mean energy in the precise quantified sense it now has in chemistry and physics.
[152] These were on 20 November, 27 November, 11 December, and 18 December (the annual Royal Society elections taking up the meeting of 1 December).
[153] Reports of meetings, presumably sourced from Fellows or guests who had been present, did nonetheless appear in the *Philosophical Magazine* and (less often) in *Nicholson's Journal*.
[154] *PM* 26 (December 1806): 269.
[155] We would now term these "separate copies" offprints or preprints; on this practice see Watts, "'We Want No Authors.'" Publication date from RS MS/212 (Phil. Trans. Receipt Book 1800-18). Davy was allowed the opportunity for revisions, and so this text would not have been quite the same as the one read at the Royal Society.

Transactions. It was undoubtedly a good decision. A month after his Bakerian lecture, Davy was named Royal Society Secretary. His age: twenty-eight.

Nonetheless, though he now eschewed announcing results in the scientific journals where he had sent his first galvanic experiments when an unknown provincial, Davy did increasingly make use of the international journal system to publicize his work abroad and build a European reputation. He sent one of the "separate copies" of his lecture to Marc-August Pictet of the *Bibliothèque Britannique*, who began to serialize it in translation starting in his journal for May—that is, just *before* it was published in the *Philosophical Transactions* in Britain![156] Another translation by Berthollet later appeared in the *Annales de Chimie*, and the French texts provided the sources for translations in the German journals.[157] This uneven publication pattern meant that full acceptance of Davy's arguments was correspondingly rather slow, and the controversy did not really fade away until well into 1807. As late as July the *Philosophical Magazine* was still publishing reprinted reports from the Société Galvanique stating their reversal and endorsement of Pacchiani's discovery.[158] Perhaps the final curtain did not fall until the Institut National endorsed Davy's work in the strongest terms in January 1808 by awarding it the Prix du Galvanisme.[159]

[156] Pictet to Marcet, 7 July 1807, *Pictet Corr.* 3:397; *BB* 35 (May 1807; published around 1 June): 16-59; Pictet's commentary praised Davy for having resolved "the difficulties and the numerous contradictions presented by the experiments and theories of philosophers on the chemico-galvanic action." Possibly the paper was taken to France by the American diplomat Colonel David Humphreys: *Blagden Diary*, 15 March 1807. Davy also forwarded "separate copies" of later *Philosophical Transactions* papers to Pictet.
[157] *AC* 63 (30 August 1807): 172-224; *Journal für die Chemie und Physik und Mineralogie* 5 (1808): 1-67.
[158] *PM* 28 (June 1807): 55-59; this was in fact the same issue in which Tilloch began to serialize Davy's Bakerian Lecture. The Société's change of heart came about after Brugnatelli sent them his description of his successful trials.
[159] The announcement of the award was made at a public session of the Institut on 4 January 1808, and printed in the *Moniteur*, 6 January 1808, p. 20. Far outpacing both the journals and letters from France, the *Moniteur* brought Davy the news of his award in only two weeks: *Times*, 20 January 1808.

Hoax in the Journals!

What of the two original simultaneous discoverers of 1805? In Italy, Pacchiani, witnessing the savage criticism of his experimental technique in the journals and especially in the Institut's report from Biot and Thenard in the *Moniteur*, began to realize that there was little chance of his work being hailed as a grand new discovery comparable to Volta's pile. Whatever the eventual outcome of the debates he had started, at best he would be remembered for blundering on a phenomenon which it would be left to others to clarify and explain.[160] Triumph gave way to self-doubt, which turned to disillusionment. "It takes real fortitude," wrote a friend, "not to get discouraged when one has a Commission of the Institut for adversaries."[161] Eventually, Pacchiani's fortitude ebbed away, and after a few more half-hearted attempts at experimental investigation, he abandoned his pursuit of the sciences.[162]

And the mysterious "Mr Peel"? In February 1807 Tilloch received the following anonymous note, sent from Cambridge. Unbeknownst to Tilloch, the writer was the chemist Smithson Tennant, to whom a young student had just confessed a guilty secret:

> I write to inform you that a letter signed by a person calling himself W. Peel is an imposture. The author of this low piece of witticism pretends to the title of chemist; but I recommend to him to consider that the first qualification for an interpreter of nature is the love of truth. Let him be detected in committing a second fraud in the republic of letters, and I shall send you his name.[163]

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[160] Such was chronicler of galvanism John Bostock's view of Peel and Pacchiani's work in 1818: "the minute examination of the effects of galvanic electricity upon water to which it [the supposed "discovery"] gave rise, may probably be regarded, in some measure, as the immediate cause of Sir H. Davy's most brilliant discoveries." Bostock, *Account of the History and Present State of Galvanism*, 63.

[161] Cioni to Fabbroni, 30 November 1805, reproduced in *Elogio del dottor Gaetano Cioni*, 38.

[162] See Abbri, "Il Misterioso 'Spiritus Salis,'" 86. His hopes of scientific glory dashed, Pacchiani seems to have turned to literary studies, particularly the work of Dante.

[163] *PM* 27 (February 1807): 82. The letter was dated Cambridge, 22 February 1807, and signed "CHEM." Tennant is identified as the letter-writer by *Blagden Diary*, 8 March 1807.

Tilloch was, understandably, upset. "It is impossible," he told his readers, "even to conceive a motive for such unprincipled malignity." The hoax had thrown the integrity of his journal into question, and made him, the editor, look like a fool. For us, and no doubt for Tilloch's readers, it calls attention to a problematic feature of the journals—the uncertainty over how much trust to put in information printed in their pages. The new journals put a paper barrier between the people making new science and its communication to the wider world; authors of material in journals did not have to appear in person at scientific societies or academies, and did not need any scientific patrons to vouch for their work. That, after all, was the point. No one had ever heard of Mr. Peel, but no one had ever heard of the careful Sheffield autodidact Charles Sylvester either, and his experiments, reproduced all around Europe, were acknowledged by Davy and others to have been an important step forward. "It is impossible," Tilloch wrote, "in conducting a work dedicated to the service of truth, and open for the communications of men of science, to guard against impositions authenticated by a false signature. To myself the injury might have been very serious; but this is a trifle compared with the mischief done to the public." Reading the revelations in the *Philosophical Magazine*, Sir Charles Blagden hastened to be the first to tell Joseph Banks about this scandalous "detection of Peele." No doubt this confirmed both men's suspicions of journals as untrustworthy arbiters of knowledge, useful maybe in their own way, but not fit to rival the Royal Society's time-honored procedures. We can presume the two old friends shared a moment of *schadenfreude* in witnessing Tilloch's misfortune.[164]

A few days later, at the Sunday evening *conversazione* in Banks's library, Blagden obtained a little more information on the scandal in a discussion with Smithson Tennant and William Hyde Wollaston. They told Blagden that "the fictitious Peele," confessing his hoax to Tennant, had said he had simply made up his reports. He had claimed never even to have

[164] *Bladgen Diary*, 6 March 1807.

performed any experiments at all. Blagden was skeptical: the pseudo-Peel's description had been "too exact" (he had a point). The motive behind the hoax remained unclear. And here the trail runs cold; as Tennant steadfastly refused to divulge the student's identity to Blagden, Mr. Peel's true name may forever remain a mystery.[165]

Blagden's private talks aside, the revelation that the British side of the simultaneous discoveries of 1805 was a fraud seems to have provoked little debate. It did not bring about a public discussion among British men of science of the merits and dangers of the new scientific journals. One reason might be that the controversy over the production of muriatic acid was no longer really active: Davy had by then more or less laid it to rest. But a more convincing explanation is that the hoax did not undermine the community's trust in journals because there was no strong belief in the trustworthiness of journals to undermine. Certainly, journals had proved themselves extremely useful as a system for communicating information, but this was no reason to conclude that they might one day become the *preferred*, much less *exclusive* venue for the formal announcement of new science. As a provincial youth, Humphry Davy had started out in galvanism with letters printed in *Nicholson's Journal*, but after his move to London, he preferred Royal Society meetings and the *Transactions* as his venue. For Davy and others like him, having a paper read at the Society trumped quick printing in a journal, and that continued to be the case well into the nineteenth century.

And strangely enough, the hoax seemed to have helped rather than hindered the advance of science. Or such was the opinion of later commentators who gave a more positive spin to the disclosure of Mr. Peel's fraud. One author, looking back in 1830, put it thus: "It is a remarkable

[165] *Blagden Diary*, 8 March 1807. The pseudo-Peel is described by Blagden as "a young man of Cambridge," and so, we can assume, a student. As Blagden realized, the letters to Tilloch (though containing various inaccuracies) do strongly suggest the writer had been trying his own galvanic experiments. Perhaps the student was intrigued by the journals' new low barrier to participation in science, and decided to test the possibility of getting his words into print by sending Tilloch some embellished descriptions of a few hastily-performed trials with the battery.

feature in the history of this branch of Philosophy, that a random conjecture, hazarded originally, perhaps, as an idle jest, should have been afterwards apparently confirmed by repeated experiments in various quarters, and should have given occasion to the masterly train of investigations, which ultimately led to the discovery of the bases of the alkalies, and to the numerous splendid results with which modern science has been enriched, and which have immortalised the name of Davy."[166]

Conclusion: The Disreputable Journal

In October 2013, the leading journal *Science* broke the news that a journalist had conducted a sting operation in which a "hopelessly flawed" hoax paper, under the names of non-existent authors, had been submitted to 304 journals, all published on an open-access model.[167] More than half accepted it for publication. As with the notorious "Sokal Hoax" seventeen years earlier, the revelation ignited a storm of debate over the real lessons to be drawn from the scandal. Was the point, as *Science* itself seemed to be claiming, about the dangers of open access, which had supposedly created an "emerging Wild West in academic publishing," in which dubious publishers charged authors lucrative fees for the privilege, essentially, of putting articles on a website?[168] Or was the real issue, as proponents of open access replied, a broader problem of scientific standards at certain journals, and a "broken" peer review system?[169] What was really at

[166] Article "Galvanism," in *Encyclopædia metropolitana, or, Universal dictionary of knowledge*, 2nd ed. (London: Baldwin, 1830), 2:188.
[167] John Bohannon, "Who's Afraid of Peer Review?," *Science* 342 (4 October 2013): 60-65. The bogus paper claimed to prove that a chemical exacted from a lichen (the specifics varied between versions of the paper) possessed anti-cancer properties.
[168] This perspective was particularly prominent in *Science*'s accompanying press-release. On the resulting controversy see http://chronicle.com/blogs/percolator/critics-say-sting-on-open-access-journals-misses-larger-point/33559; this and subsequence references accessed 11 August 2013.
[169] See the letter of response from one prominent open access journal in "Science Communication: Power of Community," *Science* 342 (6 December 2013), 1169-1170. Also http://oaspa.org/response-to-the-recent-article-in-science/, and the large list of critiques at http://svpow.com/2013/10/03/john-bohannons-peer-review-sting-against-

stake: the dark side of a new mode of scientific *communication*, or the failure of systems of *accreditation*?

Such debates have valence because journals—whether open access or subscription, online or print—now carry an aura of association with the process by which science produces knowledge. Scientific knowledge is said to reside in journals and to be guaranteed by them.[170] We have seen how things were very different at the start of the nineteenth century, when there was no epistemic wall between specialist scientific periodicals and the rest of the periodical commons. In long-running controversies like the muriatic acid experiments, in galvanism, chemistry, and other sciences, men of science explored the communicative possibilities of the new medium without worrying too much—until they had to—about the epistemological problems of trust and accreditation journals posed.

Modern debates about open access and peer review still entangle the two seemingly separate processes of communication and gatekeeping. In a very different way, they were entangled in the Napoleonic period too, when monthly journals were embraced as useful technologies of communication whose editors, rather by accident, found that they had all sorts of problems of accreditation and trustworthiness to confront. Solving these problems would take a long time, and, arguably, it is an illusion that they have ever been permanently solved. Later, during the nineteenth century, journals did come to appear more and more trustworthy, as various patches were bolted onto the model: the banning of anonymous or pseudonymous contributions, major institutional endorsement, refereeing of papers, and eventually the professionalization of science itself, which created a career structure that made it clearer who could count as a scientific author and who could not. But these changes were barely on the

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science/. *Science*'s report of the hoax was itself widely criticized for lax standards, such as not including a control group of subscription-based journals—and even for not having undergone peer-review!

[170] On how this came to be, see Csiszar, "Broken pieces of fact."

horizon for men of science in 1805. Looking through their eyes, the real surprise is not that journals were once untrustworthy newspaper-like information aggregators, but that they ever metamorphosed into the guardians of scientific truth at all.

To conclude: men of science in the Napoleonic age did not use journals because they were trustworthy and prestigious; it was the usefulness of journals as tools of information that made it eventually seem like it might be *worth* creating social systems to guarantee their trustworthiness as venues for science. Starting out as a rather dubious part of the scientific enterprise, periodical print was embraced in scientific life around 1800 largely because people could use it to find out what other members of their community were doing. Journals sold relief from information starvation—they took care of difficult problems to do with obtaining and transmitting information, and their readers bought the results each month. Although wartime circumstances caused journal editors much frustration, war also buoyed journals' rise: precisely because war made foreign scientific print so hard to obtain, and precisely because journals editors put so much time, effort, and money into getting hold of foreign journals and other rare print sources, everyone seriously interested in the experimental sciences had to read journals regularly to avoid falling behind. Over the course of the Napoleonic era, they and their editors started to become seemingly indispensable parts of the scientific enterprise. On the coming of peace in 1815, the members of the scientific community found they had acquired a fast and flexible transnational print information system that they could not and did not want to do without, war or no war. It was left to their successors to work out what to do with it, to codify its rules and invent mechanisms for policing its boundaries, and to transform journals from rapid and rough digests of new information into—at least for a while—virtually the exclusive repositories of new science.

Chapter 4: Letters, Print, and Scientific Rivalry under Napoleon's Continental Blockade

> "During this period, so little intercourse was there between France and England, that months often elapsed between the publication of a scientific discovery in one country and its being known in the other. And, perhaps, owing to this state connected with war, less delicacy was observed by men of science in the two countries, in engaging in researches which they had not themselves originated, thereby producing an active competition, which on the whole probably was for the advantage of science."
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> —— John Davy, in *The Collected Works of Sir Humphry Davy*, 1840.[1]
>
> "Ten people who speak make more noise than ten thousand that are silent."
>
> —— Napoleon (attributed).[2]

On 20 November 1806, after its customary long vacation, the Royal Society gathered at Somerset House in the Strand to inaugurate another winter season in the British scientific world. The evening's paper: Humphry Davy's Bakerian Lecture on the "chemical agencies of electricity," the detailed memoir that would lay the muriatic acid controversy to rest.[3] But as Davy delved exhaustively into the technical details of the behavior of different acids and alkalis under the action of the galvanic current, there must have been some among the assembled Fellows whose minds occasionally wandered to other matters. News of events on the European Continent, arriving in letters and foreign newspapers, has thrown London into a state of excitement, and confusion. Napoleon is on campaign with the *Grande Armée*, in the midst of another dramatic reconfiguration of the political geography of Europe, and London is filled with "reports of victories and disasters" (the *Times* newspaper declared that morning), "so numerous, so nicely balanced against each other, but at the same time so unauthenticated, that we should only trifle with the laudable anxiety of the public by repeating them."[4] A few weeks before, Napoleon humiliated Prussia with decisive victories at Jena and Auerstädt, triumphantly entering

[1] *Davy Works*, 5:3.
[2] *A manuscript found in the portfolio of Las Casas, containing maxims and observations of Napoleon: collected during the last two years of his residence at St. Helena* (London: Alexander Black, 1820), 12.
[3] Humphry Davy, "On Some Chemical Agencies of Electricity," *Philosophical Transactions*, 97 (1807): 1-56.
[4] *Times*, 20 November 1806, p. 2.

Berlin less than a month after the Prussian King's rather unwise declaration of war. Now, with Prussia still fighting on in the east and backed up by Russian troops, London waits to hear which way the scales will tilt. "Possibly the most interesting period which has ever occurred in the history of Europe" declared the *Times*, straining to make sense out of conflicting intelligence under the fog of war.[5]

These dramatic events inaugurated a new phase of difficulty for scientific exchanges between Britain and the Continent. The day after the Royal Society gathered to hear Davy's lecture, Napoleon issued his "Berlin Decrees" from the humbled Prussian capital. They began with an explicit prohibition on communications with Britain:

> "The British Isles are declared in a state of blockade. … All commerce and correspondence with the British Isles is prohibited. In consequence, letters or packets, addressed either to England or to an Englishman, or written in the English language, will not pass through the post-office, and will be seized."[6]

The Berlin Decrees launched Napoleon's grand strategy of commercial warfare, a vast blockade aimed at destroying Britain's economy by cutting off its trade. At its most ambitious, the Continental System (as it would come to be called), sought to enforce the total closure of mainland Europe to British shipping and commerce from the Mediterranean to the Baltic.[7] For the remainder of the Napoleonic Wars, it stood between men of science in London and their

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[5] *Times*, 18 November 1806, p. 2.

[6] I translate the decrees from Pierre Clément, *Histoire du système protecteur en France, depuis le ministère de Colbert jusqu'à la révolution de 1848* (Paris: Guillaumin, 1854), 308.

[7] On the Continental System see in the first instance the succinct summary in Geoffrey Ellis, *The Napoleonic Empire*, 2nd ed. (New York: Palgrave Macmillan, 2003), 109-119; the classic studies are Eli Heckscher, *The Continental System: An Economic Interpretation* (Oxford: Clarendon, 1922) and François Crouzet, *L'Economie Britannique et le Blocus Continental*, 2nd ed. (Paris: Economica, 1987); see also Kevin O'Rourke, "The Worldwide Economic Impact of the French Revolutionary and Napoleonic Wars, 1793-1815," *Journal of Global History* 1 (2006): 123-49, and Lance Davis and Stanley Engerman, *Naval Blockades in Peace and War: An Economic History Since 1750* (Cambridge, Cambridge University Press, 2006), 25-52. All these authors have emphasized how the Continental System was never totally effective, with smuggling, corrupt officials, and the lukewarm attitude of many of Napoleon's allies and client states making sure that some of the banned trade unofficially continued, even before Napoleon himself weakened the system in 1810 by granting licenses for trade with Britain.

counterparts in Paris, an imposing barrier dividing the two centers of the scientific world. We have already seen how the disruptions of war could make keeping in touch with foreign developments in galvanism a serious challenge. The Continental System increased the obstruction to an unprecedented degree. It not only officially banned all direct communication between Britain and France, but also closed neutral ports, like the free city of Hamburg, which had been points of access for information to pass between Britain and the rest of Europe. In July 1807, at the zenith of his power, Napoleon met Tsar Alexander I at Tilsit; Emperor to Emperor, in a pavilion on a raft in the middle of the Neimen river, the two agreed an alliance that (to all appearances) cemented the blockade into European political geography. Scientific practitioners surely had good reason to fear for the already fractured unity of their intellectual world.

But the blockade did not kill scientific communications between Britain and France. On the contrary: this short period witnessed one of the most dynamic episodes of concentrated Anglo-French intellectual engagement (and rivalry) in the whole history of science, in which a new field, electrochemistry, was forged out of galvanism through active competition between Humphry Davy in London and the two young French chemists, Joseph Louis Gay-Lussac and Louis Jacques Thenard, in Paris. The two sides vied with each other in a race for experimental discoveries (eight new chemical elements were isolated between 1807 and 1811) as well as in a larger struggle over the future direction of chemical theory.[8] It was a scientific battle, with nationalist overtones—but it depended crucially on exchanges of information between the two sides.[9] In this chapter I recover the human agency that went into sustaining these exchanges,

[8] Potassium, sodium, magnesium, calcium, strontium, barium, boron and (the only discovery unconnected with galvanism) iodine; chlorine was promoted from "oxymuriatic acid" to the status of an element after a debate between Davy and the French chemists.

[9] Gay-Lussac's biographer has remarked on "how eagerly each side looked to the other for every scrap of information." Maurice Crosland, *Gay-Lussac: Scientist and Bourgeois* (Cambridge: Cambridge University Press, 1978), 78.

tracing how the character of scientific communications under the blockade shaped the day-to-day process of making the new electrochemistry in interactions between London and Paris.

I focus in particular on one tool used to keep London and Paris tied together: letters. Although correspondence between Britain and France was officially prohibited by Napoleon's Berlin decrees, scientific letters nonetheless continued to make the arduous journey by a variety of routes. Keeping this information moving required a good deal of hard work behind the scenes. I uncover this through close attention to a particular archive: the papers of London's most important mediator between British and French science, Sir Charles Blagden. Blagden was above all a letter-writer, and I use a close study of his letters, and his remarkable manuscript diary, to reconstruct a picture of his correspondence during the blockade. I show who he was in touch with in France, how he managed to get letters to and fro, and what the rhythm of the exchanges was like. This detailed investigation of the means by which letters were transmitted back and forth reveals that, during the blockade, scientific information usually hitched a ride with other communications, from the diplomatic exchanges of neutral powers, to secretive banking correspondence. Additionally, governments on both sides quietly colluded to keep up scientific links, making special arrangements for letters and other material to pass between London and Paris.

I then move on to consider the timescale and pace of information exchanges under the blockade, recovering the slowed and distorted rhythm that governed the movement of news, experiments, and broadsides of chemical argumentation back and forth across the Channel. Scientific letters, I argue, generally functioned as conversational tools, for swapping news and carrying out virtual dialogs that could synchronize the elite scientific communities of London and Paris. During the Continental system, however, the question-and-answer form of these

exchanges often broke down, complicating the letters' operation in ways which had real consequences for the practical and theoretical development of electrochemistry.

The second half of this chapter investigates the relationship between letters and print, showing how they overlapped and functioned in combination with one another as carriers of information. Here I seek to relate my arguments in this dissertation about how scientific information moved in print to another body of scholarship in the history of scientific communication. That historiography has sought to use surviving letters to reconstruct the "correspondence networks" of particular individuals, from Tycho Brahe in the sixteenth century to Charles Darwin in the nineteenth.[10] Both studies of manuscript correspondence and work on the history of print have taught us a great deal about how to problematize and historicize practices of scientific communication, but as yet the two historiographies have had rather little to say to one another. This is strange, because in their day-to-day work scientific practitioners routinely mixed and matched both letters and print when gathering and making use of information. During the Continental System, they gleaned crucial clues about experiments developed on the opposing side from letters, but also newspapers, journals, and other print sources. I explore this close relationship between print and manuscript communications by following information between Britain, France, and the United States in the aftermath of a major

[10] Among many examples see Lawrence Brockliss, *Calvet's Web: Enlightenment and the Republic of Letters in Eighteenth-Century France* (Oxford: Oxford University Press, 2002); Peter Miller, *Peiresc's Europe: Learning and Virtue in the Seventeenth Century* (New Haven: Yale University Press, 2000); E. C. Spary, "Acting at a Distance: André Thouin and the Function of Botanical Networks," Ch. 2 of *Utopia's Garden: French Natural History from Old Regime to Revolution* (Chicago: University of Chicago Press, 2000); David Lux and Harold Cook, "Closed circles or open networks?: Communicating at a distance during the scientific revolution," *History of Science* 36 (1998): 179-211. For Tycho Brahe see Adam Mosley, *Bearing the Heavens: Tycho Brahe and the Astronomical Community of the Late Sixteenth Century* (Cambridge: Cambridge University Press, 2007). On Darwin's letters see Janet Browne, *Charles Darwin: The Power of Place* (Princeton: Princeton University Press, 2002), especially 10-14, and the Darwin Correspondence Project (http://www.darwinproject.ac.uk/). Recent collaborative projects have sought to move beyond a focus on key individual letter-writers to reconstruct patterns of correspondence on a larger scale: see for example the Electronic Enlightenment project (http://www.e-enlightenment.com/), the University of Oxford's Cultures of Knowledge project (http://www.culturesofknowledge.org/), and Stanford's Mapping the Republic of Letters project (http://republicofletters.stanford.edu/).

discovery, Humphry Davy's isolation of potassium and sodium in 1807. After tracking information about this event from its initial dissemination in letters into a cascade of different print forms, I then focus on two specific exchanges between London and Paris prompted by Davy's discovery. One exchange concerned the practical problem of replicating a new French experiment for the production of potassium in Britain; the other was a debate over how to interpret Davy's new substances. These episodes show how both letters and print functioned together, as different kinds of intelligence-carrying media that nonetheless repeatedly interacted and cross-fertilized with one another in the untidy, information-intensive processes of repeating and extending rival investigators' experiments and arguing over their interpretation. Information could flow across the porous boundary between manuscript and print with relative ease, and texts originally confined to letters could be drawn into the kind of large-scale reprinting phenomena we have witnessed in previous chapters. Pieces of information that a study of "correspondence networks" alone might conclude were the preserve of private letters could turn up in some quite strange and distant places, as we will see.

The Great Communicator: Charles Blagden's Scientific Intelligence Operation

"I cannot thank you enough, my dear friend," wrote the French chemist Claude Louis Berthollet in August 1808, "for the care that you take to keep us informed of [British] scientific news. But for you, it would be as if we were living in another world."[11] Berthollet was exaggerating, but only slightly. The object of his thanks was Sir Charles Blagden, indisputably one of the most industrious mediators between the parallel scientific worlds of London and Paris. When the Continental System began in November 1806, Blagden was fifty-eight years old, and firmly established among the elder statesmen of the British scientific community. He had been

[11] Berthollet to Blagden, 31 August 1808, RS CB/1/1/227.

Secretary of the Royal Society from 1784 until 1797 and remained on its Council, and was widely known as a long-time friend and supporter of the Society's President, Sir Joseph Banks.[12] In his *Life of Johnson*, James Boswell had praised a younger Blagden for his "copiousness and precision of communication."[13] Whatever Boswell actually meant by this, it is clear that the role Blagden carved out for himself in the London scientific world revolved around being a broker and communicator of scientific news. He liked knowing things before other people, and enjoyed taking credit for passing on novel bits of information. A tireless behind-the-scenes enabler of the scientific enterprise, his own original scientific output was fairly modest, and in later life especially he preferred to operate a kind of one-man dealership in other people's ideas and discoveries.[14] Surveying the pattern of his daily activities, recorded in meticulous detail in the diary he assiduously kept for some forty years, one gets the sense of a man propelled by an almost obsessive drive to keep abreast—and if possible ahead—of the latest intelligence in both the scientific and the political spheres.[15] Unmarried and with a modest gentleman's income,

[12] Blagden has received little scholarly attention, perhaps because of the rather limited extent of his published scientific contributions. He trained as a physician at Edinburgh (where he attended Joseph Black's chemical lectures), and spent 1776-80 as an army doctor in America during the Revolutionary War, but he had largely ceased to practice medicine by 1806. During the 1780s he was a close colleague and assistant of Henry Cavendish, and a good survey of this part of his life is provided in Christa Jungnickel and Russell McCormmach, *Cavendish: The Experimental Life* (Lewisburg, PA: Bucknell University Press, 1999), 291-98. See also Frederick Getman, "Sir Charles Blagden, F. R. S." *Osiris* 3 (1937): 69-87, and *DNB*. It was as a strong supporter of Banks during the Royal Society "dissentions" in 1784 that Blagden was elected as one of the Royal Society Secretaries, and although the friendship clearly had its ups and downs (grumbles about Banks are a recurring leitmotif in Blagden's diary), Blagden remained a key ally for Banks, acting as a proxy for him in the physical and mathematical sciences. His main scientific output, published entirely in the *Philosophical Transactions* during the 1770s and 80s, was in studies of heat: he investigated the freezing of mercury, the relationship between salt concentration and the freezing point of water, and the ability of the human body to withstand high temperatures.

[13] This copiousness and precision supposedly led Dr. Johnson to pronounce that "Blagden, Sir, is a delightful fellow." James Boswell, *The Life of Samuel Johnson, LL.D* (London: Charles Dilly, 1791), 2:344.

[14] As early as 1786 Blagden himself boasted in a letter to Benjamin Thomson (the future Count Rumford) that "it is scarcely possible that any discoveries can be made in England without coming to my knowledge by some channel or another." (Quoted in Jungnickel and McCormmach, *Cavendish*, 294). Davy (an idiosyncratic and sometimes insensitive observer, however) later remembered Blagden as "gentleman like in manner, cold & selfish, well informed & of agreeable conversation; but possessed of little talent & no genius. The information he had was generally exact." June Fullmer, "Davy's Sketches of His Contemporaries," *Chymia* 12 (1967): 133.

[15] RS CB/3/1-7, referred to throughout as "*Blagden Diary*." Blagden's diary is possibly the most detailed and rich single manuscript source for day-to-day scientific life in London during the Revolutionary and Napoleonic period,

Blagden had abundant time for this. Bustling and clubbable, conversational and cultivated, devoted to science but no professional, in many ways he was late-Georgian England's quintessential leisured scientific man-about-town. His days were filled with reading the newspapers, writing letters, dancing attendance at aristocratic salons like Holland House (he was politically Whig), paying calls on friends (with whom he discussed in great detail the latest news of the wars and blockade politics), attending the Royal Society's meetings and sitting on its council. Most of all perhaps, he relished his almost daily visits to Joseph Banks's Library in Soho Square—that informal epicenter of the British scientific world—where he never lost an opportunity to chat and exchange the latest news and gossip with the numerous men of science who congregated there and for whom Banks kept a daily open house.

An ardent Francophile since his first experience of the Parisian scientific scene in 1783, Blagden longed for France and the company of his French scientific friends all through the many years when war made that country inaccessible to him. But although he worked hard to position himself as the key go-between bridging the French and British scientific communities, he certainly was not alone in exchanging scientific letters with France.[16] In particular, the Swiss physician and chemist Alexander Marcet, a voluntary exile in London, tenaciously kept up his own correspondence with the Continent, especially with his Genevan friend Marc-Auguste Pictet, editor of the *Bibliothèque Britannique*. Blagden's friend and patron Joseph Banks also

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but it has hitherto been virtually untapped by historians—no doubt because of Blagden's own neglect as a historical figure and because his handwriting in the diary often verges on the illegible. This chapter draws on a general survey of the diary for the period of the blockade, concentrating especially on 1807-08.

[16] On go-betweens in science see Simon Schaffer et al., eds., *The Brokered World: Go-Betweens and Global Intelligence, 1770–1820* (Sagamore Beach, MA: Science History Publications, 2009). On Blagden's connections with France before the Revolution see Danielle Fauque, "An Englishman Abroad: Charles Blagden's Visit to Paris in 1783," *Notes and Records of the Royal Society* 62 (2008): 373-90. Blagden's conveyance of news of the work of Cavendish and Watt to Lavoisier on his 1783 trip was a key moment in the events leading to the longrunning controversy over the discovery of the decomposition of water: David Philip Miller, *Discovering Water: James Watt, Henry Cavendish and the Nineteenth-Century "Water Controversy"* (Aldershot: Ashgate, 2004), 27-57. As we saw in Chapter 2, Blagden lost no time in hurrying over to Paris as soon as the peace of Amiens was ratified in 1802; he also spent time there during the interlude of Napoleon's first exile to Elba in 1814, and was a frequent visitor to France after the end of the wars.

sent or received some French letters—though he appears to have left much of the business of exchanging scientific news with Paris to Blagden.[17] Similarly, Humphry Davy and a few others also occasionally wrote abroad.[18]

Nonetheless, with his excellent connections to the Parisian scientific community, Blagden was the leading scientific intelligence broker between Britain and France during these years, and other British scientific figures relied on him to forward news of their work (and occasionally printed material) to Paris.[19] Blagden was also active in circulating the latest French scientific news in London. In Joseph Banks's library, he would read out extracts from his letters from France to any men of science who cared to listen, and discussions he had there with Davy and other scientific figures were then carefully noted down in his diary—the tool he used to record and order information of all kinds—so that his interlocutor's opinions could be related in his next dispatches for Paris. By sheer necessity, Blagden also became something of an expert on the practical details of sending and receiving information from France under the blockade.

It is therefore especially fortunate that Blagden's Continental correspondence survives virtually complete from late 1807 to 1812, and more fortunate still that it was conducted in a manner that enables us to reconstruct a remarkably detailed picture of the mechanics of cross-

[17] Banks had no first-hand experience of France or French science: his only Continental trip was to the Dutch Republic in 1773, and he apparently never even gained much competency in the French language, presumably relying on friends like Blagden to translate letters for him; Horace Bénédict de Saussure was surprised to find Banks could speak "not a word" of French on meeting him just before his *Endeavour* voyage. Douglas Freshfield, *The Life of Horace Benedict de Saussure* (London: Arnold, 1920), 106.

[18] The extent of Davy's Continental correspondence is hard to trace because little of it has survived on either side of the Channel. While much of the information that reached France on Davy's experiments clearly went through Blagden, Davy occasionally wrote to French journal editors (a few of his letters to Pictet survive). He also exchanged letters with other correspondents in France from time to time (several letters to Ampère are also extant).

[19] For example, Blagden was often responsible for forwarding Davy's latest papers to France, e.g. Blagden to Berthollet, 10 March 1810, RS CB/1/1/234. The official Foreign Secretary of the Royal Society at this time, Thomas Young, appears to have had very little substantive communication with the Continent during the war years (the record of his outgoing correspondence in RS MS/581 contains only formal notices sent to foreign members on their election), and it was not Young but Blagden who was responsible for forwarding the latest *Philosophical Transactions* volumes to the Institut National during the blockade: Blagden to Première Classe de l'Institut, 9 April 1807, MS extract in *Pochette* for 27 April 1807, Archives of Acadèmie des Sciences, Paris.

Channel correspondence during the Continental System.[20] Apparently increasingly worried that his communications would miscarry, Blagden kept draft copies of each of the letters he wrote to French correspondents during the Blockade years, and on them he recorded information on how the letters were sent.[21] With some detective work, these cryptic and often almost illegible notes (together with many clues in the letters themselves, and in Blagden's diary) make it possible to answer the practical question of how scientific information actually travelled between the capitals of the two great belligerent powers.[22]

Blagden's available opportunities for sending letters were limited, and he had to choose his recipients carefully. He had four regular correspondents, all old acquaintances. The most eminent was Claude-Louis Berthollet (1748-1822), a longstanding friend from before the Revolution. The doyen of French chemistry during the Napoleonic period, Berthollet provided a vital connection to the Parisian chemical community and to some degree the whole French scientific world.[23] Two of the other main correspondents were members of the Delessert family,

[20] Much of Blagden's correspondence with France outside this window is clearly missing. Although Blagden's papers have been in general very well-preserved, no letters from Blagden's French correspondents survive at the Royal Society for the period from the beginning of the wars in 1793 to late 1807 (except 3 received in 1806), nor in other repositories of Blagden's papers such as Yale's Beinecke Library. But it is clear from Blagden's diary that he was in touch with his friends in France during this period: for example 3 February 1804 (Madame Gautier) and Apr-May 1805 (Berthollet, Chenevix, Delessert). From late 1807 to 1812, however, the Royal Society archive does appear to be complete for Blagden's four main correspondents. (Since both Blagden and his correspondents assiduously relayed the dates of previous letters sent and received, it is possible to track exchanges quite closely to exclude much reasonable possibility of missing letters or drafts over a given series of dates.) There are no extant letters from mid-1812 until after the fall of Napoleon in 1814, although Blagden's diary again shows that he remained occasionally in touch with France.

[21] Blagden relied on his drafts as a record of exactly what information he had previously sent a correspondent in a particular letter. Very few originals of the letters sent to French correspondents survive, but comparison of drafts with the few that do (or with extracts that were later printed) confirms the closeness of the text of the draft to the text of the letter actually sent.

[22] My analysis is based on a general survey of the letters Blagden sent to or received from France in the period 1806 to 1812—of which there are more than one hundred—with special attention to those of the period that coincides with the examples developed in the second half of the chapter, 1807-1809.

[23] Berthollet was Blagden's exact contemporary, just as Gay-Lussac was Davy's. The two men were born in the same year and their friendship, formed when both were in their middle thirties, remained warm for the rest of their lives, and Blagden was a frequent visitor to Berthollet's country house at Arcueil after the wars. On Berthollet, see Michelle Sadoun-Goupil, *Le Chimiste Claude-Louis Berthollet (1748-1822): Sa Vie, Son Oeuvre* (Paris: J. Vrin,

a scientifically-inclined Parisian banking dynasty: the naturalist, banker, and sugar manufacturer Benjamin Delessert (1773-1847), and his widowed sister, Madame Gautier (1767-1839). Blagden's fourth correspondent was Richard Chenevix (1774–1830), an Anglo-Irish chemist of Huguenot background. Despite being "an Englishman" (as he termed himself), with Berthollet's support and protection Chenevix was permitted to live freely in Paris during 1808 until he eventually returned to England in mid-1809.[24] Although Blagden exchanged a few letters with other Continental men of science—Cuvier, Laplace, Biot, Delambre and Alexander von Humboldt—his regular correspondence was restricted to Berthollet, Delessert, Madame Gautier and Chenevix (until the latter's return to England). They formed a tight group of trusted informants on whom Blagden relied for reports of goings-on in the French scientific world; in return Blagden put them in the privileged position of being able to deliver fresh news from London to Parisian scientific circles.[25]

Among the four, Berthollet was a particularly useful connection. As one of France's foremost chemists and the mentor of the rising young French chemists Gay-Lussac and Thenard, he was an authoritative informant on current discussions and the latest consensus within the French chemical community, as well as the finer details of Gay-Lussac and Thenard's experimental work in their rivalry with Davy. He also enjoyed political influence. As vice-president of the Senate from 1804, Berthollet was one of several scientific men occupying posts

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1977), also Michelle Sadoun-Goupil, "La Correspondance Berthollet-Blagden," in *Actes Du XIIe Congrès International D'histoire Des Sciences*, vol. 6 (Paris: A. Blanchard, 1971), 91-97.

[24] Chenevix had been on the Continent when war broke out in 1803; he later spent time in Madrid before his return to Paris late in 1807; Chenevix to Blagden, 9 November 1807, RS CB/1/3/41. Berthollet is mentioned as his supporter in the French capital in Chenevix to Blagden, 7 September 1808, RS CB/1/3/56.

[25] Most of the letters are in French, although English was sometimes used, particularly in the letters between Blagden and Chenevix.

within the Napoleonic state.[26] More important than any official position or sinecure, however, was his personal closeness to the Emperor—Napoleon was said to refer to him as "my chemist"—which dated from their comradeship during the 1798 Egyptian campaign. During Bonaparte's dramatic escape from Egypt in 1799, Berthollet was among only a handful of individuals chosen to accompany him on his voyage back to France, and afterwards he was one of the key "old boys from Egypt" that the historian Louis Bergeron identified as important actors in the Napoleonic regime.[27] When Blagden was accused of being an English spy after his trip to France during the Peace of Amiens (an inconvenient accusation for a man who positioned himself as a broker in *scientific* intelligence!), Berthollet was apparently able to persuade Napoleon that Blagden's conduct was, as he put it, "not at all suspect."[28]

Improvisation, Special Favors, and State Patronage: How the Sciences Ran the Blockade

Getting letters between Britain and France under the Continental System required special arrangements and privileges of a kind unavailable to most men and women. After Napoleon's closure of the free-city port of Hamburg to British shipping at the start of the blockade in 1806, it effectively ceased to be possible to send letters from Britain into most of the Continent by

[26] The variety of ways science and its practitioners were embedded in the Napoleonic state are traced in Charles Gillispie, *Science and Polity in France: The Revolutionary and Napoleonic Years* (Princeton: Princeton University Press, 2004).
[27] Louis Bergeron, *France Under Napoleon*, trans. R. R. Palmer (Princeton University Press: Princeton, 1981), 53-54. For "mon chimiste," Sadoun-Goupil, *Le Chimiste Claude-Louis Berthollet*, 52.
[28] Berthollet to Blagden, 3 April 1806, RS CB/1/1/218. Writing to Joseph Banks about the accusations, Blagden quoted a letter from Richard Chenevix stating that Napoleon suspected Blagden was a spy after matters on which they had conversed when alone "subsequently appeared in the English Newspapers"; Blagden was quick to assure Banks that he had "never sent anything from France, directly or indirectly, to the English newspapers." Napoleon was reportedly "angry" with both Berthollet and Laplace for welcoming Blagden in France, but Berthollet nonetheless continued to defend Blagden against the accusations, which had apparently been put to rest by 1806. Blagden to Banks, 29 September 1803, and Blagden to Banks, 6 October 1803, *Banks Corr.*, 5:320-321.

regular postal services.[29] Since men of science controlled no routes of their own between London and Paris, they needed to arrange for their letters and news to hitch a ride on other channels of communication. Sending letters therefore became a problem of creating and harnessing links to better-resourced individuals who *did* have access to a method of getting correspondence (and sometimes even larger objects like journals or books) through the cracks in the blockade. Blagden was well-suited to this task: urbane, well-informed, and sociable, he already possessed several connections that could be utilized to keep his correspondence going. There were three methods which he especially used.

The first was to exploit the diplomatic communications of the United States of America. The delicately-balanced position the United States occupied as a neutral power in the Napoleonic conflict before 1812 ensured a fairly regular cross-Channel traffic in diplomatic communications between the two American ambassadors ("ministers") in Paris and London. From 1803-1807, the American ambassador in London was James Monroe—the future President—with whom Blagden quickly succeed in establishing a friendship.[30] Harnessing this personal bond, as well as Monroe's own interested sympathy for scientific endeavors, as early as 1804 the persuasive

[29] Before this, an agreement between the two governments allowed an indirect mail service between Britain and France via neutral Hamburg. Nonetheless, in his correspondence with Blagden just prior to the blockade, Berthollet preferred to use his political influence to create a (short-lived) alternative arrangement, in which Blagden would post a letter to a certain mercantile agent in Hamburg, who would forward it to the French ambassador in the city, who would in turn send the letter to Berthollet at Paris. Berthollet to Blagden, 3 April 1806, RS CB/1/1/218. After the blockade began, there were various attempts, largely unsuccessful, to smuggle English mails into Hamburg via Denmark (neutral until attacked by Britain in August 1807). Postal links between Britain and its then-ally Sweden across the North Sea remained open, and Davy used them to exchange several letters with Berzelius, even when the two countries were nominally at war during 1810-1812. There are isolated instances of this route being used to forward material to France; for example Davy sent a copy of a paper to the *Bibliotèque Britannique* through Sweden in 1809 (Davy to Pictet, 25 May 1809, *Pictet Corr.* 3:159-160). Packet services from England to Portugal also continued, though these were probably unimportant for scientific correspondence. See Howard Robinson, *Carrying British Mails Overseas* (New York: New York University Press, 1964), Vivien Sussex and Sidney Shelton, *Continental Mail Service, 1793-1815: Especially by Yarmouth Packet Boat* (London: East Anglia Postal History Study Circle, 1978), Barrie Jay, "Mail between Britain and France, 1793-1815," *Postal History*, no. 322 (June 2007).

[30] From 1806 Monroe was jointly ambassador with William Pinkney.

Blagden had prevailed upon Monroe to forward some of his letters to Paris.[31] When Monroe eventually returned to the United States in November 1807, Blagden kept up good relations with his successor, William Pinkney, usually calling on him at least once a week to chat about the politics of the blockade (particularly its effects on American shipping), and to sound out opportunities for getting around it provided by the coming and going of American diplomatic couriers.[32] Over the next four years, Blagden delivered many of his Continental letters to Pinkney's residence at Great Cumberland Place, from where they could be taken to France by messengers carrying dispatches to Pinkney's counterpart in Paris, John Armstrong—sometimes, it appears, actually in the protected diplomatic bag.[33] On other occasions, Pinkney sent the letters with trustworthy American travelers about to go to France. A similar arrangement must have been operating in Paris, as Blagden also received letters from his French correspondents through the Americans.[34] Even though Blagden was careful that his letters contained nothing but purely scientific information and expressions of friendship, the arrangement was still apparently rather delicate, being essentially a clever exploitation of the protected status of the diplomatic communications of a neutral state. "I would beg the favour of you to enclose these 2 letters in your paquet [for the American Ambassador in Paris]," Blagden asked Pinkney in a note left on 16 May 1809 with letters to Berthollet and Delessert. He added: "you may be sure they contain nothing improper."[35] Blagden was also careful not to overuse the indulgence. "Be so good as to

[31] Blagden to Monroe, 20 June 1804; Blagden to Monroe, 2 August 1804, James Monroe Papers, New York Public Library Archives and Manucripts.
[32] *Blagden Diary*, 1807-1809, *passim*.
[33] A good example among more than twenty letters Blagden sent by this route is Blagden to Berthollet, 29 December 1807, RS CB/1/1/223, on which Blagden drafted a note to Pinkney left with the letter. On use of the diplomatic bag, Chenevix to Blagden, 30 June 1808, RS CB/1/3/54: "This letter goes by the American minister and I may receive your answer if you can prevail upon Mr Pinckney to forward it by his bag or otherwise."
[34] For example: Berthollet to Blagden, 31 August 1808, RS CB/1/1/227, was received through the ambassador's residence.
[35] Blagden to Berthollet, 16 May 1809, RS CB/1/1/230. Blagden to Delessert, 16 May 1809, RS CB/1/3/257. The draft note to Pinkney is on RS CB/1/3/257.

tell M Biot that I have rec[eiv]d his letter of the 9th of May, and comm[unicate]d it to the men of learning here," he wrote to Delessert in 1808, before adding that Biot should not expect a direct reply "that I may not too much encroach upon the complaisance of the American Minister."[36]

The United States government's pressing need to keep dispatches flowing between diplomats in London and Paris had the side effect of providing a vital (if irregular) link between the scientific communities of the two capitals. But as Blagden was well aware, the American diplomatic wrangling which enabled these helpful exchanges prefigured later difficulties. The threats to American shipping introduced by Britain's own blockade declarations restricting neutral trade with France (issued in response to the Berlin Decrees), and a similar declaration on Napoleon's part, placed America in an impossibly contradictory position with respect to the two major powers, and as early as 1807 Blagden was deeply worried that a British war with America was imminent—"the consequences we have so often deprecated," as he put it to Monroe.[37] His fears, of course, were not realized for several more years, but when the War of 1812 finally arrived it further disrupted scientific communications between Britain and France by rendering useless all Blagden's carefully-constructed arrangements with the American diplomatic corps.[38]

The second route used heavily by Blagden is more difficult to track, but it presents a similar instance of scientific letters hitching a ride along with other information. In this case

[36] Blagden to Delessert, 1 August 1808, RS CB/1/3/224.
[37] Blagden to James Monroe, 26 November 1807, Library of Congress James Monroe Papers (Microfilm); *Blagden Diary*, November-December 1807. Napoleon's declaration of the Continental System at Berlin (which he justified as a response to an existing British blockade of France) had ordered that any neutral shipping which had previously traded at a British-controlled port would be excluded from commerce with ports under French control. As part of their response to the Berlin Decrees in the Orders in Council of 10 January and 11 November 1807, the British had then declared any neutral ship trading with France "lawful prize" (i.e. subject to capture by the Royal Navy or British privateers) unless it first touched at a British port and paid a duty. Napoleon countered this by declaring any neutrals touching at British ports lawful prize in his "Milan Decrees" in December. This effectively made neutrals trading with either power subject to seizure by the other. President Jefferson and Congress countered by joining the blockade game themselves, passing a blanket Embargo Act in 1807 prohibiting all American trade with both Britain and France; helpfully for scientific exchanges, an exception was made for diplomatic vessels. This act remained on the books until March 1809. Davis and Engerman, *Naval Blockades in Peace and War*, 68-108.
[38] William Pinkney left Britain in 1811, after which Blagden was forced to rely on other routes.

commerce, rather than diplomacy, enabled the journey. Again, Blagden's carefully-cultivated friendships played a key role: the Delesserts, his scientific friends in Paris, were also one of the major financier families of the Napoleonic era. As a key part of an international group of Huguenot financiers, even during the height of the blockade the Delesserts somehow maintained a connection (possibly clandestine) with England, and this allowed Blagden to send them letters through a Genevan banker based in the City of London, Charles de Constant (a cousin of the writer Benjamin Constant).[39] Other sporadic international banking and mercantile links were maintained across the frontiers of war during the blockade—in 1810 Sir Joseph Banks was even able to remit money to Paris to buy medicinal water for his gout—and another prominent Parisian banker, Alphonse Perregaux, also seems to have been tapped as an intermediary for scientific communications between France and England.[40] Blagden also probably sought the help of another friend, the great London financier Henry Hope, who along with the Baring brothers had financed the Louisiana Purchase in 1803, and who continued to maintain agents on the Continent. However this commercial correspondence was carried on—whether by smuggling or through agents based in neutral countries—the connection did prove a reliable one for Blagden until early 1810, after which he had to rely on the Americans until that option too became unsustainable. In most cases he used this route for corresponding with the Delesserts only, sometimes forwarding information to Benjamin Delessert or his sister Madame Gautier with the expectation that they would pass it on to Berthollet and other French scientific figures.[41]

[39] Benjamin Delessert was made regent of the Banque de France in 1802. Charles de Constant's name is recorded on many of Blagden's letters, and in his diary Blagden noted numerous visits to de Constant to drop off or receive letters (e.g. *Blagden Diary* 11 March 1807).
[40] See Delambre to Banks, 22 November 1810, BL Add. MS 8100, fols. 65-66; Chenevix also sent a letter to Blagden through Perregaux: Chenevix to Blagden, 9 November 1807, RS CB/1/3/41. Blagden too was also able to remit money to Paris, to Delessert through de Constant in 1805: RS CB/1/3/205.
[41] As, for example, he did with the news of Davy's discovery of potassium and sodium in 1807 (for which see below).

There was one further important route for scientific communication between Britain and France that was exploited by the two scientific communities, although Blagden used it only occasionally for his own correspondence. This was the sole official cross-Channel link between the British and French states during the blockade: the "cartel" ships which sailed irregularly between Plymouth in England and Morlaix in Brittany to transport released prisoners of war back to France.[42] With the permission of the governments on both sides, the cartels were also used to keep up a discreet semi-official connection between the two centers of French and British science, the Institut National and the Royal Society, with the *Mémoires* of the one and the *Philosophical Transactions* of the other exchanged on the cartels along with packets of letters, books, journals and other scientific material.[43] Though Blagden was heavily involved with these exchanges, the implementation was left to another gentleman of science, the entomologist Alexander McLeay, a career civil servant who fortunately happened to hold the key position of Secretary of the Transport Office, the naval department responsible for organizing the cartels.[44] It probably also helped that Blagden was good friends with the Whig politician Charles Grey, who was successively First Lord of the Admiralty and Foreign Secretary during 1806-07 (he was later Earl Grey, and Prime Minister from 1830-34).[45] On the French side, there was an arrangement with the Ministère de la Marine (the head of the Navy), apparently negotiated by Jean Baptiste Delambre, the Institut's perpetual secretary for the Mathematical Sciences. Once

[42] Napoleon allowed few Britons imprisoned in France to go home by the same route. Due to the failure of the governments on both sides to agree to a regular system of prisoner exchanges, the French prisoners were mostly chronically sick or disabled men deemed to be unfit for military service. Gavin Daly, "Napoleon's Lost Legions: French Prisoners of War in Britain, 1803-1814," *History* 89 (2004): 361-80.

[43] For example, on 29 February 1808 the Institut recorded its reception of the two volumes of the *Philosophical Transactions* for 1807 from the Ministère de la Marine (Admiral Denis Decrès): *PVSA* 4:23.

[44] Blagden suggested Banks use a particular cartel ship to send the *Philosophical Transactions* to the Institut in December 1807: *Blagden Diary*, 3 December 1807. McLeay's involvement is also evident from the diary (e.g. 4 February 1808), and drafts of a few letters show Blagden sent them by the cartel through McLeay, e.g a letter to Laplace: "Left Mar 14 1810 at the Transport Office, enclosed in a note to Mr. McLeay." (14 March 1808, RS CB/1/4/226).

[45] Blagden's many calls on Grey—then Viscount Howick—are recorded in Blagden's diary.

unloaded from the cartels at Morlaix, the packets from the Royal Society were forwarded to the Ministry of the Marine in Paris by official government messengers, where they were transferred to the Institut.[46]

The cartels were usually less expeditious than Blagden's other channels; the ships sailed irregularly, and at times the prisoner exchanges were suspended altogether or the cartel was prevented by the French from entering the harbor at Morlaix.[47] Nonetheless, occasionally Blagden did send letters by this route—"Given this day to Sir Jos. Banks, to be put up in the parcel for [the] Institute to go by next Cartel," he recorded on a draft to Berthollet in February 1809—and the cartel exchanges proved invaluable for conveying bulkier material (journals, *Transactions* volumes, books, and even packets of seeds) which American diplomats were not willing to transport.[48] Similarly, much of the British material subsequently reprinted by the *Bibliothèque Britannique* appears to have arrived in France on the cartel ships, through an arrangement the editor Marc-Auguste Pictet negotiated with Talleyrand.[49]

[46] Chef de la 5e Division du Ministère de la Marine to Secrétaire Général de l'Institut, 20 Ventôse an 13 (11 March 1805), in *Pochette* for 20 Ventôse an 13, Archives of Acadèmie des Sciences, Paris; this shows that the arrangement with the cartels was apparently up and running even before the Blockade began.

[47] For example Blagden to Delambre, 1 August 1808, CB/1/3/214: "The Memoires of the Institute, & other books, which you mention as having been sent to Morlaix to be forwarded by a Flag of Truce [i.e. cartel ship], are probably still there; as our last flag, tho' it brought French prisoners to be returned, was not suffered to land them nor any thing else, nor to receive anything from the shore. If your Govt. should open the comm[unicatio]n again, we will make use of the opportunity, to send you the Phil. Trans &c." (Blagden sent this via the American ambassador.) Similarly in March 1808 the Ministère de la Marine wrote to Delambre stating that all communications had been suspended and that Delambre's package for the Royal Society (which included the official letter to Davy announcing his award of the Prix du Galvanisme) would have to wait at Morlaix until this situation changed. Ministère de la Marine to Delambre, 14 March 1808, *Dossier Biographique* for Humphry Davy, Archives of Acadèmie des Sciences, Paris.

[48] Blagden to Berthollet, 4 February 1809, RS CB/1/1/228. No doubt others in the London scientific community who did not have the other correspondence options Blagden enjoyed also made use of this route to send scientific letters to France.

[49] David Bickerton, *Marc-Auguste and Charles Pictet: The Bibliothèque Britannique (1796-1815) and the Dissemination of British Literature and Science on the Continent* (Geneva: Slatkine Reprints, 1986), 352-58.

Approval of these semi-official communications with the enemy was no doubt sought by McLeay or Blagden from the British Admiralty or government ministers.[50] On the French side, the link certainly had Napoleon's direct sanction. These purely learned communications [*communications littéraires*], authorized by the order and the example of His Majesty [Napoleon] and designed… to propagate enlightenment and widen the sphere of knowledge, have always been treated as an exception which I am willing to maintain," the Minister of the Marine Admiral Decrès informed Delambre in 1807.[51] For his part, Delambre later assured Banks that Napoleon "had expressed in the most positive terms his intention of facilitating correspondence between the Institut and the Royal Society."[52] The intelligence reports that Napoleon's feared minister of Police Joseph Fouché regularly prepared for the Emperor also show a clear awareness and sanction of the practice.[53]

This raises two questions. Why was this striking exception to the prohibition on communications permitted, and how did men of science manage to position themselves so that state agents, from the Ministère de la Marine to port officials at Plymouth—not to mention diplomats belonging to neutral powers—were persuaded to go to the trouble of maintaining these special arrangements? The first question is perhaps best addressed by looking at the unusual

[50] I have not been able to trace any written record of the British government's consent to the exchanges, which may have been sought and given by word of mouth only. The official letter-books of the Transport Office do, however, record numerous orders for the transport of various "parcels" to Morlaix aboard the cartels, and comparison with Blagden's Diary reveals some of these were evidently scientific material destined for the Institut National. For example, copy of letter to Captain James Rogers RN, Plymouth, in "Letters to Agent at Plymouth," National Archives, Kew, ADM 98/243.

[51] Ministère de la Marine to Delambre, 14 March 1808, *Dossier Biographique* for Humphry Davy, Archives of Acadèmie des Sciences, Paris.

[52] Delambre to Banks, 8 March 1812, reproduced in Gavin de Beer, *The Sciences were Never at War* (London: Nelson, 1960), 191-93, from MS in the possession of the Brabourne family. This letter mentions the assistance of the Minister of Police with forwarding scientific communications, also evident in Delambre to Banks, 18 March 1813, BL Add. MS 8100, fols. 146-47.

[53] For example, Ernest D'Hauterive, ed., *La police secrète du premier empire: bulletins quotidiens adressés par Fouché à l'Empereur*, vol. 4: 1808-09 (Paris: Clavreuil, 1963), 462 (bulletin for 8 December 1808), and 572 (bulletin for 13 March 1809); the second of these shows that (unsurprisingly) letters for England were read by police agents before being put aboard the cartel.

status of scientific information exchange, which was held both sufficiently important that it *deserved* a special exception (no special efforts appear to have been made to facilitate international communications between classical scholars, historians, antiquarians, folklorists, or other non-scientific intellectuals, for example), and also sufficiently *benign* that making that exception would not compromise the interests of the state in any significant way. The argument for an exception was built around the idea that rapid scientific progress—"to propagate enlightenment and widen the sphere of knowledge," as the Ministère de la Marine had it—depended upon a well-connected international scientific community. The natural sciences were considered the archetypal international intellectual endeavor, and this assertion was epistemologically grounded: new facts of natural knowledge, the pure stuff of scientific intelligence, were viewed as essentially neutral objects innately predisposed to "propagate" or "diffuse," and they would work their best effect in the world if allowed to cross borders freely. "In natural science there is one language universally intelligible—the language of facts; it belongs to nature, and it is permanent as the objects of nature; it is the same to the citizen of Paris and London," opined Humphry Davy.[54] The more new facts became widely known, the more they would breed more new facts, and so advance the progress of natural knowledge and the improvement of humankind.[55]

What is perhaps a little stranger to the modern eye is that the governments of the two most inveterate adversaries of the Napoleonic wars permitted a relatively free flow of scientific facts and other information to pass to their enemies. The belligerent states had little direct use for experiments in galvanism and electrochemistry, but, more surprisingly, the cartel ships were

[54] Quoted in John Davy, *Memoirs of the Life*, 1:372.
[55] Lorraine Daston has dated the emergence of "an ostensibly amoral nature, to which controversial political and social questions might be referred for allegedly impartial arbitration" to around the end of the eighteenth century: Lorraine Daston, "The Nature of Nature in Early Modern Europe," *Configurations* 6 (1998): 171; also Lorraine Daston and Peter Galison, *Objectivity* (New York: Zone Books, 2007).

used to exchange copies of the *Nautical Almanac*—Britain's most militarily-important product of the sciences because every Royal Navy captain relied on it for navigation—and its French equivalent, the *Connaissance des Temps*.[56] By the First World War, free exchanges of strategically-valuable scientific information would be almost unthinkable. Their existence during the Napoleonic Wars perhaps says more about the prestigious but essentially small, weak, and (in practical terms) still relatively useless nature of the scientific enterprise in this period than it is evidence of a nobler and healthier attitude to the status of scientific knowledge in wartime circumstances.[57] Moreover, from the point of view of the state, the exchanges in operation here were maybe rather analogous to the traditional arrangements for the exchange of prisoners of war which the cartels were supposed to facilitate: an agreement between belligerent powers to engage in a trade in scientific information which would be mutually beneficial for both sides.[58] In this view, the scientific enterprise was not a zero-sum game, and Britain and France—despite intense national rivalry—would both benefit from a general sharing of scientific facts and arguments.[59]

The second question of how men of science were in a position to carve out this rather good deal for themselves can be answered more easily. The exchanges were the product of an unusual collusion of circumstances on both sides of the Channel. In Britain, Joseph Banks had made himself indispensable as an informal advisor on scientific matters to successive British governments; with the assistance of scientific civil servants like McLeay, he and Blagden seem

[56] Delambre to Banks, 2 March 1808, BL Add. MS 8100, fols. 29-30.
[57] The latter view was the contention of de Beer, *Sciences were Never at War*.
[58] As previously mentioned, in reality Napoleon did not reciprocate the British return of French prisoners during the later period of the wars for political reasons.
[59] On the tensions between scientific neutrality and national loyalty in this period see Loraine Daston, "Scientific Neutrality and Nationalism under Napoleon," in *Solomon's House Revisited*, ed. Tore Frangsmyr (Canton, MA: Science History Publications, 1990), 95-119; also Elise Lipkowitz, "Seized natural-history collections and the redefinition of scientific cosmopolitanism in the era of the French Revolution," *British Journal for the History of Science* 47 (2014): 15-47. A rather more rose-tinted view is presented in de Beer, *The Sciences were Never at War*.

to have had little trouble securing the necessary blessing of the state.[60] In France, the position of men of science was even more favorable, given not only Napoleon's own personal enthusiasm for the sciences but also his particular closeness to several scientific figures, especially Berthollet, Laplace, and the astronomer Gaspard Monge. Furthermore, the Napoleonic regime was institutionally disposed to aid the progress of French science: the status of the Institut National as the learned arm of the state closely tied French scientific achievements to the greater glory of the Empire.

Nonetheless, though men of science in Paris and London made no great secret of being in touch with one another, publishing extracts from letters in journals and reading them out at meetings of the Royal Society or the Institut National, the actual mechanics of the exchanges were kept very quiet, never publically discussed on either side. Moreover, despite an overall blessing at the highest levels, it is not clear how far the exception extended in individual instances, especially when individuals chose to write privately rather than through the semi-official prisoner-of-war-ship arrangement. Blagden and his correspondents certainly often wrote with a variety of precautions. In 1808 Madame Gautier apologized to Blagden for not having been able to forward more "news of your friends,"

> "…who are distressed that they are not able to keep up more communication with you. It is almost as impossible as it is dangerous. To give even an occasional sign of life and token of friendship is not permitted."[61]

Correspondents clearly worried about communications being intercepted. Often letters were unsigned—leaving the recipient to recognize the handwriting—and occasionally were directed to fictitious destinations (some sent to Blagden through the Americans were addressed to him at

[60] See John Gascoigne, *Science in the Service of Empire: Joseph Banks, the British State and the Uses of Science in the Age of Revolution* (Cambridge: Cambridge University Press, 1998).
[61] Gautier to Blagden, 7 April 1808, RS CB/1/4/29.

"New Yorck").[62] Even the names of third parties were disguised: Berthollet was "our friend of Arcueil," while in one letter Davy was obliquely referred to only as "the savant who has distinguished himself by his galvanic decompositions."[63] Despite some semi-official state sanction, cross-Channel scientific exchange was carried on in a state of constant uncertainty, amid real obstructions and always with doubtful expectations of success.

Although the three routes just described were Blagden's primary links to France, during these years men of science experimented with all kinds of opportunities to get information to each other. The blockade had warped what we might call the temporal geography of communication—the relation of geographical distance to travel time—in rather strange ways, and letters were sent by routes that would have been absurd during peace but which were now plausible alternatives. Blagden tried sending a letter from London to Paris via Bilbao in northern Spain, while Berthollet and Chenevix both took the even more drastic step of writing to Blagden via America.[64] On a few other occasions Blagden used Austrian diplomatic channels as an alternative to the Americans, drawing on the help of a scientifically-inclined "Dr. Gruber," the chaplain of the Austrian Legation in London.[65] In sum, under the Continental system letters passed back and forth between Britain and France by a variety of means which were often simply creative improvisations invented by a few committed individuals under very difficult circumstances. For most people in Britain and France, exchanging letters across the Channel would have been out of the question: men of science occupied a special position and scientific communications were accorded special status. Yet these ad-hoc arrangements also demonstrate

[62] Such as Benjamin Delessert to Blagden, 29 June 1808, RS CB/1/3/222.
[63] Blagden to Chenevix, 3 February 1808, RS CB/1/3/43; Blagden to Gautier 29 April 1808, RS CB/1/4/50.
[64] Blagden to Chenevix, 7 April 1808, RS CB/1/3/45. Berthollet to Blagden, 29 January 1808, RS CB/1/1/221. Chenevix to Blagden, 7 September 1808, RS CB/1/3/56.
[65] For example, Blagden to Berthollet, 4 February 1808, RS CB/1/1/253. Blagden also apparently sought the help of Baron Jacobi, the Prussian ambassador in London, to forward scientific letters to the Continent, though this presumably ceased to be a useful channel after Napoleon's subjugation of Prussia at the end of 1806: *Blagden Diary*, 21 February 1804, 9 May 1805.

the fragility of the ties that bound the scientific world together. Never had the scientific communities of London and Paris had to work so hard to remain in touch with one another, and their exploitation of other people's circuits of communication to do so—from diplomats to prisoner of war exchanges—meant that the speed, rhythm and reliability of their exchanges were entirely at the mercy of circumstances beyond their control.

Virtual Conversation: Letters as Tools of News and Community

It was an eighteenth-century commonplace that letters were, as one how-to guide put it, "the copies of conversation," textual mirrors of an imagined oral dialogue between two voices separated by distance.[66] Conversation was Blagden's element, and his communications with his French correspondents—often incorporating technical content, but never with the formality of a scientific publication—were in many ways strongly analogous to the innumerable discussions he enjoyed with colleagues in the comfortable atmosphere of Joseph Banks's house. When Blagden read out letters received from Berthollet and other French informants to the men of science he met at Banks's library, he was bringing the voices of his correspondents into the heart of the London scientific community. Just as when letters from absent relatives were read aloud in the home, or the text of eighteenth-century newspapers declaimed in coffeehouses or taverns, so scientific news transmitted as written text could be made oral and performative, moving the latest experiments, discoveries, ideas or gossip back into the realm of word-of-mouth circulation which they had occupied in Paris.[67] Similarly, when Blagden's own letters reached France, the

[66] Thomas Cooke, *The Universal Letter-writer; or, New Art of Polite Correspondence* (London: Wilson, 1812), 18. The notion of letters as written conversation goes back to Cicero; see also *Eve Tavor Bannet, Empire of Letters: Letter Manuals and Transatlantic Correspondence, 1680-1820 (Cambridge: Cambridge University Press, 2005).*

[67] One example of the phenomenon is *Blagden Diary*, 8 May 1808, 29 September 1808, when Blagen read Cavendish and Davy his letters from Berthollet. Blagden probably generally preferred to read from his letters (rather than pass them around for others to peruse) because he only considered certain passages to be news intended for general circulation; the full letter might contain more personal information and in its entirety was a private dialog

news they contained quickly spread in Parisian circles of scientific conversation.[68] Like face-to-face conversations, letters allowed discussion of more personal matters which would be inappropriate for scientific print. When Davy fell dangerously ill for two months following his discovery of the alkali metals in 1807, Blagden passed on updates on his condition to French correspondents.[69] The next year, after Davy's arch-rival Gay-Lussac suffered a painful laboratory accident in which an explosion of potassium in water caused pieces of caustic potash to fly into his eyes, news of the temporary suspension of the Frenchman's work flowed the other way.[70] The scientific communities of London and Paris were the two most populous and active in the world, but both revolved around a tightly-concentrated (and still relatively small) nucleus of individuals who all knew one another and among whom information circulated orally before it passed into print, exchanged and discussed at venues such as the Royal Society, the Institut National, Banks's library, or the "Society of Arcueil" group that gathered at Berthollet's country house just outside Paris.[71] The virtual conversations conducted through letters tied these two localized oral cultures together, informally synchronizing them with one another. Through Blagden's correspondence, Berthollet's Arcueil circle (and more generally the Institut National) had a virtual presence in London; similarly, important news harvested by Blagden at Joseph Banks's house and elsewhere was echoed and talked over in the French capital. The small number of correspondents still exchanging news across the Channel during the blockade knew

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between Blagden and his correspondent. Furthermore, reading the letters out loud clearly drew the attention of listeners to Blagden's access to and control over incoming French news. On the newspaper as basis for oral performance see Hannah Barker, *Newspapers, Politics and English Society, 1695-1855* (London: Longman, 2000), 53-55; on the reading aloud of eighteenth-century letters and the blurred boundary between letters and speech see Bannet, *Empire of Letters*, 46.

[68] Blagden's Parisian correspondents sometimes mentioned having heard or seen news contained in letters Blagden had addressed to other correspondents, e.g. Chenevix to Blagden, 2 February 1808, RS CB/1/3/50.

[69] Blagden to Gautier, 5 December 1808, RS CB/1/4/52. Blagden to Chenevix, 2 February 1808, RS CB/1/3/43.

[70] Berthollet to Blagden, 20 June 1808, RS CB/1/1/225.

[71] Maurice Crosland, *The Society of Arcueil: A View of French Science at the Time of Napoleon I* (Cambridge, MA: Harvard University Press, 1967).

they were simultaneously writing to individuals and to the entire scientific community on the opposite shore.

But when looked at more closely, under the pressure exerted by the blockade, this powerful metaphor of letters as virtual conversation begins to manifest some points of stress. One way to get an initial measure of these exchanges is to look at the time interval from when a letter was written to when it reached its recipient. Before and after the wars, letters would usually cover the distance between London and Paris (over 250 miles by road and sea) in about five days, as Blagden's correspondence testifies.[72] This was sufficiently fast that the delay had very little relevance for all but the most time-critical kinds of information (stock prices, perhaps, or major political events). Were it not for the war and the blockade, an important piece of scientific news originating in London or Paris could easily have been transmitted to the other city in under a week, a basically insignificant interval in comparison with the customary pace of scientific work and publication.

Using the bare measure of the number of days from the writing to the receipt of a letter, the situation under the Continental System does not always seem quite as bleak as the laments of men of science might imply, though the increased delay is certainly very marked. Even a man as well-connected as Berthollet would have come to expect that his letters would only be read—at best—over a month after they were written, and that sometimes they would take much longer, or not arrive at all.[73] Yet counting days like this actually considerably underestimates the

[72] For example, Delessert wrote to Blagden from Paris in a letter dated 18 April 1816, delivered in London on 22 April (RS CB/1/3/243). Occasionally letters might take as little as three days, as when Delessert wrote to Blagden on 21 October 1815 and Blagden had the letter on the 24th (RS CB/1/3/240). Mail packets sailed from Dover to Calais and back twice weekly in 1791, and four times a week after the wars: Robinson, *Sending British Mail Overseas*, 59.
[73] It is possible to get a fairly good sample of the transit times of letters written from France to Blagden using the postmarks some of them acquired when they were posted to Blagden after they were brought to Britain, along with notes of receipt in Blagden's diary and dates of arrival occasionally given in his replies. Transit times from Paris to London ranged from a minimum of three weeks to (in one extreme case) five months. Because in most instances

difficulties of scientific communications under the Continental System, and the consequent distortions in the alignment of the tracks of scientific life in London and Paris. In reality, sending letters could not be done at will, but depended on the appearance of infrequent and irregular "opportunities" (as Blagden and his correspondents called them) for conveyance. Even with good connections and careful efforts to sound out possible routes, frequently one simply had to wait. "I have been waiting for an opportunity to send this which has lain open ever since I began it," bemoaned Richard Chenevix to Blagden after having begun a letter in November 1807; the letter only reached Blagden in London in mid-March of the next year.[74] Often, therefore, correspondents only wrote letters when a specific conveyance became available: "an American ship is about to depart, charged with dispatches, and this letter will surely reach you, I hope, by that route," as Madame Gautier informed Blagden on one occasion.[75] We consequently have to be careful not to picture these exchanges as a stable channel by which letters could be slowly but regularly and consistently conveyed back-and-forth, but rather as Blagden and his correspondents saw them: a messy, unpredictable stop-start process that required constant attention.

The scientific communities of London and Paris, used to the regular and reliable postal services of the eighteenth century, now found themselves at the mercy of arbitrary circumstances like the departure of American diplomats or occasional sailings of prisoner-of-war cartels. Scientific letters *had* been quietly-running engines of conversation; the blockade blew the hood off and exposed the unreliable mechanics of conveying news. Blagden and others spent a good deal of time and effort getting their hands dirty oiling the cylinders, but sometimes even their

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only the drafts of Blagden's letters to France survive, it is harder to be precise about how long letters took in the other direction, but the rhythm of this side of the exchange can be partially recovered from correspondents' replies.

[74] Chenevix to Blagden, 30 November 1807 (added to 15 December, London Postmarked 10 March 1808), RS CB/1/3/42.

[75] Gautier to Blagden, 5 July 1809, RS CB/1/4/34.

best attempts were insufficient, and the voice of Paris fell silent in London as one side of the exchanges broke down altogether. For example, none of Blagden's French correspondents were able to send him a letter for over seven months between early September 1808 and mid April 1809.[76] The order in which letters arrived, or even whether they would arrive at all, could be unpredictable. Frequently letters were sent and arrived in clusters, essentially determined by when a suitable vessel was going to sail, and the arrival of a cache of letters from France for Blagden and others would be an event in London scientific circles.

To take a brief concrete example of the Paris-London link at work: at the beginning of July 1808 an American ship called the *St. Michel* left France for England, bearing amongst its diplomatic dispatches several letters to Blagden from Berthollet, Delessert, Chenevix, and Madame Gautier.[77] Hearing from Ambassador Pinkney that the *St. Michel* remained at anchor down in Falmouth but was soon to return to France, Blagden quickly composed his own letters in reply. The letters then made the return passage aboard the same vessel, together with more American dispatches and the first copy to reach France of John Dalton's seminal treatise on atomism, *A New System of Chemical Philosophy*, a gift from Blagden to Berthollet.[78] When the *St. Michel* sailed homeward from France in September, the resourceful Chenevix used the ship to

[76] This was also the "dark period" for scientific journal exchanges mentioned in the previous chapter. Richard Chenevix wrote to Blagden on 7 September 1808 (RS CB/1/3/56), and none of his regular correspondents were able to get a letter through again until a cluster of letters from Berthollet, Delessert and Madame Gautier which were written 16-19 April (RS CB/1/1/229, CB/1/3/244, CB/1/4/32). That no intervening letters arrived in London is confirmed by Blagden's drafts of letters to France during this period, which mention the dates of the last letters received.

[77] *Blagden Diary*, 19, 20 July 1808. Chenevix's letters were 15 May 1808, RS CB/1/3/52 and 30 June 1808, RS CB/1/3/54; their receipt by Blagden via the St. Michel is in Blagden to Chenevix, RS CB/1/3/53. The other letters were Delessert to Blagden, 29 June 1808, RS CB/1/3/222; Gautier to Blagden, 25 June 1808, RS CB/1/4/30; Berthollet to Blagden, 20 June 1808, RS CB/1/1/225.

[78] *Blagden Diary*, 1 August 1808. Blagden's letters were Blagden to Chenevix, 28 July 1808, RS CB/1/3/53; Blagden to Berthollet, 1 August 1808, RS CB/1/1/226; Blagen to Chenevix, 1 August 1808, RS CB/1/3/55; Blagden to Delessert, 1 August 1808, RS CB/1/3/224. He also sent a letter to Delambre.

send Blagden another letter via the United States, which "finally reached me," Blagden remarked dryly two and a half months later, "after having made a rather long detour."[79]

This made for a very broken-up rhythm of conversation: syncopated and staccato as well as slow-paced. Letters continued to function as tools to exchange and synchronize news circulating orally within the British and French scientific worlds, but now synchronization was delayed and very irregular; when an occasional burst of information arrived, it would represent what Davy's French rivals had been doing or thinking one, two, or even three or more months ago, rather than what they were doing the previous week. As we shall see, this unusual pattern of long silences followed by bursts of news from multiple sources had a real knock-on effect on basic practices of science like repeating experiments and arguing over points of interpretation. And in many cases—for a genuine process of *exchange*—what mattered was not so much the transit of a single letter but rather the round-trip of the letter and a reply, and that was an even more stretched and unpredictable interval, requiring two fortunate "occasions" rather than one.

The letters themselves manifest an acute consciousness of the many problems that beset them, usually beginning with a flurry of dates of letters previously sent and received as Blagden and his correspondents sought to help each other synchronize their unreliable channels of communication and establish what they knew or did not know about one another. Methods of conveyance were explicit topics, as was their fragility: "I write at all events," Chenevix began one letter to Blagden, "and abandon my letter to the most likely chance of its reaching you."[80] Complaints about being starved of scientific news—a kind of information underload—were common: "I have had no news from England for near three months and am in total ignorance of

[79] Chenevix to Blagden, 7 September 1808, RS CB/1/3/56. Blagden to Chenevix, 19 November 1808, RS CB/1/3/57.
[80] Chenevix to Blagden, 30 November 1807, RS CB/1/3/42.

all that you have been doing there in science," Chenevix grumbled to Blagden in 1808.[81] Berthollet had perhaps a little more philosophical resignation: "It costs me much, my dear friend, to receive your news so rarely and to give you so little of my own; it is the toll exacted by the circumstances in which we find ourselves."[82] Both sides were acutely aware that only very tenuous links kept them on anything like the same intellectual paths.

The unique conversational features of scientific letters—their directness, informality, and privacy—ensured they remained important vehicles for moving information internationally. But their place in the wider landscape of scientific news was increasingly unstable during the two decades after the French Revolution. Not only was the key link between London and Paris disrupted, but everyone exchanging letters was also living through the transformations in scientific print discussed in previous chapters, especially the use of new monthly journals as a fast-paced forum for quickly announcing experiments and conducting scientific discussions, and the surge of monthly and even daily scientific news appearing in those journals and in the newspapers. Since the men and women who used scientific letters to exchange news never did so in isolation, but while swimming along in a current of information swollen with ever more scientific print, it is quite clear that discussing letters as a coherently-bounded closed system—the traditional "correspondence network"—is artificial, useful only up to a point. Information in scientific letters was always put to work as part of a larger whole, interacting with print in productive and sometimes rather unusual ways.

Scientific letters and print operated alongside and in intersection with one another in the grand game of distributing and keeping up with the latest scientific information. Letters often found their way into print when fresh news from abroad was published in the destination country

[81] Chenevix to Blagden, 15 May 1808, RS CB/1/3/52.
[82] Berthollet to Blagden, 19 April 1809, RS CB/1/1/229.

in order to circulate it more widely. Extracts from a few of Blagden's letters found their way into French journals, while French letters were sometimes used as sources for information in British publications. Print could also find its way into letters, both figuratively and literally: letters might discuss recently-arrived publications containing news or scientific papers, and printed sheets would sometimes be enclosed with letters. Print and letters cross-fertilized in the scientific communities' efforts to source fresh information from abroad, to order it, and to put it to use.

When more detailed printed descriptions of new experiments made in France or Britain eventually arrived in the other country, they were naturally fitted into and compared with information already acquired through letters. Parisian men of science would compare snippets of information furnished by Blagden and other correspondents and labor to correlate them with the latest translations of British journal articles appearing in the *Bibliothèque Britannique*, while scientific news arriving in London in a French newspaper would be amalgamated with intelligence from letters brought over by American diplomats to construct an often frustratingly-imperfect picture of what Paris was up to.

Incomplete information meant that even the best-informed never ceased to live in a cloud of unanswered questions. Some of these were questions particular to the special circumstances of the blockade. Others were of the kind that ordinarily arise between two scientific groups separated in space and in time but engaged in close cooperation and close rivalry. These questions usually vanish relatively quickly when communications are swift and reliable; under the blockade, they lingered—annoyingly. Has the latest discovery in our country already been made across the Channel, and have they already published details? What was Davy's response to the news of our recent experiments which we sent out from Paris two months ago? Has Gay-Lussac recovered his sight enough to begin working again after his ghastly laboratory accident?

Did my letter reach its destination, and when? Have the French got a copy of my paper yet, did they reprint the text as expected, and what were the editor's comments? Is this newspaper report a good guide to their objections to my ideas? What exactly was in that issue of the *Annales de Chimie* that never reached us? How far can we trust the strange details in this brief news report of Davy's latest Royal Society lecture in *Nicholson's Journal*—are these peculiar density figures for potassium perhaps misprints or mistakes? Are the French going to prevent our cartel ship and its cargo of letters and journals from landing at Morlaix yet again? Has Berzelius in Stockholm been sending his results to Paris as well as to us in London? Is my "pure potash" the same thing as Davy's? What exactly did Berthollet *mean* by that cryptic comment on ammonia in his letter to Blagden? Have my experiments been repeated abroad yet, and do my rivals give their assent to my results? Are the facts with which they are reasoning the same as my facts? And so on, as the tyranny of distance tightened its inescapable grip.

As the rest of this chapter will show more concretely, thoughts like these and many others undoubtedly passed through the minds of the scientific communities of Paris and London. Some of the work of sorting them out could be left to adept go-betweens like Blagden, who made it his particular business to get answers to questions of this kind and was probably as good as anyone at playing this game. But every man of science wanting to compete on the international stage—and most especially in the fast-moving fields of galvanism and chemistry—would now have to work hard to keep clued-in to the latest developments, and had to become a good interpreter of sometimes rather patchy information. This necessitated having a good model in your head of how the system of scientific news operated; as with other sorts of "intelligence," a sizeable part of what you sought to know were the details of what your opposite numbers knew, and what *they* thought of what you were up to. And a corollary of this was that letting slip too much too early

might even be detrimental: Blagden once thought he overheard Joseph Banks quietly telling Davy not to give away too much information on his latest investigations in conversations with Blagden, as the great intelligence broker would be sure to put whatever Davy said in his next letters for France.[83]

Letters into Print: The Atlantic Career of Humphry Davy's Potassium and Sodium

In the evening of 12 November 1807, a year into the Continental blockade, Humphry Davy returned to Somerset House to deliver another paper at the Royal Society. It constituted the first public announcement of what British commentators soon claimed as the greatest scientific event the new century had yet produced: the discovery of two extraordinary metallic elements, eventually christened potassium and sodium.[84] So light they floated on water, so reactive that even ice made them burst into flame, these new substances—"analogous to some of those imagined to exist by the Alchemical visionaries," Davy fancied—had been hidden in plain sight, chemically locked away in the "fixed alkalis" potash and soda.[85] Now they were wrested from their long concealment by the application of the chemically-decomposing power of Volta's galvanic battery, and their discovery hammered home Davy's assertion, made at the Royal Society a year before, of a deep connection between electricity and chemical affinity.[86] News of the wonderful metals quickly spread in London and, over the succeeding months, through the rest of the scientific world. "I need not say how prodigious these discoveries are," enthused the

[83] *Blagden Diary*, 8 May 1808.
[84] Some at least of the men of science present were not entirely unprepared for the revelations that evening; true to form, Blagden had a preview of the information from Davy at Sir Joseph Banks's library a week before. *Blagden Diary*, 12 November 1807.
[85] Davy Laboratory Notebook, RI HD/6 (p. 85).
[86] On Davy's electrochemistry and the importance of the discovery of the alkali metals for Lavoisier's system of chemistry see: Robert Siegfried, "The Discovery of Potassium and Sodium, and the Problem of the Chemical Elements," *Isis* 54 (1963): 247-58; Homer LeGrand, "Determination of the Composition of the Fixed Alkalis 1789-1810," *Isis* 65 (1974): 59-65; Colin Russell, "The Electrochemical Theory of Sir Humphry Davy" (Parts I and II), *Annals of Science*, 15 (1959): 1-25.

English chemist Smithson Tennant, "it is something to have lived to know them."[87] The events launched Davy's Romantic public image as an inspired chemist of "genius" (it was already his personal myth), who had, as one acolyte put it, "raised a corner of the thick veil, and untied one of the last knots of the great tissue of wonders."[88]

Superlative language flew to and fro. For Samuel Taylor Coleridge, England's resident philosopher-poet (and an old friend of Davy's from the 1790s), these were "discoveries more intellectual, more ennobling and empowering human nature, than Newton's."[89] Not everyone went quite so far—Henry Brougham, writing in the *Edinburgh Review*, merely thought the discovery the greatest thing the pages of the *Philosophical Transactions* had seen *since* Newton—but this striking evidence of what promised to be a powerful and still largely unexplored connection between galvanism and chemistry, together with the conspicuous novelty of these "metal[s] of almost ethereal Levity" (Coleridge's phrase) was widely productive of general feelings of admiration and wonder.[90] "It is one of the greatest facts discovered since the beginning of science," enthused Richard Chenevix after seeing the new metals produced in Paris, "it furnishes hope that we may push the limits of our understanding far indeed."[91] In Britain, the discovery soon became the greatest object of national scientific pride since the beginning of the wars with France, much-trumpeted—as was the discoverer himself—in the general periodical and newspaper press. By 1811 the reliably jingoistic *Anti-Jacobin Review* could observe that "all

[87] From a letter written in January 1809, reproduced in "Some Account of the late Smithson Tennant, Esq.," *Annals of Philosophy* 6 (August 1815): 98.
[88] [Louis Simond], *Journal of a Tour and Residence in Great Britain, During the Years 1810 and 1811* (New York: Mercein, 1815), 2:151-52. On Davy, "genius," and self-fashioning see: Golinski, *Science as public culture*, 188-235; Jan Golinski, "Humphry Davy: The Experimental Self," *Eighteenth-Century Studies* 45 (2011): 15-28; Christopher Lawrence, "The power and the glory: Humphry Davy and Romanticism," in *Romanticism and the Sciences*, ed. Andrew Cunningham and Nicholas Jardine (Cambridge: Cambridge University Press, 1990), 213-27; David Knight, *Humphry Davy: Science and Power* (Oxford: Oxford University Press, 1992).
[89] Coleridge to Robert Southey, 14 December 1807, in *Collected Letters of Samuel Taylor Coleridge*, vol. 3, ed. Earl Leslie Griggs (Oxford: Clarendon, 1959), 41.
[90] *Edinburgh Review* 12 (July 1808): 394-95.
[91] Chenevix to Pictet, 19 January 1808, in *Pictet Corr*., 3:142.

persons are now familiar with the nature and properties of potassium, and with the structure of galvanic batteries," taking pride (and perhaps a pun) in how the "national vanity" of the French had "received such a shock" from Davy's electrochemical success. Davy's researches had:

> "excited the curiosity and the admiration of Europe; and, notwithstanding the present unnatural and antisocial state of the world, by the Buonapartean war, the name and discoveries of Davy have resounded, not only over France, but through every civilized country."[92]

The *Anti-Jacobin's* grandiose rhetoric obscured the real human labor that went into propagating Davy's discoveries beyond Britain. After Davy had officially announced his discovery in the first part of his Bakerian lecture on 12 November 1807, the city's two main scientific correspondents with links to France hastened to be the first to break the news abroad.[93] Blagden composed his first account in a letter to his friend Madame Gautier on 16 November, writing how Davy had "just analysed the fixed alkalis, and obtained their bases, which possess very singular properties."[94] He wrote a second letter to Gautier with further details on 5 December; both letters were sent via Blagden's special link to her family through the City financier Charles de Constant.[95] Blagden then followed this up with letters sent to Chenevix and Berthollet through the American ambassador on 29 December. He apologized to Berthollet for not addressing him first because "it was easier for me to have my letters sent to her [Gautier] than to you."[96]

In the event, none of Blagden's letters brought the first news to Paris; the honor fell to his friend and competitor in Continental correspondence, the London-based Swiss physician and

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[92] *Anti-Jacobin Review* 39 (1811): 337-40.
[93] RS JBO/39, 12 November and 19 November 1807, pp. 396-417. (A misprint in the *Philosophical Transactions* gives 19 November as the date of the lecture; in fact the lecture was begun on 12 November and concluded on 19 November.)
[94] Blagden to Gautier, 16 November 1807, RS CB/1/4/51. Although Davy had shown him samples on 12 November, Blagden only managed to see the experimental production of the metals for himself several days after writing to Gautier: *Blagden Diary*, 12, 21 November 1807.
[95] Blagden to Gautier, 5 December 1807, RS CB/1/4/52; *Blagden Diary*, 16 November 1807.
[96] Blagden to Berthollet, 29 December 1807, RS CB/1/1/223.

chemist Alexander Marcet. Told "in confidence" of Davy's discovery at the end of October, Marcet wrote his own letter to Paris the day after Davy made the news public, addressed to his Swiss friend Marc-Auguste Pictet, the editor of the *Bibliothèque Britannique* scientific journal.[97] However, Pictet was no longer at Paris but at home in Geneva, and so unexpectedly it was a slightly later letter from Marcet, written on 23 November, that actually broke the news in France.[98] This letter arrived in Paris sometime in mid-December, where its report of Davy's experiments, so Madame Gautier informed Blagden, "caused a real stir, and produced great admiration."[99]

But Davy's discovery initially proved tricky to repeat in France using only this single letter, inducing a period of uncertainty that foreshadowed later struggles to replicate experiments that would plague chemists on both sides of the Channel during the next few years. After several failed attempts at the École de Médecine and in the chemist Fourcroy's laboratory, Blagden's second letter to Madame Gautier eventually arrived with further details of how to apply the galvanic battery to potash, enabling the two young French chemists Gay-Lussac and Thenard to dispel all doubts by repeating the decomposition at the École Polytechnique on 6 January 1808.[100]

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[97] On this publication see Bickerton, *Marc-Auguste and Charles Pictet*.

[98] Marcet appears to have written two letters to France on 23 November, one to his brother-in-law Pierre Prévost (a member of Pictet's circle in Geneva), and another to an unidentified correspondent in Paris among or connected to the French chemists. This letter to Paris quickly came into the hands of Gay-Lussac and an extract was printed in the *Annales de Chimie* at the end of December: *AC* 64 (1807): 319-20. It is identified having been written by Marcet in *Blagden Diary*, 31 January 1808.

[99] Gautier to Blagden, 7 April 1808, RS CB/1/4/29.

[100] A letter from Chenevix to Pictet of 7 January 1808 reports that "Your news of Davy and the fixed alkalis has already been known here for several days [Pictet had written to Chenevix from Geneva on December 27 informing him of the news]. It was spoken of, it was doubted, and I myself was first among the doubters. Another letter from Blagden arriving with the details, the experiment was repeated yesterday at the Ecole polytechnique with *full and complete success*." *Pictet Corr.*, 2:141. Although an account published 5 months later in *Correspondance sur l'École Imperiale Polytechnique* 1 (1808): 445-48 states that Davy's experiment was repeated in France on 8 December, this is evidently false, as it is contradicted by Chenevix's letter, by the announcement in the *Moniteur* of 14 January that the French chemists had "just" ("viennent") repeated Davy's experiment, and by the fact that

Information on the discovery quickly passed into print in France. Both the *Annales de Chimie* at Paris and the *Bibliothèque Britannique* at Geneva printed excerpts from Marcet's letters at the end of December.[101] But characteristically, Blagden and the others in London learned of the successful repetition of Davy's work not from these journals, or from letters, but from the official Napoleonic newspaper, the *Moniteur*.[102] The *Moniteur* provided a separate and often quite rapid source of French scientific news in Britain during the war years and especially during the blockade, one which could sometimes considerably outpace even the speed of Blagden's letter-writing operation. As detailed in Chapter 2, the *Moniteur* frequently contained scientific articles, often adapted from memoirs or reports read at the Institut National by France's leading men of science and frequently containing precise technical details. It was from a long report of a session of the Institut printed in the *Moniteur* of 6 January 1808 that Britain got the news of Davy's award of Napoleon's Prix du Galvanism (the issue took only two weeks to reach Britain).[103] Eight days after that issue's date, on 14 January, a report was printed in the *Moniteur* containing the news of Davy's discovery of potassium and sodium together with a paragraph on its successful repetition by Gay-Lussac and Thenard. That issue reached the London men of science in only two weeks, arriving shortly before the end of January, whereupon Blagden was surprised and probably rather annoyed to find it was not his letter but Marcet's which had broken the news in Paris.[104]

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neither the news of the discovery published in the *Annales de Chimie* at the end of December nor Gay-Lussac's account of it at the meeting of the Institut National on 4 January mentioned that it had been repeated in France.

[101] *AC* 64 (1807): 319-20; *BB* 36 (1807): 391-93.

[102] On the *Moniteur* see André Cabanis, *La Presse Sous le Consulat et L'Empire, 1799-1814* (Paris: Société des Etudes Robespierristes, 1975).

[103] *Moniteur*, 6 January 1808, p. 20; reprinted in *Times*, 20 January 1808.

[104] *Moniteur*, 14 January 1808, pp. 54-56. "Suppose [they] got acct of last expts from Marcet's letter," Blagden noted after seeing a copy of the *Moniteur* at Joseph Banks's Library: *Blagden Diary*, 31 January 1808. Blagden added another arc to the exchange when he told Chenevix in a letter two days later that he had just seen how Davy's work "appears by the *Moniteur* to have been successfully repeated at Paris." Blagden to Chenevix, 3 February 1808, RS CB/1/3/43.

The speed with which the *Moniteur* brought scientific news like this to Britain had very little to do with men of science themselves; again, scientific material was hitching a ride, travelling to its destination because other information was already going its way. All through the Napoleonic period the *Moniteur* was a very highly-sought-after publication in Britain. It represented the public voice of the Napoleonic regime (it was even rumored—correctly—that Napoleon sometimes anonymously wrote for it), it contained important military bulletins from French armies in the field, and it offered a synthesis of news from other sources all across Continental Europe. New issues were highly valued by the British government, which wanted the intelligence the paper might provide, and by the London daily papers competing to be the first to print important foreign news. The government naturally maintained its own sources of supply, apparently by means of the occasional cartels and flag-of-truce ships and otherwise by buying from smugglers. London newspapers could then pay a fee (through the Post Office) to receive extracts from the government's foreign papers.[105] Some papers operated their own news-gathering systems which rivalled and sometimes bettered that of the state, with the *Times* said to operate a cutter in the English Channel to buy newspapers from French fishing boats.[106] Some intrepid individuals also apparently smuggled across papers and then sold them on in London, and late in the wars a single copy of a French newspaper might be bought by London papers for exorbitant sums—occasionally over a hundred pounds if it contained important military dispatches.[107]

[105] This system probably accounts for much of the French scientific news printed in the *Philosophical Magazine*, whose editor, Alexander Tilloch, was conveniently also in charge of a daily newspaper, the *Star*.
[106] *The History of the Times: "The Thunderer" in the making, 1785-1841* (London: Office of The Times, 1935), 106-07; Edward Porritt, "The Government and the Newspaper Press in England," *Political Science Quarterly* 12 (1897): 676; Cyrus Redding, *Fifty Years' Recollections Literary and Personal* (London: Skeet, 1858), 2:107.
[107] According to the account given by the editor of the *Sun* evening newspaper, William Jerdan, up to 100 guineas would sometimes be paid for important new issues of French newspapers.

The intense demand for the *Moniteur*'s military dispatches, its listings of Napoleon's latest decrees, and its general news from the French Empire brought the scientific information which also happened to be contained in the newspaper into Britain.[108] Blagden, Davy, and other London men of science naturally sought out copies of the *Moniteur* for the scientific news they might contain, integrating information obtained from the paper with that provided by correspondents in letters.[109] On occasion, material from the *Moniteur* would prove invaluable, sometimes remaining the only description of important French experiments received in Britain for several months after it arrived. Davy's brother later called the paper "the Journal of science as well as of War."[110] Possibly never before or since has a newspaper assumed so much *purely scientific* importance as the *Moniteur* did in London under the Continental System.

While the London scientific community was scanning the *Moniteur* to gauge the response to Davy's discovery of potassium and sodium in France, others were hard at work transmitting the intelligence further afield. Enter Frederick Hall, a young American professor of natural philosophy in the midst of a scientific tour of Europe.[111] After spending the earlier part of 1807 sampling the London scientific world (where he naturally met Blagden), Hall journeyed on to Paris, going via Rotterdam on a Dutch ship not long before Napoleon's Milan decree closed all

[108] This fortunate outcome was probably not just good luck. French men of science would have been well aware that the *Moniteur* was often the fastest conduit of news to Britain, and this perhaps even accounts for the unusual technical detail of some of their articles in the newspaper: reports were maybe not so much addressed to French lay readers at home as to men of science abroad.
[109] During 1807 and 1808, issues of the *Moniteur* appear to have usually taken around two weeks to travel to London, although not every issue would have reached Britain. Beyond what was inserted by Tilloch in the *Philosophical Magazine*, original copies of the *Moniteur* were often available to Blagden and others in Joseph Banks's library, apparently passed on by scientifically-inclined civil servants like Alexander McLeay. It seems important articles were occasionally given directly to men of science by sympathetic officials: once Blagden read an article in an issue of the *Moniteur* that had just arrived at Joseph Banks's library and hurried to the Royal Institution to show it to Davy, only to find that Davy had already received a copy from the private secretary of Spencer Perceval, then the Chancellor of the Exchequer. *Blagden Diary*, 3 May 1808.
[110] *Davy Works*, 5:3.
[111] Hall was Professor of Natural Philosophy at Middlebury College, Vermont.

Continental ports to neutral vessels coming from Britain.[112] The news of Davy's discovery arrived in Paris when Hall was there, and not long afterwards he received his own version in a letter from Blagden, delivered through the American diplomats.[113]

Doing the rounds of dining with Gay-Lussac and other French scientific figures during his winter in France, Hall was well-placed to catch the crest of a fresh wave of scientific news which broke there in early March. Applying Berthollet's ideas on the variability of chemical affinities, Gay-Lussac and Thenard had effected an unexpected elaboration of Davy's discovery by producing potassium solely by chemical means. They had done this by passing potash across white-hot iron filings inside a bent gun barrel; as liquefied potash flowed through the tube, it was reduced by the iron, forming potassium.[114] This surprising result—galvanism was not in fact the *only* way of unlocking potassium from its compounds—provided a welcome boost to French chemistry's self-image after the startling discovery of "the English Lynx," as Davy was subsequently dubbed in the pages of the *Bibliothèque Britannique*.[115]

It also provided Hall with an additional juicy piece of scientific intelligence to send home to the United States. On 24 March 1808 he wrote two letters to America, one addressed to the Philadelphia physician and botanist Benjamin Smith Barton, and the other to Yale man of science Benjamin Silliman.[116] Composing them, he mixed direct quotations from the letter Blagden had sent him on Davy's discovery with his own description of Gay-Lussac and

[112] Hall's journey is detailed in his letters published in the three volumes of his periodical, the *Literary and Philosophical Repertory* (Middlebury, Vermont, 1812-1817). Hall nonetheless had to seek the assistance of James Monroe to obtain passage to the Continent; he originally intended to return to Britain before the winter but in October apparently decided that the further tightening of the Continental System made this too difficult: see Berthollet to Blagden, RS CB/1/1/220.
[113] The original letter, dated 24 November 1807, apparently does not survive, and (unusually) Blagden did not keep a draft copy; Blagden gave it to the diplomat Robert Walsh to transport. *Blagden Diary*, 24 November 1807.
[114] The experiment was announced at the Institut National on 7 March 1808 (*PVSA*, 4:25). The gun barrel and iron filings method was an adaption of Lavoisier's well-known experimental procedure for the decomposition of water with hot iron.
[115] "Preface," *BB* 40 (1809): xi.
[116] Hall probably sent them home in an official American government vessel, as France was by then blockaded by America under Jefferson's Embargo Act.

Thenard's work.[117] The letters arrived at the post office in New York on 5 July, after about two months crossing the Atlantic, and within a few more days were in the hands of their recipients.[118] What happened next provides a fascinating instance of how information could move fluidly between different kinds of media in the hybridized world of scientific communication around 1800. The audience for Blagden and Hall's words was about to expand massively, through the agency of print.

Although the news of Davy's decomposition of the alkalis seems to have circulated in America somewhat earlier, the two letters from Hall were certainly the first intelligence of the French chemists' new gun-barrel experiment.[119] Both recipients, aware of the value of the news their letters contained, therefore decided to put them into print. For Barton, the editor of the recently-founded *Philadelphia Medical and Physical Journal*, the obvious choice was his own publication, and an extract from his letter was reprinted in the issue that was fortunately just about to go to press, appearing in Philadelphia on or about 14 July.[120]

For his letter, received at almost the same time, Silliman took a different course. He sent an extract to a newspaper, the *Connecticut Herald* of New Haven, which printed it in its issue of

[117] Both letters survive: Frederick Hall to Benjamin Smith Barton, 24 March 1808, Benjamin Smith Barton correspondence, American Philosophical Society, Philadelphia, Mss.B.B284d; Frederick Hall to Benjamin Silliman, 24 March 1808, Historical Society of Pennsylvania, Philadelphia, Benjamin Silliman Correspondence, Box 1. On their recipients see Whitfield Bell, "Benjamin Smith Barton, M.D. (Kiel)," *Journal of the History of Medicine and Allied Sciences* 26 (1971): 197-203; Chandos Michael Brown, *Benjamin Silliman: A Life in the Young Republic* (Princeton: Princeton University Press, 1989). The portions of the two letters containing the news are remarkably similar; many paragraphs are almost identical. But the letter to Silliman included an additional item of Napoleonic gossip, relating how the French Emperor had taken such an interest in Davy's discovery, which had been "the principal topic of conversation among the *savans* of Paris," that he had "ordered M. Gay-Lussac, one of the most persevering and distinguished experimental chymists of France, to go over the process of Mr. Davy, in *his* presence."

[118] Both letters bear postmarks from when they were put into the mail at New York. Because of prevailing winds, the East to West Atlantic crossing was always slower; news could travel faster from West to East.

[119] Mention is made of the news of Davy's discovery having previously been in the American papers in *Connecticut Herald*, 12 July 1808; a possible source for that news is a letter sent to James Monroe by Blagden: Blagden to James Monroe, 26 November 1807, Library of Congress James Monroe Papers (Microfilm).

[120] *Philadelphia Medical and Physical Journal* 3 (1808): vii-viii. Date of publication computed from *Poulson's American Daily Advertiser* for 14 July, 22 July, 30 July, 1808. In neither of his letters did Hall give any indication that he expected or desired the text to be printed, but it seems likely that he would have assumed that the letter to Barton, at least, would find its way into the *Medical and Physical Journal*.

12 July.[121] Now the letters had entered the periodical commons, and so Hall's letter to Silliman in the *Connecticut Herald* (containing, of course, parts of Blagden's letter to Hall) was soon taken up by numerous other newspapers and reprinted down the East coast from New York to South Carolina.[122] It turned up in Philadelphia, for example, on 18 July in that city's *Poulson's American Daily Advertiser*, narrowly missing scooping Barton's publication of the news in his journal (and certainly reaching more readers than he did).[123] We can only guess at reception, but from the extent of reprinting it certainly appears that many editors independently judged the discoveries a piece of news well-calculated to excite readers' curiosity.

The activity of newspaper and periodical editing, printing, and reprinting multiplied the text of Hall's two letters in America, even leading to their reproduction in some other, less ephemeral media. Words originally written in London by Blagden as part of his letter to Hall appear—copied from printed versions of Hall's letters—in a reference work on Pharmacy printed in Boston, and also in a footnote hurriedly added to update a new American edition of a popular chemistry book directed "particularly to the female sex," the *Conversations on Chemistry*.[124] In an amusing twist, the anonymous author of the *Conversations*—then entirely unaware of the insertion made by the American editor—was Jane Marcet, wife of the Alexander Marcet whose letter had brought the news of Davy's work to Paris.

It remains only to complete the circle. By 23 July, Benjamin Silliman's letter from the *Connecticut Herald* had been reprinted by newspapers in New York City.[125] There, unknown hands put one of these newspapers on a ship bound for England, where the paper found its way

[121] *Connecticut Herald*, 12 July 1808.
[122] For example, in Baltimore on 20 July (*North American*), Washington D.C. on 30 July (*Washington Federalist*), Richmond, Virginia on 26 July (*Enquirer*), Charleston, South Carolina on 2 August (*City Gazette*).
[123] *Poulson's American Daily Advertiser*, 18 July 1808.
[124] James Thacher, *The American New Dispensatory* (Boston: Wait and co., 1810), 33; [Jane Marcet], *Conversations on Chemistry*, 2nd American ed. (Philadelphia: Humphreys, 1809), 3:185. On this book see M. Susan Lindee, "The American Career of Jane Marcet's Conversations on Chemistry, 1806-1853," *Isis* 82 (1991): 8-23.
[125] *New-York Commercial Advertiser*, 23 July, 1808, itself reprinted by *New-York Spectator*, 27 July 1808.

to Robert Aspland, the London-based editor of a general-readership periodical, the *Monthly Repository of Theology and General Literature*. Aspland had enthusiastically reported on Davy's discovery of potassium before and was ready to make use of more news on the subject, and so the much-copied letter was duly printed again, appearing in Aspland's October issue.[126] The text was by now so multi-layered that it included a quote from Blagden's letter written to Hall, copied into a letter to Silliman, printed in the *Connecticut Herald*, and reprinted by the New York newspaper that carried the quote back to England to be reprinted again in the *Monthly Repository*. Sadly, we do not know if Blagden ever read it, or what he made of it if he did.[127]

Synchronizing Experiments, Disputing Interpretation: Letters, Newspapers and Journals at Work in Electrochemistry

> "…his labours at this interesting moment [the decomposition of the alkalis] were interrupted by a dangerous illness of several weeks' duration. On his recovery, he found the subjects he had been investigating, seized on by MM. Gay Lussac, and Thenard, rather in the manner and feeling of contending generals intent on conquest, than of philosophical inquirers, members of the common republic of science. This proceeding, it cannot be concealed, annoyed him at the time; especially as there was often a want of reference on their part to his previous labours: and it necessarily had the effect of hurrying on his researches…"
>
> —— John Davy, in *The Collected Works of Sir Humphry Davy*, 1840.[128]

We have just seen that a common thing to do with fresh and exciting scientific news arriving in a letter was to pass it to others who had not yet received it. The information might be spread to a new site by copying it into fresh letters (rather as we might now forward an email), as Hall did when he copied passages from Blagden's letter for his contacts in the United States. Or the

[126] *Monthly Repository of Theology and General Literature* 3 (1808): 574-75. On Aspland and this periodical see Francis Mineka, *The Dissidence of Dissent: The Monthly Repository, 1806-1838* (Chapel Hill: University of North Carolina Press, 1944).
[127] Blagden's diary is silent on the matter. Possibly Blagden never learned that direct quotes from his letter to Hall (with his name attached) were circulating widely in the United States in newspapers and other publications, but if he did find out, he would almost certainly have been none too pleased. Blagden was always sensitive about the privacy of the intelligence in his letters, complaining to his diary on one occasion how he was "hurt" at seeing part of a letter written to Berthollet turn up in print in the *Annales de Chimie*. *Blagden Diary* 2 September 1805; letter printed in *AC* 55 (1805): 84-86.
[128] *Davy Works*, 5:101.

audience could be broadened locally, by word-of-mouth transmission, as Blagden did when he read out letters at Joseph Banks's Library. Or the letter could be publically broadcast in print, as Silliman and Smith Barton both did using the *Connecticut Herald* and the *Medical and Physical Journal*. Once printed, the text from letters became fair game for reprinting by other publications like any other piece of information in the periodical commons, potentially spreading the word to yet more places and more audiences.

The main response in America to the news of Davy's discovery carried in Hall's letters had been to print it and keep printing it. In a nation with a disproportionately high number of newspapers and newspaper readers but possessing no tight-knit community of research-oriented chemists in the manner of Britain and France, that was natural.[129] In London and Paris, however, fresh information about new developments pertaining to Davy's decomposition of the alkalis had a more practical use and a more urgent and competitive dimension: both sides were acutely aware that new intelligence might hold the key to another major discovery. As with the Voltaic Pile seven years before, a new, startling, and spectacular experiment had opened a hidden door in the edifice of scientific knowledge. There was a general feeling that great prizes lay just beyond, ready for the taking, but the appearance of this new passage forward was disconcerting as well—old assumptions about the structure of chemistry might have to be reimagined. John Davy remembered how "after the decomposition of the fixed alkalis, doubt naturally spread through every department of chemistry; a salutary doubt, prompting further inquiry and scrutiny, by means of the new powers of analysis."[130] Some of the scientific news that subsequently crisscrossed back and forth formed the seeds of lasting knowledge; other exciting claims or ideas

[129] Hall did write to Silliman that he thought that Silliman would "be interested in repeating these experiments of the English & French philosophers, and I hope your labors will throw additional light on this new subject of physical research." (Hall to Silliman, 24 March 1808). But Silliman does not appear to have reproduced the experiments in America.
[130] John Davy, *Memoirs of the Life of Sir Humphry Davy*, 1:407.

failed to germinate, and were quickly forgotten. Davy is still remembered for his isolations of magnesium, calcium, strontium, and barium during this time; less so for his "discovery" (so he believed!) of the compound nature of sulphur, phosphorus, and carbon, though this was just as eagerly reported as news by Blagden in his letters and was printed as fact in English newspapers.[131] In these fluid circumstances the chemists of London and Paris worked hard to use the scarce scientific intelligence they had from one another both to open up their own fresh lines of investigation, and to repeat and expand on—and sometimes challenge—experiments made by their foreign counterparts.

To see more concretely how the movement of scientific intelligence in both letters and print back and forth across the Channel was bound up with day-to-day struggles with tricky experiments and debates over their interpretation, let us return to the moment in early March 1808 when Gay-Lussac and Thenard invented their method of making potassium by chemical means using potash heated with iron turnings in a gun barrel. Motivating their invention of the new method was an intuition (correct, as it turned out) that potassium might open the way to further important discoveries if its extraordinary reactivity could be harnessed to chemically analyze other substances.[132] As Hall had reported to Silliman, it was "generally believed" at Paris that the discovery would "lead to others of equal, and, perhaps, superior importance."[133] Davy's method of making potassium and sodium using the decomposing power of the voltaic battery had only yielded small quantities of the new metals, and the point of the gun barrel experiment was to allow potassium to be manufactured in quantities sufficient for its use as a reagent in further

[131] For example, Blagden to Berthollet, 25 May 1808, RS CB/1/1/222. Blagden to Gautier, 29 April 1808, RS CB/1/4/50. *Observer*, 2 October 1808, p. 2. Davy claimed to have decomposed all three and to have shown that they contained hydrogen.
[132] The intuition proved true with their subsequent isolation of boron later in 1808, which became the subject of a priority dispute between the French chemists and Davy.
[133] Hall to Silliman, 24 March 1808.

investigations. This would hopefully yield glorious prizes for French chemistry and recapture intellectual territory ceded to the victorious Englishman. Berthollet did not give any hint in his letters to Blagden of a sense of embarrassment in French chemical circles at having been beaten to the decomposition of the alkalis, but Richard Chenevix, as Blagden's English informant in Paris, was more than willing to pass on a report of an awkward moment: Napoleon, having seen Davy's experiments repeated by Gay-Lussac and Thenard, had "laughed a little at the French Chemists for letting such a number of discoveries slip through their hands."[134] Clearly the preeminence of French chemistry—secure since Lavoisier—was in need of defense, and the best way to do this was by quickly capitalizing on Davy's work to open new ground.

Gay-Lussac and Thenard announced their gun barrel experiment at a session of the Institut on 7 March 1808. This announcement became a short article in the *Moniteur* the next day, which only gave the barest sketch of the process but hinted at its potential by noting how the large quantities of potassium and sodium now available made it possible "to study all their relations with other bodies."[135] In a simpler world, the London chemists would have had this news in less than two weeks. The reality was more chaotic: for some reason this particular issue of the *Moniteur* failed to reach England.[136] London therefore remained completely ignorant of what the French had been doing after they had repeated Davy's experiments until the first days of May (some two months after the gun barrel experiment was announced in Paris), when an American ship called the *Osage* sailing from France under diplomatic orders finally brought a

[134] Chenevix to Blagden, 2 February 1808, RS CB/1/3/50.
[135] *Moniteur*, 8 March 1808, p. 268. Perhaps deliberately, the *Moniteur*'s paragraph is vague on the specifics of what the French chemists actually did—saying that they had treated the alkalis with "iron at a high temperature" but failing to mention the use of the bent gun barrel as a vessel.
[136] Or, in the unlikely event that it did, it was somehow never seen by anyone with scientific interests, including the newspaper editor Alexander Tilloch.

cache of letters.[137] Unfortunately for Blagden, his correspondents mentioned the news only in passing, assuming he already had it.[138] Happily for everyone else, the Americans also carried a letter for Davy from a Scottish prisoner of war with scientific interests who was detained at Paris, William Archibald Cadell, as well as letters for Marcet from his Genevan friends Marc-Auguste Pictet and Charles Gaspard de la Rive.[139] All contained descriptions of the experiment; Marcet's letter even enclosed the two printed pages from the *Bibliothèque Britannique* which had communicated the same news to that journal's readership.

But in this instance textual news of a discovery in letters was not sufficient for the experiment to be repeated successfully at the new site. Confronted with a case like this, historians of science might raise issues of tacit knowledge or even the notorious "experimenters' regress" (where the question of whether an experimental setup has been properly replicated is hopelessly entangled with the question of whether such experiments can actually be said to produce the results claimed by their originators).[140] But early-nineteenth-century British men of science preferred to see things in terms of failures of communication. Having soon heard or seen all the letters that brought the news to London, Blagden was dissatisfied by their brevity and lack

[137] The letters were carried by the American diplomatic messenger, Captain Lewis. *Times* 3 May 1808; *Blagden Diary*, 3-5 May 1808.
[138] Delessert to Blagden, 8 April 1808, RS CB/1/3/225; Gautier to Blagden, 7 April 1808, RS CB/1/4/29. Berthollet had also written to Blagden on 20 March with the news, but his letter did not arrive until later in May; he too assumed that Blagden already had information on the French process—presumably from the *Moniteur*. Berthollet to Blagden, 20 March 1808, RS CB/1/1/224; Blagden to Delessert, 6 May 1808, RS CB/1/3/258.
[139] Blagden heard the letter to Cadell read aloud at Joseph Banks's library (*Blagden Diary*, 4 May 1808), after which it was also read out at the next meeting of the Royal Society and subsequently summarized in the *Philosophical Magazine*, 30 (May 1808): 366. The letter to Marcet is mentioned in *Blagden Diary*, 5 May 1808. Cadell (see *DNB*) subsequently escaped from France the next year, supposedly by passing as a Frenchman.
[140] Harry Collins, *Changing Order: Replication and Induction in Scientific Practice* (London: Sage, 1985), esp. ch. 4, and Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1986), esp. ch. 6; for "tacit knowledge" and the difficulties of communicating it, see Michael Polyani, *The Tacit Dimension* (Chicago: University of Chicago Press, 1966). But here, as explained below, the experiment was in fact eventually replicated to British chemists' satisfaction using only textual information—though with a good deal of work, multiple failures, and a lingering dissatisfaction on the part of the British with the purity of the potassium they were getting from the French experiment. This success of course drew on a deep base of tacit skills shared by chemists on both sides of the channel.

of "intelligibility" as to the minutiae of the experiment.[141] His fears were borne out when Davy, Marcet, Smithson Tennant, and several other chemists all tried and failed to replicate the French experiment in the next few weeks. "We have not been able to succeed in decomposing the alkalies by chemical means without Electricity," Davy told an aristocratic Fellow of the Royal Society early in June, suggesting that "probably there has been some mistake in the communication that has been made of their method."[142]

The repetition of the gun barrel experiment taxed the ingenuity of London's most skillful chemists all through the summer of 1808. It was tricky and potentially dangerous to perform even when one already knew the exact procedure, requiring a white hot furnace, a particular kind of double bend in the gun barrel (one part of which was placed in the furnace while another part was cooled with ice), and a very dry sample of pure potash; sometimes the potassium product would spontaneously burst into flame. But Davy and others were struggling with a much messier dual problem, which combined the replication of difficult experimental technique with the puzzle of trying to make sense of the patchy details of the procedure they had received from France. Because of the horribly stretched timescale of round-trip scientific conversations in letters between London and Paris (as we saw, several months rather than under two weeks), seeking the obvious solution of asking France for more information proved problematic. After the initial attempts had failed, Blagden sought to use Chenevix, his trusted English informant, to obtain further details. "We have tried to repeat the experiment here… but without success," Blagden wrote on 19 May, asking Chenevix to "send us the details of the procedure, if you can."[143] Six weeks later, when more trials yielded only what seemed to be an alloy of potassium with iron, he wrote to ask again, blaming the "imperfect directions" which London had received for the

[141] *Blagden Diary*, 5 May 1808.
[142] Davy to Lord Webb Seymour, 3 June 1808, RI HD/26/J/3 (Copy of original in Aylesbury Record Office).
[143] Blagden to Chenevix, 19 May 1808, RS CB/1/3/48.

failure.[144] But—neither of these letters ever reached Chenevix.[145] Blagden was only able to get his request through when an American ship—in fact the *St. Michel* mentioned earlier—left Britain to go to France in early August.[146] And Chenevix only managed to send a reply on 7 September, which did not reach Blagden until mid-November—by which time the information it contained was no longer useful.[147] In this instance, Blagden's letter-writing intelligence system had proved insufficiently capable of even the most basic question-and-answer conversation.

During this time, however, further information had arrived from France which eventually allowed the British chemists to repeat the experiment. A more detailed description had come to London in the second half of July, apparently in a letter for Davy, but failed to completely clear up the difficulties.[148] A fuller account of the experiment also made it to Britain in print, as part of a long report on Gay-Lussac and Thenard's work with potassium published in the *Moniteur* on 27 May, and its fairly detailed technical description of the gun barrel experiment must have proved valuable to Davy.[149] Even more useful was an issue of the journal *Correspondance sur l'École Imperiale Polytechnique* which someone in France fortunately thought to send to Britain, and eventually by early October the London chemists were finally satisfied that the French

[144] Blagden to Chenevix, 7 July 1808, RS CB/1/3/44.
[145] Blagden attempted to send one of the letters, unusually, via Spain, and the other by some unknown route which he apparently did not consider very reliable: Blagden to Chenevix, 28 July 1808, RS CB/1/3/53. Chenevix wrote to Blagden on 30 June and 7 September (RS CB/1/3/54, RS CB/1/3/56) and did not mention these letters among the ones he stated he had received.
[146] Blagden wrote to both Chenevix and Berthollet by this route stating that their experiments were still only giving an alloy with iron and not pure potassium, and once again specifically asking Chenevix whether he could "send us any light on this matter." Blagden to Chenevix, 1 August 1808, RS CB/1/3/55. Blagden to Berthollet, 1 August 1808, RS CB/1/1/226.
[147] Chenevix to Blagden, 7 September 1808, RS CB/1/3/56. Chenevix recommended an additional distillation step.
[148] This missing French letter is mentioned in Davy to John George Children, [July 20] 1808, British Library Add. MS 38625, fols. 1-2. As Blagden's letters to France written ten days later show, it was the problem of the potassium being alloyed with iron that still plagued the London chemists.
[149] *Moniteur*, 27 May 1808, pp. 581-582.

experiment had been repeated correctly.[150] Only then did Davy publically release the information received from France, by passing what was probably the only copy in Britain of the relevant issue of the *Correspondance sur l'École Polytechnique* to Alexander Tilloch of the *Philosophical Magazine*, who reprinted from it at the end of October.[151] Incidentally, this was at exactly the same time that the other printed version of the news of the gun barrel experiment that had come via Frederick Hall and the New York newspapers appeared in Aspland's *Monthly Repository*.[152]

I have dwelt on the details of these struggles to replicate the French method of producing potassium to illustrate the messy and often chaotic processes that lay behind cross-Channel exchanges of experimental knowledge under the blockade. Letters went astray, issues of newspapers sometimes failed to make their usual rapid passage, correspondents assumed details had already been received or simply failed to elaborate in sufficient depth. Information in print overlapped with information in letters; any fragment or detail, brought by whatever strange paths from across the frontiers of war, might end up proving useful. Fast-paced and reliable cross-Channel scientific conversations, structured around question-and-answer-based dialogue in letters, were simply not possible in the way they were before and after the wars. For all that, an inferior slow-motion and intermittent version of this process was productively kept alive by Blagden and others. Although all participants regularly complained of how hard circumstances

[150] Although they still complained that there was a little iron combined with the potassium they obtained. Blagden apparently did not actually see a successful trial of the experiment until Davy showed it to him at the Royal Institution in November: *Blagden Diary*, 18 November 1808.

[151] Tilloch noted in this instance that "we need scarcely state to our readers that, in the present interrupted state of communication between this country and the Continent, we find it extremely difficult to procure the foreign journals," adding that "our friends who happen to obtain any of them cannot confer on us a greater favour than by allowing us the use of them for a few days." *PM* 32 (October 1808): 88-92.

[152] It may not now come as too much of a surprise to the reader that the *Philosophical Magazine*'s text was soon taken up and reprinted by other British periodicals, finding its way into the *Universal Magazine* and the *Monthly Magazine*, and even, via yet another reprinting of a single paragraph taken from the latter, into the London *Observer* newspaper in the first days of 1809. *Universal Magazine* 10 (Nov 1808): 442-44. *Monthly Magazine* 26 (Dec 1808): 469. *Observer*, 8 January 1809.

were, hard work monitoring and managing communications with letters and print did eventually allow experiments originating on one side of the Channel to be satisfactorily replicated on the other. Both sides were acutely aware that other people's experiments might be intermediate steps to further discoveries, and this consciousness, combined with the double struggle to obtain good intelligence and to actually physically replicate experiments, served to fuel a strong sense of hurried rivalry in both London and Paris.

Agreement on interpretation proved much more difficult. It formed the other half of the rivalry between Davy and the French chemists; disagreements over theory were perhaps just as responsible for fuelling the public sense of national scientific antagonism as was the competition for experimental discoveries. One particular point of interpretive disagreement arose in the immediate aftermath of Davy's decomposition of the alkalis, prompting further experimentation and a flow of arguments and counterarguments. This was the contention of the French chemists that potassium and sodium were not true metallic elements, but rather combinations of potash and soda with hydrogen. That claim denied Davy his central assertion (and achievement) of having decomposed the alkalis, and it formed the background to other theoretical debates between the London and Paris chemists for the next few years.

The news of the French chemists' opposition apparently came as something of a shock to Davy, who had been under the impression that they fully accepted his claim to have discovered two new metallic elements. Soon after he had read a new lecture to the Royal Society on 30 June 1808 on the isolation of the alkali earth metals, a copy of the May 27 issue of the *Moniteur* rather belatedly reached the London chemists.[153] It contained a long description of work done by Gay-Lussac and Thenard on samples of potassium procured with their gun barrel experiment—not yet

[153] Probably it made the trip on the *St. Michel* truce ship, as Blagden's diary indicates it was seen by the London chemists only around 20 July: *Blagden Diary*, 28 July 1808.

successfully replicated in Britain—investigations that claimed "to remove all the doubts, and prove that the metals which are obtained from the alkalis are actually combinations of these alkalis with hydrogen."[154] The decisive experiment exhibited by the French duo was a complex series of reactions of potassium with ammonia gas, the final products of which appeared to be potash, the original volume of ammonia, and hydrogen.[155] Subsequent events proved most of this *Moniteur* article to be something of a blind alley; from the point of view of contemporaries, it was among the most important pieces of French scientific news to hit Britain in 1808. Naturally the arrival of the newspaper caused something of a stir among the London chemists, and its contents irritated Davy, who apparently considered research on potassium to be his own personal demesne and who thought he had sufficiently proved over half a year before that potash decomposed to potassium and oxygen. "The French Chemists are reviving Phlogiston," he sarcastically quipped to Marcet.[156] From the viewpoint of Paris, however, Davy was the one who had made an embarrassing error. Chenevix blamed the excessive haste of his research. "I am

[154] *Moniteur* 27 May 1808, pp. 581-582.

[155] This complex experiment involved melting potassium in dry ammonia over mercury; the ammonia was absorbed, furnishing a new "grey-greenish" substance, while at the same time hydrogen was given off. The grey-greenish matter was then heated *under* mercury, driving off some of the absorbed ammonia. After that, the remaining substance was reacted with water, furnishing more ammonia. The total volume of ammonia absorbed at the beginning, so the French chemists claimed, was equal to the total driven off in the other steps; the hydrogen was, they claimed, from the potassium. A modern chemist trying to replicate their work would probably interpret this sequence in terms of the successive generation of potassium amide, potassium nitride (both unknown substances before this experiment), and potassium hydroxide (potash), with the hydrogen coming from the water. The experiment was apparently exceptionally tricky to perform. John Davy, writing in 1836, claimed somewhat hyperbolically that "it was an experiment of extreme difficulty, perhaps the most difficult ever recorded in the annals of chemical research." He noted especially the danger of contamination: "The metal had the most powerful attraction for oxygen; potash as powerful an attraction for moisture." John Davy, *Memoirs of the Life of Sir Humphry Davy*, 401.

[156] Davy to Marcet, 25 July 1808, American Philosophical Society, Philadelphia, MSS.B.D315.1. Davy was referring to the dogma of the phlogiston theory that metals were composed of a calx (oxide) plus phlogiston (here identified with hydrogen); the French claim was that potassium was composed of potash plus hydrogen. Nonetheless, Davy was himself willing to entertain a modified "phlogistic chemistry," in which potash would be the oxide of potassium but potassium *itself* (and all other metals) a compound of hydrogen with an unknown and more fundamental base.

afraid Davy goes on too quickly in metallising all these substances," he informed Blagden. "It would be well to caution him as every little error of his is a subject of triumph elsewhere."[157]

After receiving the *Moniteur*'s news, Davy immediately launched into a period of concentrated work, repeating the French chemists' fiendishly challenging manipulations with potassium and ammonia and marshalling experimental and theoretical arguments of his own. "The labour [he] expended on this experiment was extreme," recalled his brother, "he repeated it a vast number of times, in a variety of forms, and with every precaution that his ingenuity could invent."[158] For his part, Blagden, who had lost no time in sounding out Davy's view of the French experiments, mobilized his correspondents to try to reach a more informal *rapprochement* on the issue.[159] "I see that the Chemists on your side of the water take all these new substances to be hydruets [hydrides] rather than metals" he observed to Delessert, before harnessing his own correspondence archive to show that just after the discovery "the same opinion occurred to us" (but was rejected) and that he had even informed France of it through Madame Gautier back in November 1807.[160] To Chenevix he remarked that "Davy has seen the account of the new experiment with ammonia in the *Moniteur* at full length & is of opinion that it decides nothing," passing on several of Davy's objections.[161] He sent similar comments to Berthollet.[162] Berthollet, however, chose not to enter too much into the details, simply blaming the whole disagreement on the difficulty of keeping London and Paris in dialogue with one another during the blockade:

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[157] Chenevix to Blagden, 30 June 1808, RS CB/1/3/54.
[158] John Davy, *Memoirs of the Life of Sir Humphry Davy*, 401. Davy's laboratory notebook does indeed show his acute preoccupation from late 1808 to the summer of 1809 with ever-varying quantitative experiments on the action of potassium on ammonia, clearly in response to the French chemists' work. Davy Laboratory Notebook, RI HD/6, pp. 123-235.
[159] *Blagden Diary*, 28 July 1808; Blagden found that Davy thought the hydrogen in the French experiment was from the decomposition of ammonia.
[160] Blagden to Delessert, 1 August 1808, RS CB/1/3/224. Blagden to Gautier, 16 November 1807, RS CB/1/4/51.
[161] Blagden to Chenevix, 1 August 1808, RS CB/1/3/55.
[162] Blagden to Berthollet, 1 August 1808, RS CB/1/1/226.

> "I am very sensible of the care which you take to inform me of anything, as much as is possible for you, of the progress of the sciences. The slowness and the imperfection of communications doubtless retards this progress somewhat, because we would doubtless fall promptly into agreement, if we had a reciprocal knowledge of the facts on which we establish our respective opinions. Thus it is not surprising, that although we give full justice to the fine discoveries of Mr Davy, we remain in opposition to many of his opinions; besides, science cannot but benefit, because the difference of opinion and discussion serves to multiply the facts."[163]

A solution of the disagreement was not forthcoming through Blagden's news exchanges conducted in letters. The French chemists had moved the discussion onto the territory of lengthy technical argument, and it was played out in a series of publications over the next few years, exchanged with much difficulty and sometimes with Blagden's help.[164] Gay-Lussac and Thenard only capitulated in 1811.

Berthollet was surely right that the dispute was sustained in part through distance. Yet there is also a sense in which these exchanges actually bound the elite scientific communities of Paris and London together in what was increasingly becoming a two-horse race. In the first place, many more humble or less well-resourced chemists who might have participated in the debate in the monthly journals were undoubtedly shut out by the sheer difficulty (and expense) of some of the experiments. Gay-Lussac and Thenard's trials with ammonia were not only extraordinarily tricky to perform, but required a sample of potassium—the best source of which was the gun barrel experiment which had given Davy and his London friends so much trouble. For the London and Paris elite, this period might have reprised the feeling of the heady months

[163] Berthollet to Blagden, 19 April 1809, RS CB/1/1/229.
[164] Blagden arranged to send copies of several of Davy's papers to France (Blagden to Berthollet, 4 February 1809, RS CB/1/1/228; Blagden to Berthollet, 10 March 1810, RS CB/1/1/234); the second volume of the *Memoires* of the Society of Arcueil contained some of Gay-Lussac and Thenard's contributions to the debate, and this was carried to from France to Britain by Richard Chenevix (finally on his way back home), who passed them on to Blagden: Berthollet to Blagden, 9 July 1809, RS CB/1/1/231.

after the appearance of the Voltaic pile; for most others, the excitement was now restricted to that of the spectator, watching from the sidelines while Davy and the French traded blows and notched up further discoveries.

But alongside the difficulty of the actual experiments, the pattern of the distribution of intelligence was also crucial. Davy faced some of the same challenges to his claims about the alkali metals from chemists within Britain but who were not part of the London scientific conversation—in particular John Dalton in Manchester, Charles Sylvester in Sheffield and the Edinburgh Chemist John Murray all expressed belief in the possibility that the new metals were hydrides. But these critics from the provinces were cut out of the loop of debates between Britain and Paris: they did not have American diplomats carrying letters and journals back and forth for them, or access to any of the other channels used by Blagden and his friends, and their knowledge of French scientific news was second hand, and late. This was a situation which Davy was not above exploiting: when in 1811 Murray re-entered the lists in *Nicholson's Journal* with new experiments that he claimed showed potassium was a compound containing hydrogen, Davy put his younger brother John to work correcting the Edinburgh chemist, using information on Gay-Lussac and Thenard's experiments on peroxides of potassium straight from an issue of the *Moniteur*, a publication to which Murray and anyone else in Britain outside Blagden's metropolitan clique was very unlikely to have direct access.[165] Davy's arguments in later lectures to the Royal Society were explicitly oriented towards France, and he even took the unusual step of writing directly to French journals to controvert some of Gay-Lussac and Thenard's

[165] Murray, *NJ* 28, Supplement (April 1811): 359-69. John Davy, *NJ* 29 (May 1811): 35-39. A translation of the *Moniteur's* article (from *Moniteur* 5 July 1810) was then appended to John Davy's letter. John Davy asserted that the substance on which Murray based his claim was a peroxide of potassium rather than potash united to hydrogen; Murray's letter mentions Gay-Lussac and Thenard's work with peroxides briefly, but implies that he knew about it only through statements in Davy's publications; John Davy responded by chiding Murray for making experimental claims "contradicted by the ample statement of clear and decisive facts already made."

assertions. Outside his base in London, it was the judgement of Paris—still the home of chemistry—that mattered most, and everyone else in Britain would have to wait until London told them what it was. Blagden had done his work well.[166]

Conclusion: The Quill and the Press

Are "correspondence networks" real? To understand what this question might mean, let's run through the recipe for determining Blagden's during the Continental system. Go to the archives and find every letter written by or to Blagden between 1806 and 1814.[167] Tabulate the names of his correspondents, and the dates the letters were sent (if you are truly dedicated and like detective work, you can try to tabulate the dates received as well). Put it in a database. Make an elegant visualization with some network analysis software. Generate conclusions about scientific communication during the Napoleonic Wars.[168]

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[166] Blagden died in France in 1820, from an attack of after-dinner apoplexy at the house of his friend Berthollet at Arcueil. His death caused barely a ripple in the British press. But Berthollet and his other French friends ensured he got a laudatory obituary in the *Moniteur*. "None of his countrymen," it read, "have done more justice to the labors and discoveries of the French." *Moniteur*, 22 September 1820, p. 1296.

[167] Since Blagden's Continental correspondence is only intact from late 1807 to 1812 (letters outside those dates are missing), this is *in practice* an impossible task. But my purpose here is not to critique correspondence network models on the obvious grounds of incomplete survival of letters (though the unknown unknowns of archival preservation do indeed often erode confidence in historical claims made from correspondence data). Rather, I want to encourage careful thinking about what the correspondence network model *itself* captures and does not capture about communication.

[168] Readers may (possibly) have noticed I have carefully avoided using the term "network" for any explanatory or analytic purpose in previous chapters. In fact, outside this short section, the word appears nowhere in this dissertation except within quotation marks and in citations of other authors' titles. I have tried to write without relying on this much-used metaphor because I worry that its loose deployment in historical writing can tend to hinder rather than help a concrete understanding of how communication actually worked in the past. In part this is because the network metaphor has its roots in a specific historical transformation in communications technology itself. Its rise to ubiquity coincides with the mid-nineteenth-century emergence of a new kind of connected industrial world, bound together by physical links: the iron rails of the steam-train, and the copper wire of the electric telegraph. By 1900, when telegraph cables encircled the planet and tied New York to New Zealand and London to Los Angeles, men and women were living *self-consciously* in a world of networks in a way that their ancestors in 1800 had not. That is why I am very wary of using the word network to refer to communications around 1800: to do so is to risk eliding dramatic changes in people's experiences of distance, space, time, and connectedness across the nineteenth century. On the origins of the network concept after the Napoleonic Wars in French interpretations of British industrialization (building on earlier "route theory" developed in Napoleonic France) see André Guillerme, "Network: Birth of a Category in Engineering Though[t] during the French Restoration," *History and Technology* 8 (1992): 156-57; searches of Google Books indicate that the use of the term "network" in reference to

What we would create following this recipe is surely something real in the sense that it consists of nothing but the aggregate of (hopefully) factual information about things that happened in the past. Yet there is a crucial step that we must make from a list of names and dates to the concept of "Blagden's network." In this step the data from the archive are transformed into an instance of a particular *model* of communication. We use a letter to define a link between two people, and interpret this link in a certain way. We expect the aggregate of these links to capture some larger feature of Blagden's life and his communicative world.

And certainly it does—we can learn a great deal from looking at patterns of letter exchanges. But the model is not necessarily real. And the danger is that we might be tempted to let the model stand in for the historical reality of communication itself—as if the thickness of the link between Blagden and Berthollet on our network graph (its "weight") can straightforwardly represent information flows between Blagden and Berthollet. Worse, we might fall into giving "the network" its own *intrinsic* agency to shape the scientific process, forgetting that it is simply the aggregate of many human decisions to put pen to paper made across a span of time for a complex of different reasons.[169] In Blagden's case, those reasons combined ties of personal friendship with the desire to obtain information and to control its flow between London and

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communications begins to take hold in English around the 1840s, contemporaneously with the rise of railways and electric telegraphy. Similarly, I have avoided leaning analytically on another now-ubiquitous metaphor: the "circulation" of knowledge (although in this case I have permitted myself to use the word on occasion!). The historian Fa-ti Fan has noted how unreflective use of the image of circulation "tends to suggest that people, information, and material objects flowed smoothly along networks and channels." It can "impose too much unity, uniformity, and directionality on what was complex, multidirectional, and messy," and also "tends to substitute a general metaphor for a careful examination of what actually happened." I fully agree with this assessment. Fa-ti Fan, "The Global Turn in the History of Science," *East Asian Science, Technology and Society* 6 (2012): 249-58, quote on 252.

[169] Endowing "the network" with agency is an explicit methodological precept in the work of Bruno Latour; see his *Science in Action: How to Follow Scientists and Engineers through Society* (Milton Keynes: Open University Press, 1987). While the ways the network metaphor has been deployed in the historiography of science owe much to Latour's work, the correspondence network model is more indebted to the more mainstream "network science" used by sociologists, which is grounded in the rigorous mathematics of graph theory. The practitioners of network science have generally had little time to spare for Latour's rather amorphous networks, with one writer denouncing Latour's actor-network theory as being "about neither networks nor theory." Charles Kadushin, *Understanding Social Networks: Theories, Concepts, and Findings* (Oxford: Oxford University Press, 2012), 218.

Paris. But he also wrote letters with very specific practical ends in view, such as: "I will write to Chenevix because he can give me information on how to make the gun barrel experiment work." Similarly, once received, each letter was put to use in a different way. Some of Blagden's letters turned out to contain crucial new information; others were virtually useless—for example, if what they contained was already known. But the network model, in which all letters are equal, cannot contain these reasons and these uses, which depend both on the practical goals of the people writing and on other flows of information in the world outside the network's bounds.

This chapter has suggested that when we follow *information* across different media—both letters *and* print—rather than tabulate and graph recipients and dates of letters alone, the story of scientific letters in the Napoleonic world looks more complicated and interesting that can be captured by the correspondence network model. In other words, perhaps the main defect in our historical picture of how manuscript letters mediated communication in science is that we lack a full understanding of those letters' relationship to information in print. By tracking information exchanges in particular episodes in galvanism, we have seen letters functioning as tools in larger mixed systems of information-gathering, in which different kinds of print media also played a role. This is not to deny that differences between manuscript and print often mattered a great deal, but rather to take seriously the truism that manuscript and print operated concurrently in the same information world and were deployed together as tools in the same episodes of scientific practice. Studies of correspondence alone can tell us a great deal about the patterns of connection between particular people and groups, but as histories of communication they will be incomplete without factoring in the role of print; similarly, histories of science in periodicals and newspapers will be incomplete without an engagement with the history of letter-writing as a communicative form. Both historiographies, both approaches, can complement one

another, and we could do worse than follow the example of Blagden, Davy, and the rest in moving back and forth between both letters and print when tracking scientific information in the past.

Chapter 5: Ørsted's Mailing List: Communicating Discovery after Napoleon

In Copenhagen in mid-July of the year 1820, the Danish man of science Hans Christian Ørsted took a manuscript to the printer's. It contained a short description of a new discovery in galvanic electricity. Passing the electric current from a powerful battery through a metal wire, Ørsted had demonstrated a systematic effect on a magnetic compass needle positioned a few inches away: compelling evidence of a long-sought connection between electricity and magnetism.[1] As with Volta's battery, announced almost precisely two decades before, subsequent historians would see this discovery as marking a key step on the path to an electrified modern world.[2] Unaware of this future, with entirely unwitting irony Ørsted wrote up his results so that every word pointed back into the past. Avoiding his native Danish—incomprehensible to the wider European scientific community—and apparently unwilling to show national favor by choosing one of the major languages of science, French, German, or English, he composed his announcement in Latin. It was the same language Luigi Galvani had used in 1791 to write his first account of the electricity

[1] Ørsted had first noticed a weak deflection of a magnetic needle while giving a lecture in April 1820, but he did not immediately investigate the effect: see Dan Charly Christensen, *Hans Christian Ørsted: Reading Nature's Mind* (Oxford: Oxford University Press, 2013), 328-31.

[2] For example, *Oersted and the Discovery of Electromagnetism* (Norwalk: Burndy Library, 1961) by the electrical engineer, entrepreneur, and historian of science Bern Dibner. While historians' interest in Ørsted naturally remains linked to his work on electromagnetism, more recent scholarship has explored Ørsted's relationship to the interrelated intellectual currents of the *Naturphilosophie* tradition, Kantian metaphysics, and Romanticism. See Dan Charly Christensen, *Hans Christian Ørsted* (a detailed biography); also the essays in Robert Brain, Robert Cohen, and Ole Knudsen, eds., *Hans Christian Ørsted and the Romantic Legacy in Science: Ideas, Disciplines, Practices* (Dordrecht: Springer, 2007); H.A.M. Snelders, "Oersted's Discovery of Electromagnetism," in *Romanticism and the Sciences*, ed. Andrew Cunningham and Nicholas Jardine (Cambridge: Cambridge University Press, 1990), 228-40; Andrew Wilson, "Introduction," in *Selected Scientific Works of Hans Christian Ørsted*, ed. Karen Jelved and Andrew Jackson (Princeton: Princeton University Press, 1998), xv-xl. The last of these is the standard English edition of Ørsted's work. Spelling of Ørsted's name varies across early-nineteenth-century sources; the most common spelling in English and French journals was "Oersted," with "Orsted," "Orstedt," and "Oerstedt" appearing as well. Some secondary sources also use "Oersted" rather than the standard "Ørsted." I retain original spellings in titles and quotations.

in frogs. Three decades on, the old universal language of early modern learning was making another appearance at the cutting edge of experimental science.[3]

But the linguistic symmetry is misleading: Ørsted and Galvani had little in common. In fact, the other decisions Ørsted made when announcing his discovery were those of an effective self-promoter entirely up-to-date on the transformations in the norms and structures of European scientific communication that had occurred during the years since Galvani's first experiments. Galvani's *Commentarius* had been a learned discourse, the result of long and patient investigation by its publicity-shy author. It had been printed in the collection of memoirs of the local scientific academy in Bologna—not the most rapid or far-reaching of media. Galvani's work was spread outside Italy not so much through this printed text, but via the efforts of other Italian experimenters, particularly Eusebio Valli, who made a tour demonstrating Galvani's experiments on frogs in Paris and London in 1792. Galvani himself apparently made no effort to publicize his discovery beyond Italy.[4]

Ørsted was operating with a different model of the scientific process, a different sense of how intellectual activity was embedded within the flow of time. He was not expounding a systematic interpretation of an aspect of nature but communicating a single new experimental discovery, and he believed it mattered for that discovery to be known quickly, to many people. And like Volta in 1800, he saw the announcement not as a single moment of print publication, but in terms of the problem of getting information to key individuals far away. But his approach to this was far more systematic and businesslike than Volta's had been. Ørsted decided to run a

[3] Even Ørsted's contemporaries apparently found his choice of Latin rather odd, and were bemused by his idiosyncratic command of the language. "Why have not you just written your report in plain German and French, languages you have mastery of, or just in a normal Latin?," wrote the German mineralogist Christian Samuel Weiss to Ørsted in August 1820 (Quoted in Christensen, *Reading Nature's Mind*, 350).

[4] Galvani's experiments were already being tried in London when Joseph Banks wrote to his Italian correspondent Giovanni Fabbroni to ask for a copy of the Bologna Academy's volume. Banks to Fabbroni, 18 August 1792, *Banks Corr.*, 4:145-46. Valli's trip to London is described in detail in George Pearson to Banks, 22 October 1792, *Banks Corr.*, 4:163-66.

direct mail campaign. When he walked to the printer's that summer day in Copenhagen, he was not going to *publish* his results, but to have them reproduced many times over so he could package them up in letters. The printer duly ran off a small pile of copies of the four-page text, entitled *Experimenta Circa Effectum Conflictus Electrici in Acum Magneticam* ("Experiments on the effect of the electrical conflict on the magnetic needle").[5] On 26 July, Ørsted addressed forty-eight of these copies to key scientific figures in Germany, France, Britain, Switzerland, Italy, and Sweden, and put them into the mail at Copenhagen. Europe had been at peace for five years, and its efficient (if expensive) postal system would—Ørsted hoped—spread his discovery across the scientific world, reliably, and quickly.

This chapter surveys what happened to scientific communications when the Napoleonic Wars ended. A naive reading of this transition might conclude that with political and military barriers to international exchanges removed, the European scientific community was returning to normal, and its members could cease to view communication as such a pressing problem. In fact, I contend, problems of information and distance remained a preoccupation of men of science long after Napoleon's defeat and final exile in 1815 (arguably, scientists have never ceased to be preoccupied with such issues). Nor was there any possibility of the scientific world returning to a normal *status quo ante bellum*. After over two decades of wartime transformations in the institutions and practices of scientific information, the circumstances of science in 1789 no longer provided a model to revert to. The Enlightenment Republic of Letters was over. Instead, European peace in 1815 meant that scientific practitioners had to decide on how they wanted the flows of scientific information to work in the new and relatively more stable world ushered in by the Great Powers at the Congress of Vienna. From London men of science suddenly able to carry

[5] Very few copies of the printed announcement survive. The copy now in the Burndy Library (Smithsonian) can be viewed at https://archive.org/stream/Experimentacirc00Orst ; the same copy is reproduced in facsimile in Dibner, *Oersted*, 23-26.

on rapid and reliable exchanges of letters with their Parisian counterparts (but how often, with whom, and enclosing what information?) to scientific journal editors faced with a welcome but unfamiliar abundance of foreign texts for translation and reprinting, new possibilities of moving information around demanded new kinds of choices and new difficulties to resolve, posing the problem of information and communication in novel and unfamiliar ways.

Furthermore, while it was true that scientific interchanges generally did become markedly faster and more reliable when the wars ended—a few days usually sufficed to get a letter between London and Paris—peace certainly did not make communications transparent or effortless. The tyranny of distance was still felt acutely, particularly by anyone faced with the problem of shipping objects larger than a letter, like journals, books, or instruments. (Ørsted's decision to use the post for his announcement, rather than printing his results in a single journal and waiting for the information to spread from there, is telling.) And, while the nineteenth century did see steady—and eventually, with the railway, spectacular—reductions in the time and money required to get people or things from point A to point B, better transportation and infrastructure did not provide a ready-made answer to the question of how best to organize information exchanges "for the advancement of science" in all its material, social, and institutional complexity. Speed mattered, but so did other things.

In fact, peace in 1815 meant that men of science had to confront the possibility that effective communication—both within and between different national scientific groups—was not just about fast postal services and the absence of war and blockades, but involved reckoning with difficult structural problems of access to and control over information: in new journals, in academies and societies, and in the relationships among elite figures, more humble scientific practitioners, and an expanding lay audience for scientific discovery. More and more, and for a

variety of reasons, men of science began to concern themselves with specifics of the *how* and especially the *when* of information: dates of discovery, dates of receipt of news, and dates of publication (now aided and structured by the monthly rhythm of the new journals). This seemed all the more important because failures of information to reach the right people and places at the right time—failures that might previously have been conveniently blamed on wartime disruptions—incongruously persisted: letters missed their recipients, journals failed to make it across international borders, and many individuals or groups still complained of being isolated or cut off. As we will see in this chapter, all this had consequences for the ways particular episodes of discovery or intellectual exchange unfolded. As they had during the long years of war, people doing science continued to be preoccupied with problems of information, distance, and community, and, like Ørsted with his direct mail strategy, they continued—as they still continue—to devise creative and practical solutions.

Mapping Ørsted's List: Change and Continuity in the Scientific World

Ørsted's findings were startlingly simple—so simple that his contemporaries must have wondered how the phenomena he reported had gone undetected for so long, particularly as the relationship between electricity and magnetism had been a long-running topic of speculation.[6] He took a straight section of metal wire and suspended it a few inches above his worktable, and then a hung a magnetized needle on a thread so that the needle was positioned to one side of the wire, and a little below it. When a powerful galvanic battery was connected to the wire's ends, the needle deflected away from north. If the needle was instead placed above the wire (on the

[6] Johann Wilhelm Ritter, who was a good friend of Ørsted's (and whom we met in Chapter 1) had even claimed to have found experimental proof of a link in 1803. His experiments—almost universally rejected as invalid, or simply ignored—were "communicated" by Ørsted, who was in Paris at the time, to the *Journal de Physique*: *JP* 57 (1803): 406-9.

same side as before), the deflection was in the opposite direction; if placed on the same level as the wire, the needle stayed pointed north but tilted upwards. Ørsted tried placing glass, metal, wood, water, resin, and stone between the wire and the needle; the effect remained the same. With some additional supporting trials, he concluded that he had produced what he called a "conflict of electricity" ("conflictus electrici") in concentric circles around the wire. (Later investigators would interpret this as a magnetic field.) These were the results Ørsted transmitted across Europe in July 1820. "This discovery," Humphry Davy proclaimed a few months later, "from its importance and unexpected nature, cannot fail to awaken a strong interest in the scientific world: and it opens a new field of inquiry, into which many experimenters will undoubtedly enter."[7] Davy's words echoed those he had once used to describe the advent of Volta's battery, two decades before, and his prediction proved true again.

Ørsted's rather idiosyncratic mass-mailing campaign to publicize his work in electromagnetism provides us with access to one man's mental map of who, where, and what mattered in the physical sciences in Europe in 1820, five years after the battle of Waterloo and Napoleon's final exile to St. Helena. His list of addressees targeted, as he put it, "the most important scientific centers in Europe," viewed from the perspective of someone situated on the periphery of European science.[8] He chose to write to some of the foremost names interested in galvanism: Davy and William Hyde Wollaston in Britain; Laplace and Biot in France; Pfaff and Erman in Germany, and Berzelius in Sweden. But, attuned to the new print-based landscape of scientific information that had emerged during the last twenty years, he also particularly targeted

[7] *Philosophical Transactions* 111 (1821): 8.
[8] In an autobiographical account printed in 1828, Ørsted related that "this half-sheet [the *Experimenta*] was now sent on the same post-day to the most important scientific centers in Europe. The experiments were soon repeated in all countries in which there were friends of science, and the greatest reward an inventor can enjoy, that of seeing his invention become the object of the most industrious investigation, seeing it expanding and fructifying, was his [i.e. Ørsted's] to the fullest." *Conversations-lexicon, eller encyclopaedisk haandbog*, ed. H. A. Kofod (Copenhagen: Goldin, 1828), 28:537. Translation from Danish taken from Dibner, *Oersted*, 18.

editors of Europe's leading scientific journals, who he hoped would reprint his experiments and so make them widely available. He wrote to François Arago, now joint editor (with Gay-Lussac) of the *Annales de Chimie*, Henri Marie Ducrotay de Blainville of the *Journal de Physique*, the Swiss Marc-Auguste Pictet whose *Bibliothèque Britannique* had become the "*Bibliothèque Universalle*" after the end of the wars, Gilbert of the *Annalen der Physik*, and Johann Schweigger, Gehlen's replacement running the *Journal für Chemie und Physik*. The British journals were represented by the chemist Thomas Thomson's *Annals of Philosophy*, founded in 1813.[9]

One eminent name is conspicuously absent from Ørsted's mailing list: Sir Joseph Banks. As Ørsted had no doubt learned from the newspapers, Banks had died a month before, after over forty years as President of the Royal Society. Banks's death, and the contest over who would succeed him, precipitated what would turn out to be a long-running debate over the future of British science. It also meant a sudden eclipse of that one-time social center of the British scientific world, Banks's house and library in Soho Square.[10] While the power of Banks's house as the central office for scientific information in Britain had been on the wane—challenged by scientific journal editors like William Nicholson, Alexander Tilloch, and (after 1813) by Thomas Thomson, Banks's death still deprived London men of science of their private, clubbable location where news could be aggregated and shared. Such informal semi-private exchanges

[9] An incomplete mailing list of thirty-three names survives among Ørsted's papers and is transcribed in Christensen, *Hans Christian Ørsted*, 349; Christensen finds (p. 345) from Ørsted's expenses ledger that he in fact sent the announcement to forty-eight people. The incomplete list of thirty-three is as follows. Britain: Davy, Wollaston. France: Ampère, Arago, Biot, Blainville, Boisgiraud, Fresnel, Hachette, Laplace, Petit, Savart. Germany: Althaus, Beck, Buch, Döbereiner, Erman, Gilbert, Meier, Moll, Neef, [Johann Wilhelm Andreas] Pfaff, [Christian Heinrich] Pfaff, Poggendorff, Prechtl, Schweigger, Seebeck, Yelin. Italy: Confiliachi, Ridolfi. Switzerland: Pictet, de la Rive. Sweden: Berzelius. Some other recipients, like Thomson Thomson, can be deduced from evidence in journals and letters.

[10] In his will, Banks left his library and herbarium to the care of his librarian, Robert Brown, with the condition that it would eventually go to the British Museum. Brown continued to live in the Soho Square house after Banks's death and donated the collection to the Museum in 1827. Banks's Soho Square residence therefore remained an important resource in natural history during the 1820s even when it had ceased to be a central site of scientific news and sociability. See Harold Carter, *Sir Joseph Banks* (London: British Museum, 1988), 521, 542.

certainly did not cease to be important for men of science, but they were never again so centralized and so dominated by a single personality as they had been in the Banks era.

The year 1820, the year of Ørsted's discovery and of the end of the Banks regime in Britain, is therefore as good a date as any to mark the departure of the generation of elder statesmen of science who remembered what the scientific world had been before the French Revolution. Science lost Charles Blagden that year as well—the Royal Society's great Francophile having died in March at his old friend Berthollet's house in Arcueil just outside Paris. Berthollet himself died two years later, the last of the group that had worked closely with Lavoisier to reform chemistry in the 1780s. A younger cohort who had begun their scientific careers during the long wars moved naturally into the places of the old guard—individuals like Davy (aged 41 in 1820), Gay-Lussac (also 41), Ampère (45), Biot (46), and Thomas Thomson (47). Ørsted himself was 42 in 1820. On the heels of these new leaders, an even younger generation was emerging for whom the coming of the French Revolution was not even a childhood memory, and who would eventually carry the torch into the mid-nineteenth century—among them John Herschel (28), François Arago (34), Charles Babbage (28), William Whewell (26), and Michael Faraday (28). It is therefore hard to disentangle the reasons behind transformations in the scientific landscape in the wake of 1815, to say how much change was due to Napoleon's fall and the coming of peace, and how much can be attributed to the inevitable arrival of a new generation of leaders with no memory of the Republic of Letters of 1789 and no personal identification with the pre-war patterns of international science.

On the Continent at least, the final throes of the Napoleonic wars—beginning with Napoleon's defeat at the Battle of Leipzig in October 1813—were naturally a turbulent and apparently not particularly productive period for scientific life. In November 1813, the Genevan

savant Marc-Auguste Pictet wrote to Davy that Britain was now the place where the sciences were "cultivated with more speed and success than anywhere else, on account of the tranquility you enjoy while Europe is upside down."[11] With the collapse of the French Empire six months later, savants who had enjoyed high favor and patronage under Napoleon were understandably worried about their future. Just after Napoleon's first abdication in April 1814, Jean-Baptiste Biot seized "the first moment when communications are reliable" to write to Charles Blagden in London to tell him that he was anxious about the possibility of imminent civil war in France, and was considering taking refuge in England to continue his scientific work there.[12]

When Biot's letter arrived in London eight days afterwards, Blagden as usual read it out loud in Joseph Banks's library, and a discussion later ensued at the Royal Society in which "all agreed as to probability of civil war in France." Anti-French feeling ran high in Britain: "Sir JB," Blagden recorded with consternation in his diary, "said they had not been punished enough, Paris should have been burnt!"[13] Once the prospect of civil war had given way to what looked like the fairly stable restoration of the Bourbon monarchy, Blagden prepared to cross to France, which he had not seen for over ten years. Arriving in Paris on 12 September, he was welcomed by his old friends Benjamin Delessert and Madame Gautier, and later went to stay with Berthollet at Arcueil. In Paris, he resumed his role as the Royal Society's informal ambassador that he had filled a decade earlier during the Peace of Amiens, attending meetings of the Institut, visiting leading French savants like Cuvier and Laplace, and sending bulletins of scientific news back to Joseph Banks in London.[14] For his part, Banks arranged to keep Blagden—and therefore his French friends—connected to the latest developments in British science, sending him issues of

[11] Pictet to Davy, 8 November 1813, *Pictet Corr.*, 2:164-166.
[12] Biot to Blagden, 20 April 1814, RS CB/1/1/263.
[13] *Blagden Diary*, 28 April 1814. Peace was not officially concluded between Britain and France (now represented by the restored Louis XVIII) until 30 May, six weeks after Napoleon's fall.
[14] *Blagden Diary*, September-October, 1814.

Thomas Thomson's *Annals of Philosophy* (now Britain's preeminent journal for chemistry), as well as his own letters filled with news.[15] Blagden and Banks coordinated to fill some of the lingering gaps left by the wartime disruptions: when Banks heard that the Institut did not even have a copy of the *Philosophical Transactions* for 1811, he shipped one to Bladgen to present to the French. He also sent Bladgen and his French friends information on a new invention that was the talk of London, Friedrich Koenig's steam-printing press, installed at the *Times* newspaper in November 1814.[16]

Of course, as Blagden discovered six months later, one last short act remained in the Napoleonic drama. In March 1815, when Blagden was still in Paris, news came up the semaphore telegraph that Napoleon had escaped from Elba with a small band of soldiers, landed on France's Mediterranean coast, and was marching north. Laplace assured Blagden that Napoleon had "no chance" and that the crisis would soon be over. Acting on rather better advice from Madame Gautier, Blagden decided to play it safe and decamped to the seaport of Le Havre in Normandy, where he soon heard some terrible news: Marshall Ney, sent with 6,000 men to apprehend Napoleon, had joined him instead. On 21 March word reached Le Havre that the Bourbons were gone and the Tricolor was flying again on the Tuileries Palace. It was time to leave. The next day, Blagden was standing on the deck of a ship watching the French coast recede. Back in London, he returned to his old wartime station in Joseph Banks's library, piecing together the political and military news from the Continent: the mobilization of the allied powers, the battle of Waterloo and the occupation of Paris, Napoleon's second abdication, his

[15] Banks to Blagden, 2 January 1815, *Banks Corr.*, 6:117-19 (mis-dated as "2 January 1814").
[16] Banks to Blagden, 14 February 1815, *Banks Corr.*, 6:165-66.

surrender to the British Navy, and second exile. A year after Waterloo, with Napoleon securely confined on remote St. Helena in the South Atlantic, Blagden finally returned to France again.[17]

Nonetheless, Blagden would have been pleased to observe that despite the turbulence of 1814 and 1815, the institutions and personnel of Napoleonic science—like so much else of the Napoleonic state—proved very resilient in surviving the Emperor's fall. The Première Classe of the Institut National did not cancel a single weekly meeting in all of 1814 and 1815, processing scientific information with uninterrupted bureaucratic rhythm through three changes of regime and two military occupations of Paris.[18] It was eventually re-branded as the Académie Royale des Sciences in March 1816—distressingly, now only third in precedence among the French Academies, but with a remarkable continuity of personnel. Most of Napoleon's most favored savants like Berthollet, Laplace, and Chaptal retained their positions, with Laplace, along with Georges Cuvier, managing a particularly impressive transition to new favor under the Bourbons. Only the astronomer Gaspard Monge and the engineer Lazare Carnot were purged, but they had been Regicides in the Convention of 1793.[19] Journal publication was also relatively unaffected in 1814-15, during which the *Annales de Chimie* continued to appear each month, its pages registering not a trace of the drama of Napoleon's exile, return, and second defeat. Clearly, the disruptive effects of 1814-15 on French science did not go nearly as deep as those felt during the wartime crisis of 1792-94, when the Jacobins had abolished the Académie des Sciences, journals had suspended publication, and Lavoisier had died under the guillotine. French science never again enjoyed quite the same fabulously exclusive prestige and cultural authority—relative to other areas of intellectual life—as it had done under the first Empire, but the deep symbiosis

[17] *Blagden Diary*, March-April 1815.
[18] *PVSA* 4 (1812-15).
[19] On the arrival of the new order see Robert Fox, *The Savant and the State: Science and Cultural Politics in Nineteenth-Century France* (Baltimore: Johns Hopkins University Press, 2012), 9-39.

between science and the French state which Napoleon had fostered remained intact, observed by young British would-be reformers like Charles Babbage with envy and admiration.[20]

In sum, the underlying pattern of Ørsted's mailing list of 1820 would not have looked too different had he written it in 1812, with Napoleon still securely on the throne of France. The end of Napoleon's Empire, the restoration of the Bourbons, and the advent of Congress Europe did not witness any deep structural shift or rupture in European science, analogous to the exclusion of Germany (and the German language) from the international scientific scene after World War I.[21] Of course, the natural loss of members of the older generation—like Joseph Banks—meant that some names were missing from Ørsted's list. And new younger figures had risen to prominence, like Arago, who had become joint editor of the rechristened *Annales de Chimie et de Physique* in 1816. But the broad map—as viewed from Copenhagen—looked much the same. Paris remained the dominant center (ten of the names on Ørsted's list were Parisian savants), with London as Paris's main competitor. Ørsted sent about twenty copies of his announcement to German men of science, but they were scattered across hundreds of miles of territory, primarily in small university towns.[22] Only a handful of copies went beyond France, Britain, and the German lands: two to Geneva (now restored to the Swiss Confederation), two to Italy, and one—for Berzelius—to Sweden. (Ørsted presumably considered the American scientific community too distant, or too unimportant, to warrant sending anyone there a copy of his work.)

[20] Most forcefully expressed in Babbage's *Reflections on the Decline of Science in England* (London: Fellowes, 1830). On Babbage's book (and contemporaneous British projects for the future of science) see James Secord, *Visions of Science: Books and Readers at the Dawn of the Victorian Age* (Chicago: University of Chicago Press, 2014), 52-79.

[21] On the Boycott and its consequences see Michael Gordin, *Scientific Babel: How Science was done before and after Global English* (Chicago: University of Chicago Press, 2015), 159-85.

[22] On the personnel, geography, and institutional structure of German physical science after 1815 see Christa Jungnickel and Russell McCormmach, *Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein* (Chicago: University of Chicago Press, 1986), 1:3-62.

Ørsted had complete freedom in choosing his recipients. But, like Volta in 1800, once the letters were sent, his ability to control how information about his discovery spread came to an end. His role was now to wait and see what others would do with what he had given them.

Encountering Electromagnetism: Arago and Davy

Ørsted's pan-European mass mailing campaign is testimony to one change the peace certainly did usher in. After years of disruptions to postal routes, it was at last possible for members of the European scientific community to use the mail to send letters to one another relatively quickly and reliably. Britain and the Continent were now finally linked again by regular packet-boats carrying the mail, and Paris had recovered the connections to Germany and Italy which it had lost during the collapse of Napoleon's Empire in 1813 and 1814. "Letters Seem to Come from Paris much Quicker than formerly" remarked Joseph Banks to Blagden in 1815, impressed that he had just received a letter sent by Blagden from Paris to London only three days after Blagden wrote it.[23] It made a tremendous contrast with the last few months of the wars, when Banks had described "communications of correspondence" with the Continent simply as "lock'd up."[24] Not all of Blagden's letters home were as swift; bad weather or unfavorable winds in the Channel could still introduce delays, but these were a small annoyance compared to the disruptions to exchanges of scientific information between Britain and the Continent of the previous ten years. Moreover, the peace made foreign communication not only faster, but more generally accessible. No longer was it necessary to have Blagden's extensively-cultivated connections to diplomats, bankers, and state functionaries to get letters though, although sending letters remained, as it had always been, a fairly expensive luxury. With mail moving back and forth between London and

[23] Banks to Blagden, 2 January 1815, *Banks Corr.*, 6:117-19 (mis-dated as "2 January 1814").
[24] Banks to Lord Liverpool, 14 January 1814, *Banks Corr.*, 6:120-21.

Paris with a swiftness that would not disgrace a letter sent in the twenty-first century, we might be tempted to conclude that those doing science could afford to be less concerned about access to information from distant people or places after 1815.

Yet the story of the spread of electromagnetism after Ørsted's announcement shows this to be a false view of things. Peacetime conditions and reliable postal services did not (of course) mean that new scientific information became universally available to everyone potentially interested in receiving it. Even when someone wanted to publicize a new discovery as widely and rapidly as possible, as Ørsted did, distance and other circumstances might still intervene to fracture the announcement into a series of geographically-scattered and temporally-staggered encounters. Although Ørsted sent out his printed sheets from Copenhagen on the same day, and most of them probably arrived at their destinations within a week or so of one another, this did not guarantee that the experiments in them would be tried, that journal editors would reprint them immediately—or even that they would be read at all. Inevitably, different people in different places learned of the discovery at different times. As with Volta's battery, which was reproduced in Britain months before it was known in France, replication of Ørsted's work on the Continent and in London was not simultaneous—affecting who was able to garner credit for later discoveries by building on the first. This time, however, it would be the British who were slow getting off the mark.

While Ørsted was busy applying seals to dozens of letters in Copenhagen, the young savant François Arago was packing his bags in Paris for a trip. During the Napoleonic wars Arago had survived capture by Spanish troops, a prison escape, kidnap by Barbary corsairs, quarantine in the Marseille lazaretto, and even the examination system of the École Polytechnique, all before being elected to the Institut National—at the age of 23. Now 34, he was

already a formidable presence in French science, wielding (with Gay-Lussac) the joint editorship of the *Annales de Chimie et de Physique,* and nursing a quietly intense Republican dislike of the restored Bourbon regime. As the copy of Ørsted's *Experimenta* addressed to him made its way from Denmark to Paris, Arago left the French capital and took the road south.

Geneva is 860 miles from Copenhagen, yet it turned out to be one of the first places where Ørsted's work was repeated, and also a crucial site for its wider distribution in print. Ørsted had sent copies of the *Experimenta* to the Genevan savants Marc-Auguste Pictet and Charles-Gaspard de la Rive—probably because he hoped they would quickly reprint his work in their journal, the *Bibliothèque Universalle* (the retitled *Bibliothèque Britannique*). And, as it happened, Geneva was Arago's destination too. Ørsted's letter arrived there some time in the first half of August, and Pictet publicly announced its contents to the local *Société de physique et d'histoire naturelle* on the 17th.[25] A few days later, the Swiss savants recruited Arago to join forces with them in an attempt to repeat Ørsted's experiments, which Arago thought appeared "singular," but "accompanied by too many details to prompt any suspicion of error."[26] Using a powerful battery belonging to de la Rive, they set to work. All were impressed to see the magnetic needle affected by the wire just as Ørsted had claimed.

Being journal editors as well as experimenters, Arago and the Swiss hastened to put Ørsted's announcement into print as soon as possible in their respective publications. They collaborated on a translation from Latin into French; Pictet prepared this for printing in his *Bibliothèque,* and Arago sent a copy to Paris for immediate printing in the *Annales*.[27] Ørsted's experiments duly appeared in print in both Geneva and Paris at the beginning of September; a

[25] Note transcribed in *Pictet Corr.*, 4:546.
[26] See Arago's editorial description prefacing his reprint of Ørsted's *Experimenta* in *AC* 14 (1820): 417-25; also the Swiss savants' reprint and comments in *Bibliothèque Universelle* 14 (1820), 274-84.
[27] The French translations printed in the *Annales* and the *Bibliothèque Universelle* are identical.

few days later, Arago himself was back in Paris, announcing the verification of Ørsted's electromagnetic phenomena to a session of the Académie on 4 September.[28] By then, of course, as Arago noted in the *Annales*, Ørsted's claims had already reached Paris by another route, in the copies of the Latin *Experimenta* Ørsted had mailed to Biot, Ampère, Laplace, and other savants.

The news of the connection between electricity and magnetism prompted Parisian savants to throw themselves into investigating (and, in the true French analytical spirit, quantifying) the new phenomena. Ampère embarked on an intense work program which produced a new report to the Académie almost every week from mid-September to November, including his famous experiment demonstrating the mutual force between two parallel current-carrying wires. Jean-Baptiste Biot and Félix Savart announced at the end of October that they had determined the precise force on a magnetic pole acted on by a straight current-carrying wire: it was proportional to the reciprocal of the distance, and (in line with what Ørsted had said) was tangent to a circle around the wire.[29] Arago himself had lost no time in starting his own investigations in September. He discovered that the conducting wire *itself* acted as a magnet (Ørsted had demonstrated only its *effects* on a magnetic needle), and proved this conclusively with iron filings, which stuck to the wire when the current was on and fell off after the current was switched off. The experiment worked even if the wire was made of copper, a non-magnetizable metal. Arago was also able to use the electric current to permanently magnetize a sewing needle. He went on to investigate the magnetic effects when the wire was bent into a helix (as Ampère was also doing). Publishing was easy for Arago: he simply printed all his experiments in the next issue of his own *Annales de Chimie* (appearing at the beginning of October). He signed off his

[28] *PVSA* 6:83-84 (4 Sept 1820). Arago demonstrated the experiments to the assembled savants at the Académie's next weekly meeting.
[29] *PVSA* 6:94-101 (18 Sept - 13 Nov 1820). On these first investigations into the mathematization of electromagnetism see Olivier Darrigol, *Electrodynamics from Ampère to Einstein* (Oxford: Oxford University Press, 2000), 1-41.

report with a hope that the electromagnetic phenomena might provide an explanation for the mysterious Aurora Borealis.[30]

Arago had been lucky: even though he had left Paris at the end of July (and so missed the letter Ørsted had addressed to him), he had gone to a place (Geneva) where Ørsted's *Experimenta* reached him by another route. But traveling away from a center of scientific information was a risky venture for anyone who cared about staying up-to-date on the news of discovery. For others, it could mean disconnection and delay. To run at the front of the experimental race, it helped to stay put.

In June 1820 Humphry Davy had cut short another long Continental tour to return quickly to Britain, brought back by news of a serious decline in Sir Joseph Banks's health and thus a potential power vacuum at the head of the Royal Society.[31] But after some intense canvassing for votes during the crucial few weeks after Banks's death on 19 June (the election of Banks's replacement would not be held until the Society's anniversary meeting in November), Davy apparently felt secure enough in his own prospects to take a trip to Scotland to indulge in his favorite country pastimes of shooting and (most of all) angling. A highlight of his tour was a memorable gathering at the house of the poet and novelist Sir Walter Scott, where the guests also included the chemist William Hyde Wollaston, who had been named interim President of the Royal Society just after Banks's death.[32] Wollaston was another of the addressees of Ørsted's *Experimenta*, and so it happened that two of Ørsted's key British targets in his mailing campaign were far away from their usual haunts, while Ørsted's letters to them presumably sat waiting,

[30] *AC* 15 (1820): 93-102.
[31] Davy to Grace Davy, 16 June 1820, using transcription from Davy Letters Project (http://www.davy-letters.org.uk/); original in Science Museum Archive, Wroughton, MS 333/33.
[32] Davy and Wollaston's visit (they may have been traveling together) was recalled by Scott's son-in-law in John Gibson Lockhart, *Memoirs of the life of Sir Walter Scott* (Edinburgh: Cadell, 1838), 238-46. In the last years of his life, Davy even published a book on angling: *Salmonia: Or Days of Fly Fishing* (London: Murray, 1828).

unopened, in London.[33] Ørsted's work also appears to have been delayed reaching the one other British addressee (that we know of), journal editor Thomas Thomson, then based in Glasgow—or at least he did not manage to print an English translation in his journal until the beginning of October.

Arriving back in London around 10 October, Davy finally crossed paths with the news of Ørsted's experiments—hearing about them for the first time when he read the English translation that had just appeared in Thomson's *Annals*.[34] Eager to make up for lost time, he set to work. "I have been caught by an inquiry of the greatest importance & till I can conclude I can not stir," he wrote to his brother, adding that he had found the wire to be magnetic and that he hoped to explore the implications for "the theory of the Earth." The information was to be kept under wraps—"do not say any thing on this subject." As if to underline that he had not a moment to spare, he added that "I write from the table where I am *magnetising*."[35]

A month later, on 16 November, the results of Davy's investigations were read before the Royal Society, couched in the form of a letter to Wollaston, who was still the interim President.[36] Like Arago, Davy used iron filings to demonstrate that the wire itself became magnetic when the current was flowing. He was also able to permanently magnetize steel needles, again unknowingly following in Arago's footsteps, and later small steel bars. He managed to show the same effect using the electricity from an electrical machine rather than a battery, and also tried to clarify the spatial pattern of the magnetizations by sticking needles to a cardboard circle around the wire, with results, he said, that seemed to support an idea of Wollaston's: "revolving

[33] Assuming they arrived at all: there is no firm evidence that Davy or Wollaston actually received Ørsted's letters.
[34] For the timing of Davy's return: Davy to Faraday, 4 October 1820, *Faraday Corr.*, 1:201. Faraday later remembered that both he and Davy first heard of Ørsted's work from the *Annals*: Letter by Faraday of 10 May 1836 printed in *PM*, 8 (1836): 521-22.
[35] Davy to John Davy, 19 October 1820, RI Davy MS HD/26/B/10.
[36] *Philosophical Transactions* 111 (1821), 7-19. The letter to Wollaston was dated 12 November 1820.

magnetism." While declining, in good Baconian fashion, to "indulge myself by entering far into the theoretical part of this subject," Davy nonetheless wondered whether "the magnetism of the earth may not be owing to its electricity, and the variation of the needle to the alterations in the electrical currents of the earth in consequence of its motions, internal chemical changes, or its relations to solar heat." Like Arago, he also mentioned the Aurora Borealis.[37]

In his Royal Society paper Davy had expressed some concern about whether others, who had perhaps had the information earlier, had beaten him to his latest observations. "Where there are so many objects of research obvious," he noted, "it is scarcely possible that similar facts should not be observed by different persons." Nonetheless, he declared, "the progress of science is, however, always promoted by a speedy publication of experiments; hence, though it is probable that the phenomena which I have observed may have been discovered before, or at the same time in other parts of Europe, yet I shall not hesitate to communicate them to you."[38] His fears proved well-founded. Days afterwards, the September issue of the *Annales de Chimie et de Physique* arrived in London, and he learned that Arago had already found many of his results. The printed version of Davy's paper in the *Philosophical Transactions* carried a long footnote acknowledging the earlier French discoveries.[39]

Like Ritter twenty years before after learning he had been scooped on his experiments with the battery, Davy was careful to spell out when *precisely* he had heard about the prior French experiments, stating that the key issue of the *Annales de Chimie* had only arrived in London on 24 November, a week after the reading of his paper. Delays of information like this

[37] Ibid., 16-17. Davy also tried experiments with multiple wires (as Biot had done in France), showing that two wires with opposite directions of current repelled and that those with the same direction of current attracted.
[38] Ibid., 8. Here, Davy almost certainly meant "publication" not in the sense of distribution of a printed text, but in the sense of a public announcement (in this case, to the Royal Society).
[39] Davy did manage to maintain a claim to have been the first to successfully repeat the experiments using "common electricity" from an electrical machine rather than from the battery. Davy to Arago, 15 December 1820, using transcription from Davy Letters Project; original at Uppsala University.

had been familiar enough under Napoleon's Continental blockade, but in 1820, with Europe at peace, they carried a different valence. At any time after Arago had announced his experiments at the Académie in September, someone in Paris could have mailed a letter to Davy describing Arago's work, and it would have reached London within a few days. But no one did. Charles Blagden was dead, and the savants in Paris were happy to rely on their journals to transmit such information to Britain. Direct personal connections still mattered—Davy exchanged a few letters with Arago and Ampère in 1820-21, and Faraday maintained an occasional correspondence with Paris for years—but the role of the go-between "intelligencer" managing private correspondence was, in some respects, giving way to the role of the journal editor as a manager of information. More systematic, more widely-distributed, but maybe slower.

This greater reliance on monthly journals after 1815 went hand in hand with the relative decline of the newspapers as an information channel between specialists in London and Paris. During the Napoleonic era, London men of science might have hoped to hear about announcements like Arago's from the *Moniteur*. But the Bourbons did not have Napoleon's passion for science nor his desire to make scientific production appear central to the public business of the state. Under the Restoration, the *Moniteur* (which remained the official government newspaper) did not print much scientific information.[40] And, while the Restoration French press in general certainly did not lose its interest in scientific news (and appears to have paid more attention to it under the July Monarchy—Faraday read reports of the Académie's meetings in *Le Temps* in the early 1830s), no newspaper ever again represented the official voice of French science, as the *Moniteur* had done.[41] By 1820, the role of reporting news from Académie meetings had largely been taken over by the very publication that brought Davy the

[40] Based on a general survey of the *Moniteur* for 1820.
[41] Hachette to Faraday, 30 August 1833, *Faraday Corr.*, 2:147.

news that many of his electromagnetic experiments had, as he later put it to Ampère, "lost the merit of novelty": Arago's own *Annales de Chimie et de Physique*.[42] In 1835, as Perpetual Secretary of the Académie, Arago was to make these reports of meetings an official matter, founding the Académie des Sciences's *Comptes rendus hebdomadaires* to provide a weekly record of the Académie's activities.[43]

With both Charles Blagden and Joseph Banks gone, and with the newspapers standing in a different relationship to elite scientific activity, British and French science were in a process of reconfiguring their links with one another. In 1820, as Davy found, there was still a sizable cross-Channel information lag: a window of uncertainly in which novel experiments and novel claims could be announced only to prove later not so novel after all, and credit could be provisionally awarded only to be annulled by new information arriving about prior work.

Nonetheless, at this stage in his career Davy could afford not to worry. In 1799, aged 20, he had scrawled the audacious words "Davy and Newton" across the pages of a notebook.[44] Now, he must have felt on the brink of fulfilling his destiny as the Isaac Newton of British chemistry. On 30 November, two weeks after the reading of his paper on electromagnetism, and at the same meeting at which the Society voted to award their Copley medal to Ørsted, Davy was elected President of the Royal Society of London.

Davy's electromagnetic experiments probably earned him few, if any, extra votes for his presidency. Far more important for the result was the canvassing for support Davy had done among the Fellows during the summer, and the withdrawal of the other leading candidate,

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[42] Davy to Ampère, 10 February 1821, in *Correspondence du Grand Ampère*, 3 vols. (Paris: Gauthier-Villars, 1936-1943), 2:563-64.

[43] On the founding of the *Comptes rendus* see Csiszar, "Broken Pieces of Fact," 76-100. Nonetheless, newspapers did continue to spread much scientific information even to expert readers right through the nineteenth century. For an example, see James Secord, "Extraordinary Experiment: Electricity and the Creation of Life in Victorian England," in *The Uses of Experiment: Studies in the Natural Sciences*, ed. David Gooding, Trevor Pinch, and Simon Schaffer (Cambridge: Cambridge University Press, 1989), 337-83.

[44] RI Davy MSS HD/20/b, p. 182. Davy appears to have been under the influence of nitrous oxide when he did so.

Wollaston (a favorite of young Cambridge men like John Herschel, William Whewell, and Charles Babbage, as well as of old anti-Banks stalwarts like the mathematician Charles Hutton).[45] Nonetheless, the timing of the announcement of his electromagnetism work, at the last meeting before the election, must have been, from Davy's point of view at least, a strong statement about the sort of person who ought to fill Newton's historic chair. The key question that summer was whether the president should be an aristocratic grandee able to act as a scientific patron—the Duke of Somerset and Lord Spencer were possible candidates—or whether the honor should go to someone distinguished purely as an active "man of science."[46] (Banks had been both wealthy landed gentleman and, at least by a charitable interpretation, a distinguished botanist, so his legacy here was ambiguous.) "All sorts of plans, speculations and schemes are afloat, and all sorts of people proper and improper, are penetrated with the desire of wielding the sceptre of Science," wrote the young Charles Babbage to his friend William Whewell that summer, wondering whether "this elective throne shall be filled by a philosopher or peer, a priest or prince." "The Society," he scoffed, "is in a position of unstable equilibrium."[47]

Davy's views were that the presidency of the Royal Society—"the highest situation a Scientific man can fill"—had to be "a reward of scientific labours, and not an appendage to rank or fortune." He also thought it crucial that the holder have a strong scientific reputation abroad. "I have long felt that I wanted a position or situation in the country which might fix & direct my

[45] Davy's most important move appears to have been to persuade Wollaston to marshal his "friends" (i.e. supporters) in Davy's cause. Davy to Wollaston, 25 June 1820, RS MM/6/87.
[46] On Davy's election to the Presidency and his later difficulties while in the chair, see David Philip Miller, "Between Hostile Camps: Sir Humphry Davy's Presidency of The Royal Society of London, 1820–1827," *British Journal for the History of Science* 16 (1983): 1-47. On the end of the Banks era and subsequent projects for reform more generally see also David Philip Miller, "The Royal Society of London, 1800-1835: A Study in the Cultural Politics of Scientific Organization" (PhD diss., University of Pennsylvania, 1981).
[47] Babbage to Whewell, 15 May 1820, quoted in Miller, "Between Hostile Camps," 24.

efforts & call forth my powers" he wrote to his old patron, Davies Gilbert.[48] But, as Davy's critics among the young reformers must have been aware, Davy's own scientific labors had tailed off in recent years; after his marriage to a wealthy widow and resignation from his lecturing position at the Royal Institution in 1812, he spent far more time traveling on the Continent, fishing and shooting, and cultivating an image as a "man of fashion." Even Joseph Banks, gentleman of science *par excellence,* had disapproved; for him, it was a question of Davy rising above his station. "It is a cursed consideration," he wrote to Blagden (complaining of Davy's "shooting & fishing" expeditions), "that all our Chemists become Gentlemen, who have more Pleasure in dissipation than in Labor."[49] Davy's one great discovery after leaving the Royal Institution had been the miners' safety lamp in 1815, which had earned him a baronetcy. His experiments with electromagnetism—which, as we saw, he later found to his disappointment had been mostly done already in Paris—were, in part, an attempt to remind men of science of his credentials as an active researcher, who would not be above getting his hands dirty in a laboratory if called upon by science to do so.

Davy's Presidency was less of a reward than he might have hoped. Caught between vocal young proponents of wholesale reform like John Herschel and Babbage, and old "Josephites" nostalgic for the Banks era, he was unable to please either faction.[50] Davy was relatively popular during the first years of his presidency, but after that his support among the Fellows fell away. His resignation in 1827, after a debilitating stroke forced him to travel for recuperation on the

[48] Davy to Grace Davy, 22 December 1820, RI HD/26/A/18; Davy to Thomas Poole, 10 December 1820, in *John Davy, Memoirs of the Life of Sir Humphry Davy*, 2: 176-77 (original not extant); Davy to Gilbert, 25 June 1820, transcription from Davy Letters Project (original in Royal Institution of Cornwall, County Museum, Truro). Banks had nominated Gilbert (formerly known as Davies Giddy) as a preferred successor to his Presidency, but Gilbert had declined to stand. In his letter to Gilbert, Davy also acknowledged personal wealth was necessarily a factor, promising he would be able to support living arrangements "not disgraceful to the president of the Royal Society."
[49] Banks to Blagden, 3 October 1816, *Banks Corr.*, 6: 217-19.
[50] I take the term "Josephites" from Miller, "Between Hostile Camps," 26; Miller draws it from an 1820 letter from Edward Bromhead to Charles Babbage.

Continent (he was to die at Geneva two years later), brought divisions between reformers and conservatives into the open, precipitating another succession battle.

One of Davy's major problems was that he was not in a position to exert a firm control over the flows of scientific information, as Banks had done. He certainly tried—in a more limited way—to follow Banks in making his own house a scientific center, holding "conversatzione"-style gatherings every Saturday evening. As his brother remembered, "the subjects of interest of the day were there discussed, and curious information obtained from the best sources, and knowledge exchanged between individuals, as in a great mart of traffic, each giving and receiving according to his acquirements and wants. … An evening seldom occurred without some novelty in art, science, or nature, being brought forward—as the bones from the Kirkdale Cave, or a new chemical compound, or a magnetical experiment, or a recently discovered mineral, or some new instrument or apparatus; and a great zest was given by the presence, as was generally the case, of the inventor or discoverer, who was always willing to offer explanation, and give detailed information to those who were desirous of receiving it."[51]

But in the end, Davy's style as President probably served more to highlight the difference between him and Banks—he did not have the resources (or perhaps the inclination) to run the semi-public open house that Banks had operated, with gentlemen of science wandering in and out every day, and with a dedicated space like Banks's library for them to gather in a club-like atmosphere, read issues of French and German newspapers and journals, and exchange news (the Athenaeum Club, founded by Banks ally John Wilson Croker with Davy's help in 1824, later filled this void in grand style). Nor, unlike Banks, did Davy have a loyal aide-de-camp like Charles Blagden, whose death in 1820 meant that there ceased to be the same strong personal

[51] John Davy, *Memoirs of the Life of Sir Humphry Davy*, 2:131-34. The "bones from the Kirkdale Cave [in Yorkshire]," were fossils subsequently analyzed by the geologist William Buckland.

connection to French savants at the top of the Royal Society. Blagden died in March of that year, and so never witnessed the election to the Presidency of the man he had once described as "puffed much beyond his merits."[52]

Henceforth there would be no center of information in British science with the paramount position once occupied by Joseph Banks's house and library. With his unpopularity in the Royal Society growing, Davy canceled his Saturday gatherings when he moved to another residence in Grosvenor Square in 1826. Other rising venues which Banks had sought—ultimately unsuccessfully—to stifle, like the new Astronomical and Geological societies, and the growing band of commercial journals led by the *Annales of Philosophy* and the *Philosophical Magazine*, further weakened the Royal Society's old monopoly on scientific information and discovery. With the founding of the British Association for the Advancement of Science in 1831, whose meetings in cities like York, Oxford, Edinburgh, and Liverpool were intended to bolster science in the provinces, even London's dominance was not as complete as it had been.[53] British science entered a more diverse, fragmented, and decentralized—if more interconnected—world.

A New Wave of Journals

In May 1814, just over a month after Napoleon's first abdication, the British chemist Thomas Thomson addressed the readers of his new scientific journal, the *Annals of Philosophy*, founded the year before. Thomson apologized for the "very small proportion of foreign papers" reprinted in his pages during the last year. He had gone to "considerable pains" to obtain foreign journals, but with limited success: European communications had been so disrupted by the great conflict

[52] *Blagden Diary*, 24 October 1813. The other main go-between with the Continent during the war years, the Swiss Alexander Marcet, returned to Geneva in 1821 and died the year after. (*DNB*)
[53] Jack Morrell and Arnold Thackray, *Gentlemen of Science: Early Years of the British Association for the Advancement of Science* (Oxford: Oxford University Press, 1981).

that had brought down Napoleon (the War of the Sixth Coalition), that it had been "impossible to obtain any Italian journals; and the French and most of the German journals, hitherto received in this country, are almost a year behind-hand." Furthermore, what little information he did receive had not been particularly groundbreaking, because "the state of the Continent has been such during the last two years that much attention to science could not be expected either in Germany, France, or Italy." But with the fall of Napoleon, surely better times were ahead. "The Editor," he wrote "anticipates a new and a happy era for the progress of science, and the general diffusion of knowledge. Within a few months he expects to receive foreign journals regularly, and to find them filled with much more valuable materials than could be expected during a period of general confusion and calamity."[54]

By November, his predictions were fulfilled: Britain was "regularly supplied with foreign Journals," allowing him "to lay before the British Public the scientific improvements as they are made in the different countries of Europe." His readers would now have something a little closer to a real-time view of scientific progress on the Continent.[55] In January 1815, Thomson was finally able to report that he had been able to obtain enough back-issues of French and German periodicals to bring him (and so his readers) up-to-date on all the work done around Europe during the wartime blockade:

> "After an almost total exclusion from the Continent for about seven years, all the kingdoms of Europe have been suddenly thrown open; and it has been in our power, by importing the different foreign journals, to make ourselves acquainted with the various additions which the sciences have received during this eventful period."[56]

[54] Thomas Thomson, Preface dated 27 May 1814 to *Annals of Philosophy* 3 (1814): iii-iv.
[55] Thomson, Preface dated November 1814 to *Annals of Philosophy* 4 (1814): iii.
[56] Thomson, "Sketch of the latest Improvements in the Physical Sciences," *Annals of Philosophy* 5 (January 1815): 1.

But even if many of the gaps created by the war could now be filled in, staying up-to-date with the Continent remained a problem requiring much ingenuity, time, and money. This was not entirely a bad thing for Thomson; in fact, it was fundamental to his business model. The system earlier editors had pioneered during the war years of doing the hard work of obtaining scientific information, packaging it up, and selling it, could still work well during peace. Thomson could be confident that very few of his readers would have direct access to foreign journals, and so would continue to find the reprints and summaries in the *Annals* worth paying for.

Thomson was a member of Britain's second generation of scientific journal editors.[57] In January 1813 he had noisily announced the launch of his *Annals of Philosophy* with a preface simultaneously glorifying scientific journals in general and dismissing as inadequate the two existing British journals, *Nicholson's Journal* and the *Philosophical Magazine*. The power of journals, Thomson claimed, was that they "scatter every discovery over the whole extent of their circulation," in contrast to exchanges of letters, which "convey the requisite information to a single individual only, and to the small circle of his friends." Prompt announcements of discoveries in journals made the scientific world more efficient, by reducing the chance of "two or more individuals being occupied with the same pursuits, publishing on the same subject, and making the same discoveries without any knowledge of what was done by each other." Echoing statements made by both British and Continental editors who had preceded him, Thomson praised the ferment of information that journals created: "Every discovery is published as soon as made, fruitless labour is spared, and emulation is kept up and increased by the mutual

[57] Sadly no in-depth biographical study of Thomas Thomson exists, but see J. B. Morrell, "The chemist breeders: the research schools of Liebig and Thomas Thomson," *Ambix*, 19 (1972): 1–46; J. B. Morrell, "Thomas Thomson: professor of chemistry and university reformer," *British Journal for the History of Science*, 4 (1968–9): 245–65; also Morrell's article on Thomson in the *DNB*.

discoveries of different individuals, by the jarring of opinions, and the clashing of different interests."[58]

Taking his own advice on clashing interests and jarring opinions, he then went on to claim, in essence, that both *Nicholson's Journal* and Tilloch's *Philosophical Magazine* were failures because they were run with the profit-mongering interests of booksellers and printers in mind, rather than solely for the good of the scientific world. *Nicholson's Journal* had, he said, certainly been well-run to begin with, and its editor "well qualified for the office which he undertook." But now, he claimed, "for some years past, if report says true, it has not been the property of the original editor, but of a bookseller; and in reality edited not by Mr. Nicholson, but by some unknown person employed by the bookseller." As for Tilloch, he was merely "a printer from Glasgow, and publisher of the evening newspaper called the *Star*."

Well aware that scientific journals still had a reputation problem in Britain because of their close association with the disreputable general periodical and newspaper press, Thomson was at pains to show that his new publication would be different: run by a man of science beholden to other men of science alone. Nicholson was not amused by Thomson's efforts to, as he put it, "shew that the English Journals are bad, and, by inference, that his own will be much superior," asserting that he, and not any mysterious "bookseller," was entirely responsible for the contents of *Nicholson's Journal.*[59] Whatever the truth of Thomson's accusations (and we have no evidence to confirm them), they soon proved moot. *Nicholson's Journal* ceased to exist later that year when it was absorbed by Tilloch's *Philosophical Magazine*.

By 1820, other new journals had appeared in Britain, taking advantage of the expanding market for scientific information after the wars. *The Quarterly Journal of Science and the Arts*,

[58] Thomson, Preface, *Annals of Philosophy* 1 (January 1813): 1-4.
[59] *NJ* 34 (Feb 1813): 152.

edited out of the Royal Institution by Davy's successor William Brande, was launched in 1816, a "favorable" time, Brande noted, when "the great Commonwealth of Europe is recovering its ancient social relations," and "those energies of the human mind," previously given over to waging war, would now be "directed to objects connected with the happiness of society." In Scotland, David Brewster founded his *Edinburgh Philosophical Journal* in 1819, relaunching it in 1824 as the *Edinburgh Journal of Science* after a dispute with the publishers over the rights to the title.[60]

Thomson, Brande and Brewster were all men of science on the make, looking to combine their own research (Thomson and Brande in chemistry, Brewster in optics) with journal editing and other literary labor. Brewster edited the *Edinburgh Encyclopaedia*, Thomson compiled a "*History of the Royal Society*"; the three also sought out the few paid positions connected with science in Britain, with Brande finding employment at the RI, and, after 1817, Thomson at Glasgow University.[61] Unlike their predecessors of the last generation of editors—Tilloch (still editor of the *Philosophical Magazine* in 1820) and Nicholson (who died in 1815)—all three were Fellows of the Royal Society (Brande was one of its Secretaries). The respectability of scientific journals in Britain was, slowly, on the rise, though no editor could yet hope for the kind of commanding position that the *Annales de Chimie* enjoyed in France under Gay-Lussac and Arago. As I argued in Chapter 3, this rising profile for journals was in part due to journals making themselves indispensable during the war years as sources of useful information. By the wars' end, even someone as generally skeptical of periodical print as Sir Joseph Banks could not

[60] "Advertisement" dated April 1816, *Journal of Science and the Arts* 1 (1816): ii. For Brewster's publishing dispute and relaunch of his journal see "Advertisement" dated 7 June 1824, *Edinburgh Journal of Science* 1 (1824).
[61] Brewster was less successful in his quest for stable scientific employment, with multiple failed bids for Scottish university positions; he eventually found one at the University of St Andrews in 1838.

avoid being co-opted into the journals' intelligence system—as we saw, he agreed to send issues of Thomson's *Annals* to Paris for Blagden and his French friends during 1814-15.

Nonetheless, the relationship between the Royal Society and the periodical press remained rather uneasy. Thomson used his status as a Fellow of the Society to good effect, printing regular accounts of the Society's "proceedings" in his journal that were presumably read by Fellows and non-Fellows alike. But, unsurprisingly given Joseph Banks's old sensitivity about private or semi-private information finding its way into print, Royal Society rules banned note-taking "with a view to publication before the appearance of the *Transactions*" during the reading of papers. Undeterred, Thomson simply trained himself to commit the entire substance of papers to short-term memory (including any salient numerical details), making sure to get his summary down on paper as soon as he possibly could after the end of a meeting.[62]

It was because of Thomson's editorship of the *Annals* that Ørsted chose him as one of the recipients of his announcement in Britain. By that time, however, Thomson was attending Royal Society meetings rather less than before, having accepted a professorship of chemistry at the University of Glasgow in 1818. Even with Thomson now editing the *Annals* from Glasgow, the journal was still printed in London, and it is likely that Ørsted addressed his letter to Thomson in London when Thomson was in fact four hundred miles away in Scotland. Certainly this or other problems of coordination between London and Glasgow were probably the reason that Ørsted's paper did not appear in Thomson's journal until his October 1820 issue, although Ørsted had mailed his announcement in July.

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[62] Walter Crum, "Sketch of the Life and Labours of Dr. Thomas Thomson, F.R.S.," *Proceedings of the Royal Philosophical Society of Glasgow* 3 (1855), 256. Similar feats of memory had previously been used by British newspapermen when reporting parliamentary debates, as note-taking by reporters was not allowed in the House of Commons until 1783. (The question of whether the printing of parliamentary debates in the newspapers should be permitted remained controversial even after this.) See Arthur Aspinall, "The Reporting and Publishing of the House of Commons' Debates, 1771-1834," in *Essays Presented to Sir Lewis Namier*, ed. Richard Pares and A.J.P. Taylor (London: Macmillan, 1956), 227-57.

In the end, the distance between London and Glasgow proved too detrimental for Thomson to continue—"a residence more than four hundred miles distant from the place of publication is scarcely consistent with the active duties of an Editor," he lamented—and so at the end of 1820 he handed over the editorship of his journal to the London chemist Richard Phillips. While acknowledging that he now had to compete in a fairly crowded field, since there were "numerous similar sources through which information is now conveyed," Phillips advertised "the favourable circumstances in which I am placed for the early acquisition of philosophical intelligence," and also alluded to the "assistance" he had been promised by various unnamed people in his endeavors.[63] One of these people was the young Michael Faraday.

"I regret the haste which made me print the paper": Faraday and the Scandal of Electromagnetic Rotation

In 1820 Faraday was still only the "chemical assistant" at the Royal Institution, and very much in the shadow of his mentor Humphry Davy, but he was steadily building a reputation in London scientific circles.[64] Richard Phillips was an old friend, whom Faraday had first met at meetings of the City Philosophical Society ten years before. During the summer of 1821 they agreed that Faraday would write an article on electromagnetism for the *Annals*: a "Historical Sketch" to try to gather and summarize, in "a digested and scientific account," the numerous papers or reports of work in electromagnetism that had appeared in the year since Ørsted's announcement.[65] Presumably because Faraday was still young (29), had only a modest scientific reputation, and

[63] "Advertisement" dated December 1820, *Annals of Philosophy* 1, New Series (1821): 1-2.
[64] An authoritative condensed survey of Faraday's life and work is Frank James, *Michael Faraday: A Very Short Introduction* (Oxford: Oxford University Press, 2010); see also Geoffrey Cantor, *Michael Faraday, Sandemanian and Scientist: A Study of Science and Religion in the Nineteenth Century* (New York: St. Martin's Press, 1991).
[65] The Sketch was printed in two parts in the September and October issues, with a third part later appearing in February 1822. [Michael Faraday], "Historical Sketch of Electro-magnetism," *Annals of Philosophy* 18 (1821): 195-200, 274-90; *Annals of Philosophy* 19 (1822): 107-121.

would be commenting on the work of important and influential figures, the Sketch would be published anonymously.

As Faraday and Phillips would have known first-hand, it was far from the case that the movement of issues of journals, even between the two centers of London and Paris, was now easy, immediate, and fully reliable. It still took a lot of effort, involved delays and a probability of failure which had real practical consequences for discovery claims—as Davy had found out when he was embarrassed by announcing results already printed by Arago in a journal issue that had not yet reached Britain. But Faraday's *Sketch* was a solution not to this problem of distance but to the problem of organization and synthesis of a large body of locally-accessible texts. This ancient problem was expressed and manifested in new ways after the end of the wars with France led to fuller exchanges of journal literature between different European centers of learning. Thomas Thomson, for example, had complained in January 1815 of being "overwhelmed with matter," faced with "so many volumes" once London had imported all the back-issues of Continental journals that could not be obtained in 1813-14. In this sense the feeling among men of science of "information overload" (in *our* meaning of information, not theirs) was quite compatible with a feeling of information starvation that persisted after the wars' end because exchanges between Britain and the Continent still involved delays and interruptions.

The problem Faraday was addressing was not a problem of communication; it was one of sorting, historical synthesis, and giving an orderly chronological account that would allow the correct attribution of credit. It was the identification of what, at this provisional stage, looked like the most important contributions to scientific progress: which new facts had been discovered, and who had been first to discover them.[66] All through the summer of 1821, Faraday worked at

[66] For the later relationship of this chronological ordering of scientific progress to print and journal publication see Csiszar, "Broken Pieces of Fact." On the range of British work on electromagnetism in the 1820s see David

the task of imposing order on the various papers written on electromagnetism, trying to tame "their great variety, the number of theories advanced in them, their confused dates, and other circumstances."[67] The Royal Institution's library was an excellent place to find issues of the French scientific journals, and Faraday—who lived and worked in the building—must have used these to put his account together. "I referred," he later put it, "with great labour and fatigue, to the different journals in which papers by various philosophers had appeared, and repeated almost all the experiments described."[68] Displaying the same passion for order, routine, and the serialization of time that later led him, over the course of thirty years, to divide his research diary into 16,041 sequentially-numbered paragraphs, Faraday set to work. He synthesized the many available journal articles on electromagnetism into neat, compressed, chronologically-arranged summaries, spinning the different tangled narratives of the discovery's reception in France, Britain, and elsewhere, into a single thread of historical progress, a single linear march of time.

I claimed in Chapter 3 that journals started to shape the idea of scientific progress in their own image, because of the way scientific practice now took place, and was narrated by participants, within a flow of time punctuated by the regular monthly appearance of journal issues. Faraday used this periodicity to order his account, but he tried to be even more precise, if possible, about exact dates of experiments and when they were announced, indulging in some careful detective work. For example: "Ampere and Arago made experiments with spirals or helices, and the results are mentioned in M. Arago's paper, on the Communication of Magnetism to Iron Filings, published in the Annales de Chimie, xv. 93; so that probably the experiments date from Sept. 25, though there is no date to the paper." Faraday also acknowledged that his

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Gooding, "'Magnetic curves' and the magnetic field: experimentation and representation in the history of a theory," in *The Uses of Experiment*, 183-223.

[67] "Historical Sketch," *Annals of Philosophy* 18 (1821): 195.

[68] *Quarterly Journal of Science and the Arts* 15 (1823): 291.

mentor Humphry Davy's experiment with the iron filings had been previously done in France by Arago, "but it was made by the two philosophers independent of each other."[69]

By the time those words were printed, Faraday was ready with his own discovery to add to the cumulative narrative of progress in electromagnetism.[70] Early in September 1821, in the Royal Institution laboratory, he had succeeded in making a small current-carrying wire rotate continuously around a fixed magnet (a similar experiment, in which the magnet rotated around a wire, followed soon after). The experiment seemed to him to give a new picture not only of "magnetic action," but in fact "of magnetism altogether." He quickly wrote up his experiments for the next issue of his Royal Institution colleague William Brande's *Quarterly Journal of Science,* and sent them to the printer. Faraday had already had some of his work in print—including a report on two new chlorine-carbon compounds that had just appeared in the *Philosophical Transactions*—but this was his first really major public claim, the first that might count towards advancing science at the most fundamental level.[71]

It did not go well. Having departed for a short break in the country after sending his paper to the printer, Faraday returned to London to find himself the subject of some disturbing rumors: it was alleged that Faraday had failed to acknowledge information and ideas taken from Davy and Wollaston, who had also been at work on electromagnetism. Faraday complained of being accused of "not acknowledging the information I received in assisting Sir H. Davy in his experiments," and "with dishonorably taking Dr. Wollaston's thoughts and pursuing them without acknowledgement to the results I have brought out." Wollaston had suggested (months

[69] "Historical Sketch," *Annals of Philosophy* 18 (1821), 277, 283.
[70] When the belated third part of Faraday's *Sketch* appeared (again anonymously) in the *Annals* in February 1822, it did indeed contain a long description (apparently inserted by Phillips), of "the discoveries that have been made by Mr. Faraday, of the Royal Institution."
[71] Faraday, "On some new Electro-Magnetical Motions, and on the Theory of Magnetism," *Quarterly Journal of Science and the Arts* 12 (1821), 74. The report on chlorine compounds is "On Two New Compounds of Chlorine and Carbon, and on a New Compound of Iodine, Carbon, and Hydrogen," *Philosophical Transactions* 111 (1821), 47-74.

before) a possible experiment to use a magnet to make a conducting wire revolve on its own axis—rather different from the kind of rotation that Faraday demonstrated—and he and Davy had unsuccessfully tried an experiment to produce the rotation at the Royal Institution. Faraday always maintained that he had not been present, and that although he knew vaguely of Wollaston's interest in the subject during the spring of 1821, he had no idea that Wollaston was still pursing the question. Davy assured Faraday that he had no complaints about Faraday's conduct, and Faraday also wrote to Wollaston asking him for an interview where he could explain himself. "You seem to me to labour under some misapprehension of the strength of my feelings upon the subject," Wollaston rather loftily told him. In fact, the accusations appear to have been circulated by various "friends" of Wollaston's—chiefly, as Faraday only found out much later, Henry Warburton, a Fellow of the Royal Society (and future Radical MP).[72]

The implication was that Faraday had made good use of his position at the Royal Institution, where he must have heard and seen work by Wollaston and Davy on electromagnetism, and used their ideas to push forwards his own experiments. It must have seemed to Faraday that he was being accused, effectively, of spying—a mere "chemical assistant" listening in on the conversation of his elders and then using the information to his own profit. No one ever doubted that Faraday was the first person to produce electromagnetic rotation; this was a question of ideas, of trespassing on others' territory, and of the proper use and acknowledgment of information informally received. As with the upset over Volta's battery twenty-one years ago, involving the Royal Society, the *Morning Chronicle*, and *Nicholson's Journal*, the scandal was wrapped up in the still uncertain relationship of scientific discovery to print. It was particularly murky because it involved accusations that Faraday had violated several

[72] See Faraday to James Stodart, 8 October 1821, *Faraday Corr.* 1:228-30; also Faraday to Wollaston, 30 October 1821, *Faraday Corr.* 1:231-233; Faraday to Warburton, 30 May 1823, *Faraday Corr.* 1:318.

different norms when printing his discovery. He had supposedly failed to follow the norm of acknowledging privately-received information and advice when making a claim in print; he had violated the vaguely-defined rule (still common among London men of science) that someone already known to be working in a new field had a stake of ownership in that territory on which others should not trespass without permission; and, worst of all, he had been accused of taking an idea that Wollaston was patiently developing, rushing ahead with it, and passing the whole thing off as his own.

But Wollaston—notoriously rather secretive about his scientific work—had made no public statement or claim about his ideas on electromagnetic rotation or his continued interest in electromagnetism (he never did publish a word on the matter).[73] That was why Faraday did not mention him in the paper; as he said, "I was afraid to attach his [Wollaston's] name to any thing which Dr. Wollaston had not published or authorised." What he should have done, apparently, was consult with Wollaston first. Faraday did later say he had tried to do this—he had gone to Wollaston's house to talk to him about the paper he was going to publish, but Wollaston had not been in London.[74] Faraday was a young man trying to play by the simple and direct rules that he had used in composing his *Historical Sketch*: science proceeded by dated public claims which were either made in print or, as with announcements at the Royal Society or the French Académie, could be determined from the printed archive of monthly periodical issues. He had found that the scientific world, in practice, did not operate with these rules, but with a set of more shadowy and ill-defined notions of ownership and credit. "I had learned to be cautious," he recalled afterwards, "upon points of right and priority."[75] In Faraday's view, however, the only

[73] Wollaston has now received a definitive biographical treatment; see Melvyn Usselman, *Pure Intelligence: The Life of William Hyde Wollaston* (Chicago: University of Chicago Press, 2015).
[74] *Quarterly Journal of Science and the Arts* 15 (1823): 291.
[75] *PM*, 8 (1836): 538.

error he had made was getting his paper printed in the *Journal of Science and the Arts* so quickly, without showing it to Davy and Wollaston first. "How much I regret the haste," Faraday told a friend, "which made me print the paper in the last No of the Journal."[76] So much for "work, finish, publish."[77]

Much to Faraday's distress, the scandal resurfaced two years later, in 1823—sparked, again, by the appearance of an issue of a journal. This time, the culprit was the *Annals of Philosophy*, and an unfortunate mistake by Faraday's friend Richard Phillips. On 6 March 1823 Humphry Davy had a paper entitled "On a new Phenomenon of Electromagnetism" read at the Royal Society detailing some experiments on how a magnet could create motion in a vessel of electric-current-carrying mercury. Following his predecessor Thomas Thomson's practice, Phillips (a Fellow since 1822) attended Royal Society meetings and then wrote out summaries of the papers from memory for printing in his journal. However, his powers of recollection may not have been as great as Thomson's—or at least they were not on 6 March, when he remembered Davy having closed his paper with a comment that "we owe to the sagacity of Dr. Wollaston, the first suggestion of electromagnetic rotation; and that, had not an experiment on the subject, made by Dr. W. in the laboratory of the Royal Institution, and witnessed by Sir Humphry, failed, merely through an accident which happened to the apparatus, he [Wollaston] would have been the discoverer of that phenomenon."[78]

Faraday had not been present at the Royal Society as he was not yet a Fellow, but Davy soon assured him that the report was erroneous and that he had only meant to say that, on

[76] Faraday to James Stodart, 8 October 1821, *Faraday Corr.*, 1:228-30. Faraday's haste was presumably dictated by the schedule of the *Journal* itself: if he had waited, he would have missed the issue for October.
[77] Whether Faraday ever actually uttered this famous maxim later ascribed to him is unclear. It first appears in an obituary notice of Faraday written by William Crookes in the *Chemical News* in 1867 (supposedly, it was advice Faraday had given to the young Crookes). [William Crookes], "Faraday," *Chemical News* 16 (1867): 111.
[78] *Annals of Philosophy* 5 (April 1823): 304.

Wollaston's suggestion, he and Wollaston had tried an unsuccessful experiment to make a wire rotate around its own axis (not the electromagnetic rotation Faraday had discovered, where the wire revolved around a magnet or vice versa). Faraday and Davy then jointly drew up a statement of correction noting that the report had been "unjust to Mr. Faraday, and does not at all convey the sense of the author [Davy]," which they got Phillips to insert (as if it was a statement from the editor) in the next issue of the *Annals*.[79] This new appearance of the accusations against Faraday was a completely accidental artifact of the strange relationship between Royal Society meetings and the periodical press, but it set off more rumors that Faraday had taken the ideas leading to his discovery from Wollaston. As Faraday later recalled, "all seemed confusion, and generally to my disadvantage."[80]

In May 1823 the rumors had apparently still not gone away, and Faraday realized that more concerted action was needed, because he had just been proposed for election as a Fellow of the Royal Society.[81] Faraday first decided to publicly acknowledge authorship of the previous "Historical Sketch" survey, so as to be able to use it as proof that he had been on his own path to electromagnetic rotation and had not known of Wollaston's views. He did this by sending Phillips a letter claiming authorship of the *Sketch*, which Phillips printed in his next issue of the *Annals*.[82]

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[79] As later recounted by Faraday in *Philosophical Magazine*, 8 (1836): 523-24 (when Faraday felt compelled to defend himself against the faux-scandal after it surfaced *yet again* following the appearance of John Davy's biography of Humphry Davy). Davy's version of record later printed in the *Philosophical Transactions* reads: "we owe to the sagacity of Dr. Wollaston, the first idea of the possibility of the rotations of the electro-magnetic wire round its axis, by the approach of a magnet; and I witnessed, early in 1821, an unsuccessful experiment which he made to produce the effect in the laboratory of the Royal Institution." *Philosophical Transactions* 113 (1823): 158-59. The correction is *Annals of Philosophy* 5 (May 1823): 391.

[80] *PM*, 8 (1836): 524.

[81] Faraday's election was opposed by Davy (as President); his reasons have never been entirely clear, but it may be that Davy wished to avoid an appearance of nepotism. For an alternative narrative of Faraday's difficult election see June Fullmer and Melvyn Usselman, "Faraday's Election to the Royal Society: A Reputation in Jeopardy," *Bulletin for the History of Chemistry* 11 (1991): 17-28.

[82] *Annals of Philosophy* 6 (July 1823): 67.

Then, in a new "Historical Statement respecting Electro-Magnetic Rotation" inserted in the *Journal of Science and the Arts*, Faraday offered the "Historical Sketch" printed in the 1821 *Annals* as "a public testimony of the state of my knowledge on the subject before I began my own experiments." The *Sketch* also demonstrated, Faraday asserted, that claims "that I could never, unprepared, have gained, in the course of eight or ten days, the facts described in my first paper," were false.[83] In fact, he said, the *Sketch* showed he had been well-prepared for his intensive week of discovery because he had been working on electromagnetism all through the summer of 1821, laboriously repeating all the experiments made by others. Behind the scenes, Faraday also went to work to make sure of the good will of Wollaston and his troublesome "friends," writing to the main instigator Henry Warburton asking for an interview so he could personally explain "all the circumstances of the case."[84]

The scandal surrounding Faraday's first major discovery highlights continued tensions in the early nineteenth century between periodical print and the ill-defined codes governing the use and acknowledgment of informally-acquired information. But Faraday's final response to the accusations also demonstrates the rising power of periodical print in Britain as a venue for appearing publicly as a man of science and as a tool for fixing and recording information in linear, serialized time. Faraday needed to combat a private whispering campaign accusing him of having unscrupulously profited from privately-acquired information. To do so, he defended himself in the public pages of the *Journal of Science and the Arts*, and enlisted the public evidence of the journals themselves: his historical sketch in the *Annals*, with its numbered and dated issues, would offer "proofs of my ignorance of Dr. Wollaston's views" at the time he began his course of experiments in September 1821. (He could have exhibited his carefully-kept

[83] "Historical Statement respecting Electro-Magnetic Rotation," *Quarterly Journal of Science and the Arts* 15 (1823): 288-92.
[84] Faraday to Henry Warburton, 30 May 1823, *Faraday Corr.*, 1:381.

experimental notebook from that summer, but he did not do so. Manuscripts could be altered; past issues of journals could not.)[85] It was a powerful public statement of sincerity and integrity; even Henry Warburton pronounced himself more or less satisfied.[86] Wollaston—if he had ever cared much about the affair—clearly bore no ill will now, as he had signed Faraday's nomination certificate for Royal Society Fellowship. Faraday was elected FRS on 8 January 1824. "Sought and paid for," he put it years afterwards.[87] Probably not a pleasant memory.

Conclusion: Science Under Steam?

In November 1822 Hans Christian Ørsted set out from Copenhagen on a European tour.[88] The first stage took him through Germany, with a stay at Berlin followed by Jena and Weimar, where he found that Goethe had been an admirer of his discovery of electromagnetism ever since reading about it in the Genevan *Bibliothèque Universelle* two years before. Next: three weeks in Munich, with visits to Josef Fraunhofer's workshop, and then a long series of rides five hundred miles across the Continent to Paris, using the eighteenth and early-nineteenth centuries' premier mode of land transportation, the horse-drawn coach (with a secure university income, Ørsted could now at last afford to travel well). In Paris, Ørsted was welcomed by Laplace, Ampère, Arago, Gay-Lussac, Thenard, and other luminaries of the Académie des Sciences. As a young man in the same city in 1803, he had tried to convince the savants of the Institut that his good friend Ritter had discovered that the earth possessed galvanic as well as magnetic poles—thus demonstrating a link between electricity and magnetism. The campaign (intended to win Ritter

[85] The relevant section of Faraday's notebook is reproduced in Thomas Martin, ed., *Faraday's Diary: Being the Various Philosophical Notes of Experimental Investigation Made by Michael Faraday, during the Years 1820-1862*, vol. 1 (London: Bell, 1932), 49-57.
[86] Henry Warburton to Faraday, 8 July 1823, *Faraday Corr.*, 1:322-323.
[87] Faraday to Thomas Spring Rice, 23 April 1838, *Faraday Corr*., 2:495-96.
[88] Ørsted's tour is detailed at length in Christensen, *Hans Christian Ørsted*, 360-403.

the Prix du Galvanisme) had ended in embarrassment when Ritter's experiments could not be repeated at Paris.[89] Now, twenty years on and thirteen years after Ritter's death, Ørsted was confident that the *Naturphilosophie* tradition in which he and Ritter worked was vindicated at last. The Laplacians, committed to Coulomb's model of central forces, had been skeptical of any link between electricity and magnetism. Ørsted had shown they were wrong.[90]

At the end of April, the triumphant Ørsted prepared to depart Paris for Britain, a country he had never visited. Arriving at Calais, he embarked upon a recent innovation in cross-Channel travel: a steamship.[91] A smooth crossing to Dover and a speedy coach ride on well-made roads took him up to London, where Ørsted was impressed by the gas lighting and the iron bridges across the Thames, and entertained by the city's men of science, including Davy, Wollaston, Faraday, Thomas Young, John Herschel, and also Charles Babbage, who showed him plans for his Difference Engine, and the young Charles Wheatstone, future designer of Britain's first electric telegraph. After his formal introduction as a foreign member of the Royal Society, ample dinners at the Crown and Anchor Tavern in the Strand, and a visit to William Whewell at Cambridge, Ørsted embarked on another steamship, the appropriately-named "SS James Watt," which carried him from London to Edinburgh in only 48 hours. After Edinburgh he toured in the west of Scotland, and arrived at Glasgow, presumably meeting Thomas Thomson. Another steamship took him to Liverpool and so to Manchester, where Ørsted was able to see firsthand the splendor and squalor at the heart of Britain's Industrial Revolution. From there it was back to London, and so again across the Channel by steam in rough weather, and a long series of bumpy coach rides home. He had been away for nearly a year.

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[89] Christensen, *Hans Christian Ørsted*, 147-155.
[90] On the problems electromagnetism posed for the Laplacian orthodoxy see Robert Fox, "The Rise and Fall of Laplacian Physics," *Historical Studies in the Physical Sciences* 4 (1974): 116-17; Fox calls electromagnetism "perhaps the best illustration of the importance of a discovery in weakening Laplacian physics."
[91] Cross-Channel steamship sailings had begun in June 1821.

This dissertation has largely discussed changes in the world of scientific information in a period before new industrial technologies had any great impact on communications methods and media. The material infrastructure underlying scientific exchanges in the Napoleonic wars was still that of the eighteenth century—and in many respects, not far from that of the late fifteenth century: the hand-press of movable type printing, the letter, the sailing ship, and the horse. As Ørsted's encounters with steamships and other industrial wonders of early-nineteenth-century Britain show, by the 1820s technological innovations were beginning, slowly, to have an impact on the movement of goods and people. But such change would be gradual and uneven. Only by about the time of Ørsted's death in 1851 would it have been possible for someone to make most (though not quite all) of his Continental tour by rail and steamer. In the nineteenth century, science was indeed carried on in an industrializing world of steam and coal and iron, in which ever-faster communications appeared to contemporaries to make the world a smaller and smaller place. But the effects of these transformations on actual scientific practice would be contingent on a multitude of other factors far more important than raw speed.

A similar point about gradual transformation holds for printing technology. Newspapers began adopting the steam-powered mechanized printing press from 1814, when the first was installed for the London *Times* by Friedrich Koenig. Later, the technology would make possible new publication models that would bring science to mass readerships, exemplified by *Chambers's Edinburgh Journal* and the Society for the Diffusion of Useful Knowledge's *Penny Magazine*, both founded in 1832.[92] But specialized scientific journals, which did not target mass audiences and did not require large print runs, would be less affected by the economies of scale the new printing technology made possible. "I do not suppose that any scientific journal ever

[92] See Aileen Fyfe, *Steam-Powered Knowledge: William Chambers and the Business of Publishing, 1820-1860* (Chicago: University of Chicago Press, 2012).

reached the sale of a thousand in this country," commented the publisher and editor of the *Philosophical Magazine* Richard Taylor in 1838, "if they can keep up to five hundred, it is almost as much as they can do."[93] Low circulation of journals could in part be blamed on high costs (passed on as high subscription prices), some of which, like postal rates, had nothing to do with printing technology. But ultimately, despite increased *popular* interest in science, the specialized audience for the technical details of scientific results remained rather limited, making hand-printing rather than steam more economical. Science stood for progress like nothing else in the nineteenth century, but—during the first half of that century at least—many of its most important discoveries would be first put on paper using a process that would have looked familiar enough to Johannes Gutenberg. Steam printing technology and the mass print revolution that it enabled transformed the relationship between scientific specialists and wider audiences, but it had less dramatic effects on those specialists' own communications.

As I have emphasized over the preceding pages, disruptions and gaps in communication played an active role in structuring science during the Napoleonic wars; they continued to do so even after the wars were over. Monthly journals, in particular, retained their wartime value as useful tools of distance but even with the coming of peace the connections they forged could still prove unreliable and sporadic. And journals remained much more cumbersome than letters when it came to shipping across international borders. In November 1820, four months after sending his announcement of his discovery to Thomas Thomson, Ørsted was still having trouble getting his hands on any recent issues of Thomson's *Annals*; he had, he complained to a friend, only seen issues up until May 1819.[94] Ørsted therefore remained in the dark for months about the

[93] "Minutes of Evidence Taken Before the Select Committee on Postage," in *Sessional Papers Printed by Order of the House of Lords* (Session 1837-38), vol. 33 (London, 1838), 322.
[94] Ørsted to Johan Georg Forchhammer, 21 Nov 1820, *Breve fra og til Hans Christian Ørsted* (Copenhagen: Th. Linds, 1870), 2:17.

printing and reception of his discovery abroad. And to really get a sense of how his discovery had been received in Britain and France, he had to go to those places himself.

The problem was not always one of simple distance and cost, but of setting up and maintaining adequate supply routes. German journal editors apparently felt their publications were insufficiently noticed by the British and the French, who (it seemed to them) wanted only to read each other's work while ignoring most of what issued from Central Europe. "How can sincere friends of science and of the truth expressly ignore all that is being done by a great nation," complained the journal editor Ludwig Wilhelm Gilbert to Faraday in 1821, lamenting the lack of attention given by British publications to papers in his *Annalen der Physik*.[95] Booksellers figured heavily in these exchanges of print, and were often blamed when they broke down. "It is a very singular fact," Thomas Thomson told his journal's readers after the coming of peace in 1814, "which we do not pretend to explain, though the French booksellers in London probably can, that the supply of Paris Journals is more irregular and dilatory than of any other, though the distance of that capital from our own is so small."[96] With the Germans complaining of being ignored in Britain in favor of the French, and the British bemoaning stoppages of French information (but seemingly less worried about connections to Germany), the map of scientific communication certainly looked rather different depending on the who, the where, and the when of the observer. With everyone necessarily viewing that map from individual and partial perspectives, no neutral assessment of the situation in international scientific exchanges was possible uncolored by perceptions and expectations of how information ought to travel.

In 1838, at the dawn of the railway age, men of science were still complaining about interruptions of communications between Britain and France (to say nothing about other more

[95] Ludwig Wilhelm Gilbert to Faraday, 5 June 1821, *Faraday Corr.*, 1:211-213.
[96] Thomson, Preface dated November 1814 to *Annals of Philosophy* 4 (1814), iii.

distant parts of Europe). In that year, the Parliament of the United Kingdom convened a Select Committee on "the present rates and mode of charging postage"—an initiative which ultimately resulted in the establishment of the Uniform Penny Post throughout Great Britain and Ireland. One of the witnesses called was the publisher Richard Taylor, who had taken over as editor of the *Philosophical Magazine* from Alexander Tilloch and who was now the dominant personality in scientific journal publishing in Britain, his journal having successively absorbed *Nicholson's Journal*, Thomson's *Annals*, and Brewster's *Edinburgh Journal of Science*.[97] Reprinting remained a key tool in the international journal system in 1838, and Taylor reported his exchanges of journals with foreign editors, noting that "the papers which appear in the English scientific journals, are, as soon as those journals reach the Continent, translated and published in the journals of France, Italy, and Germany, and also in America." But Taylor painted a picture of financial hardship due to high rates of postage—"it is almost an impracticable thing to keep a scientific journal alive in this country"—and complained that he had received word from the French Academy that "for the last three months of the last year, no English scientific journals were received by the French Institute, nor at Geneva; of course, any discoveries made in England remained unknown abroad." He also revealed that editors and other men of science in the 1830s frequently used diplomatic channels (through the Foreign Office) to send journals abroad and to obtain foreign publications.[98] A later witness, the scientific writer Dionysius Lardner, revealed that many international scientific letters also went through diplomatic routes.[99] What had worked for Charles Blagden during the wartime Continental Blockade could work during peace as well.

[97] On Taylor see W.H. Brock and A.J. Meadows, *The Lamp of Learning: Taylor & Francis and the Development of Science Publishing*, (London: Taylor and Francis, 1998).
[98] "Minutes of Evidence Taken Before the Select Committee on Postage," 315, 325. Booksellers provided another route for organizing these exchanges: see Arthur-Auguste De La Rive to Faraday, 21 October 1830, Faraday Corr. 1:531-535.
[99] "Minutes of Evidence Taken Before the Select Committee on Postage," 385-7.

Taylor's overall message was that cheaper postage costs would mean that "new discoveries would be communicated with much more facility and rapidity than they now are"—both within Britain and internationally. The Committee agreed.

As historians and sociologists of science well know, the problem of successfully replicating new scientific experiments and other natural phenomena at different sites divided by distance is a perennial trouble.[100] In almost all cases, however, this is a *second order* problem—the first order problem being to arrange for the transport of texts (and sometimes people) to provide other researchers with an adequate description of the new work so that attempts at replication can begin. In most instances, physically moving the experiments themselves is not a viable option. But not always. One person who managed to circumvent the second-order issue, and so make his work more mobile, was Michael Faraday. Even while dealing with the imbroglio generated at home in London by the publication of his rotation experiments in October 1821, Faraday still found time to work on communicating electromagnetic rotation to colleagues across the Channel. He managed to shrink his electromagnetic rotation experiment to make it pocket-sized, with the wire, the magnet, and the mercury all hermetically sealed inside a glass tube. All the recipient had to do was connect a battery and the wire would rotate around the magnet. He sent examples to several Continental savants, including Jean Nicolas Pierre Hachette in Paris and Charles-Gaspard de la Rive in Geneva. Nonetheless, though small, the apparatus was not a letter and so could not be mailed. Faraday instead had to entrust the apparatus to convenient travelers; de la Rive's went with an unnamed "Italian gentleman who is on his return home." Once on the Continent, the apparatus was distributed further (now dematerialized, in print) with Hachette arranging for a description and engraving in the *Annales de Chimie* in the

[100] The classic sociological study is Harry Collins, *Changing Order: Replication and Induction in Scientific Practice* (London: Sage, 1985).

same issue that contained the translation of Faraday's rotation paper. Finally, Faraday also offered William Wollaston an example of his apparatus "as a mark of strong and sincere respect" to smooth over any offense he might have given by his too-hasty publication.[101]

Faraday's portable rotation experiment was a rare exception. Ørsted's solution to the problem of distance had been entirely paper-based: mass-mailing through the well-developed (but expensive) European postal system. This had worked well, but, as we have seen, it worked in a partial and unpredictable fashion, over which Ørsted had no control. This chapter has only looked at the immediate consequences of Ørsted's discovery, up to Faraday's electromagnetic rotation in 1821, using those events to map out the shape of scientific information exchanges after the coming of European peace in 1815. In the years to come, many more investigators would join the field across Britain, France, Germany, and all over the scientific world, seeking to develop the theoretical and experimental aspects of electromagnetism further, and so probe deeper into Nature's secrets.

To later nineteenth-century commentators, however, the most important consequence of Ørsted's discovery was not any new understanding of the natural world it enabled—important though that might be—but a technology: the electric telegraph. Yet, even though the telegraph was a direct application of the galvanic science of the early nineteenth century, and came to exemplify science's power to remake the world and revolutionize human communications, it ironically saw rather little use in the communications of scientists themselves. With its short and expensive messages, the telegraph transformed commercial, political, and diplomatic news—but not scientific information, which moved largely in other media. Right down into the late

[101] Faraday to de la Rive, 16 November 1821, *Faraday Corr.*, 1:236-7; Hachette to Faraday, 30 October 1821, *Faraday Corr.*, 1:234. Faraday to Wollaston, 30 October 1821, *Faraday Corr.*, 1:231-2; *AC* 18 (November 1821): 330. Faraday was probably inspired by a small electromagnetic apparatus that de la Rive had sent to him previously: Faraday to de la Rive, 12 September 1821, *Faraday Corr.*, 1:221-3.

twentieth century, until the rise of email, the Internet, and the Web, written scientific information still traveled largely as ink on paper, either privately in letters or publicly in print.[102] But, as we have seen, long before it was discovered that galvanic electricity itself could be a tool for pushing back against distance, the combined experimental pursuit of galvanism across Europe helped to transform and shape communication in science in ways often subtle but with long-term consequences. That has been our story here. And so it makes sense that, on the threshold of the Victorian age, in which electricity would come to be *identified* above all else with rapid communication, we should bring that story to a close.

[102] On the effects of the arrival of new electronic forms of textual communication on the reception of a major discovery claim in the late twentieth century see Bruce Lewenstein, "From Fax to Facts: Communication in the Cold Fusion Saga," *Social Studies of Science* 25 (1995), 403-36.

Conclusion: The Information Question

This dissertation has aimed to be two things. First: a history of information, communication, time and distance in science during and after the Napoleonic wars. And second: a history of a particular scientific field and the people who worked in it, followed closely over several decades. I have sought to show the many interrelationships between these two histories, the history of scientific information and the history of galvanism around 1800. On the one hand, we have seen how different media and methods of scientific communication shaped galvanism—in terms of how the dynamics of texts on the move informed experimental practice in the laboratory and the conduct and content of intellectual debates, and in terms of how the workings of media like newspapers contributed to making the public image of galvanism and its practitioners. But on the other hand we have witnessed how the scientific content and practical problems of galvanism affected the development of scientific communications at a crucial moment of hybridity, fluidity, and wartime instability. People doing galvanism bolstered Europe's nascent system of scientific journals, articulated and clarified their norms and codes of operation. Galvanism gave a new visibility and new direction to how science was treated in the newspapers, and bolstered the formation of new public roles for men of science—exemplified by Humphry Davy's career at the Royal Institution. And galvanism endowed wartime exchanges of letters between London and Paris with the urgency and intensity of a race for experimental discovery—it provided motive and content for much of the cross-Channel correspondence carried on by Charles Blagden and his friends in Paris. To generalize: information media are not fixed and stable channels for the communication of new science, but flexible tools always under development. And neither does the intellectual development of a science exist in isolation from the nitty-gritty details of the

exchanges of information media that link its practitioners together. Both make each other, all the time, all the way down.

By showing that process at work, I have sought to deliver on my promise to pursue information as a *historical object*. Taking this view, information in the world around 1800 was not data, the information of research libraries and server farms; it was not simply experienced as an ever-growing archive of stored knowledge. Certainly people doing science had the sensation of there being too much to know—a problem surely almost as old as written scholarship itself—but this had little to do with the more urgent *problem of information*, which was about timely access to intelligence about what distant people had said or done. In this world, information was mobile, always in the *process* of moving from person to person and place to place. Information was something for which you hungered, always.

But nor were the inhabitants of the Napoleonic world dealing with the dematerialized information of modern-day fiber-optic cables (or Victorian telegraphs): instantaneously transmitted, invisible, disembodied signals. Over and over again, we have seen how the process of doing science and linking its community of practitioners together hinged on the ways information was material, embedded in physical objects—whether newspapers, journals, letters, or people—things that required time and effort to move across distance. Delays and disconnection in that movement of things has been as much as part of my story as communicative success. As experienced managers of information like Charles Blagden or Humphry Davy knew intuitively, delay and distance could be active ingredients in the scientific enterprise—something to be exploited in the pursuit of discovery, galvanism's greatest prize.

There is a historiographical point lurking here. In framing our narratives, we historians hold time and space in the palm of the hand. In our historical stories—now ever more

transnational and even global in scope—we are able to fast-forward, pause or rewind time, able to move effortlessly back and forth between cities and continents, carrying our readers along like Goethe's Mephistopheles whisking Faust from the Holy Roman Empire to ancient Sparta and back again. We all know how much physical distance, geopolitical borders, and cultural divisions posed problems for the inhabitants of the past—distance, in Fernand Braudel's phrase, is "the first enemy."[1] Yet weaving distance into historical writing is not so easy. How to dramatize the drawn-out anxiety of waiting six weeks for a reply to a crucial letter, or the sheer boredom and disconnection of a long journey by horse and sail? Such experiences were integral to how people in the Napoleonic world dealt with space and time and processed information—in science and in other areas of life. But the history of science is usually about making the connections, about stitching people and texts together. It's hard to show the gaps and delays.

This dissertation has been an experiment trying to do just that: writing a history of science that takes time, distance and disconnection in intellectual exchanges seriously as objects of historical inquiry, rather than as awkward bumps to be smoothed over in the finished narrative. To that end, I have made a point of relentlessly asking a particular empirical question of my sources when tracking galvanism across the decades around 1800. That is the *information question*: when, and through what pathways, did particular people get informed of new things in galvanism, and what did they do with the information once they had it? The Olympian power historians have of flying back and forth across the spatial and temporal landscape of the past could make it tempting to dismiss the phenomenon of incomplete information in intellectual exchanges as something accidental—a pathological state to be naturally overcome as information diffuses across the world. But if we embrace the simple realization that incomplete information

[1] Fernand Braudel, *The Mediterranean and the Mediterranean World in the Age of Philip II*, trans. Siân Reynolds (Berkeley: University of California Press, 1995 [1949, 1966]), 1:355.

was (and is) the normal experience of people on the ground, then we can use that same Olympian power to track information day by day, month by month, across time and space, to recover the different information cultures of the past, and thus historicize the phenomenon of distance and incomplete information itself.

I certainly do not mean to suggest that historians should always break down scientific exchanges using the kind of painstaking micro-level analyses of information flows I have performed in this dissertation. If historicizing information is not the focus, often these details will add little to the argument. But I do want to encourage critical reflection about what we smooth over and elide when we make our often necessary narrative simplifications about how people got to know what they knew about one other. One motive of this project has been to uncover what sort of complex and sometimes alien communicative processes can lie behind seemingly straightforward statements like "Davy repeated Gay-Lussac and Thenard's experiment," or "Ritter received information about the battery," or "Volta's discovery was published." We have been able to look over Charles Blagden's shoulder as he penned letters to France unsure of whether they would ever be received, or over Alexander Tilloch's as he gathered reports of experiments, made by people far away whom he had never met, and stitched the texts together into a new issue of his journal. Unlike them, we did not need to wait weeks or months to learn what was happening in Paris, Berlin, or Milan; we could go straight there. Possessed of better large-scale knowledge of how galvanism travelled than Volta, Davy, Tilloch, Ritter, Berthollet, Ørsted, and even Charles Blagden had at the time, we can reconstruct the rich and often fractured information landscape in which they lived, worked, and interacted with one another.

Historical processes operating at both the macro and the micro scales combined to redefine that landscape of scientific information between the 1790s and the 1820s. At the macro

scale, I have charted how the rhythms of scientific communications at the international level were enmeshed within the great geopolitical struggles of the Napoleonic wars—above all the long war between Britain and France. Wartime closures of borders and postal routes, and, from 1806, Napoleon's Continental System, certainly slowed or blocked many scientific information exchanges. But they did not always weigh strongest when determining the overall pattern of exchanges in galvanism. We saw, for example, that communications between Britain and France (or, more precisely, between London and Paris) were often better and more productive than either of those countries' connections to German-speaking Europe, even when France and Britain were at war and both had good postal links with Germany. And, with the final defeat of Napoleon in 1815, the scattered European scientific community did not naturally spring back into a connected whole. Complaints about poor international communications remained common, and forging and keeping long-distance connections took work during peacetime as well.

On the micro scale, we have witnessed men of science pursuing their own ends amidst all this geopolitical turmoil, often striving to keep open links with friends, colleagues or even rivals far away—perhaps less from motives of selfless internationalism than from a desire for information that could prove useful. It was, I have claimed, precisely the *difficulty* of getting science from point A to point B—the experience of distance and information starvation—that prompted men of science to become more self-conscious and more inventive managers of communications during the disruptive years of 1792-1815. The conditions of war, combined with the unprecedented pace of developments in galvanism and chemistry across the same period, generated a ferment of creative experimentation in techniques of communication which lived on after the wars' end. This of course was manifested in different ways and for different reasons:

Blagden and Berthollet's high-level system of correspondence through American diplomats looks very different, for example, from the work of commercially-minded journal editors like Alexander Tilloch or Thomas Thomson who sought to bring Continental science to a large readership in Britain. And implicit in my story is an assertion that many of the changes in the landscape of scientific information around 1800 came about not because of planned and controlled decisions by the scientific elite, but as long-term unintended consequences of day-to-day problem-solving with information, in activities like making, repeating, and communicating experiments. I have tried to bring out the agency of actors outside the scientific world in these changes as well: newspapers especially, but also public lecture audiences, magazine editors, and European states. In the final analysis, finding ultimate *reasons* behind the rise of scientific information around 1800 is surely a problem of irreducible complexity, but certainly we can identify some of the main constraints and actors in the process.

And we can point to consequences. The increasing interaction of the daily newspaper press with science (described at length in Chapter 2) is one clear result of these changes. We still lack a well-developed historical picture of the subsequent story of that interaction—even for Victorian Britain, on which much of the best work on the relationships between science, public audiences, and general readerships has been done. And while the rise of scientific journals during the nineteenth century has now received attention from historians (see Chapter 3), I have sought to answer a new question which that recent scholarship creates: what precisely were scientific journals doing *in practice* around 1800, before they become privileged gatekeepers to scientific knowledge? My answer was that, building on a century and a half of previous uses of periodical print in science, journals emerged as a coherent format and interconnected information system around 1800 because they proved useful as tools of information and communication for the fast-

moving sciences of galvanism and chemistry during the difficult circumstances of the Napoleonic wars. In galvanism and chemistry, journals became essential platforms sustaining intricate practice-oriented conversations within and across national borders. Short reports in their pages, concerned above all with the pursuit and announcement of "discovery," became required reading for specialists, destabilizing established mechanisms for announcing new claims to knowledge about nature.

Finally, I have sought to track continuity in the forms of information exchange alongside the arrival of new forms. Older methods of communication did not go away, but instead merged into the widening stream of information, interacting and combining with newer kinds of media. Charles Blagden's letter-writing activity may look reminiscent of the Enlightened Republic of Letters before the French revolution—and indeed that was where Blagden's view of science was formed—but all his letter-writing was inflected by his interactions with print-based sources of news, like monthly journals and the *Moniteur* newspaper. And, while the Napoleonic wars did indeed mean an end to the old broad-based Republic of Letters that Blagden had known, the scientific letter-writing he and his friends were engaged in stands not at the end, but in the midst, of a long history of private communications between scholars and scientists, preceded by figures like Erasmus to Henry Oldenburg, and followed by great Victorian letter-writers like Charles Darwin, right up to the email exchanges that weave together the present-day social fabric of academic life.

There is surely much to say about the history of scientific exchanges in the early nineteenth century that a study of galvanism cannot capture. With its broad and often non-elite cohort of participants all caught up in the race for discovery, galvanism represents one end of a spectrum in the operation of scientific communications around 1800. For precisely that reason, I

have argued, galvanism drove forward the rise of time-critical information in science in newspapers, journals, letters, and elsewhere. But at the other end of the spectrum we can find extended and slow-maturing projects of enquiry published in landmark books: Laplace's *Mécanique Céleste* (1799–1825) or Dalton's *System of Chemical Philosophy* (1808) are two major examples from this period. Weighty synthetic treatises that staked major claims to knowledge about nature still had very influential roles to play in science around 1800, and they continued to do so long afterwards, with books like Darwin's *On the Origin of Species* (1859) and Maxwell's *Treatise on Electricity and Magnetism* (1873). Around 1800 there was a sharp epistemic contrast between such synthetic works and the much shorter and more fragmentary texts that propelled galvanism forwards in journals (and newspapers): as we have seen, journals were not trusted repositories of knowledge, but bearers of up-to-date information. And although journals did eventually attain the trusted status of books (in the process, all but eclipsing them as venues for the publication of new science), the opposition between timely information and verified knowledge has proved durable. Modern debates about "open science," electronic preprint circulation (arXiv.org), and peer review still revolve around tensions between, on the one hand, the desire for the rapid and broad distribution of information on the latest science, and on the other, the demand that science's products be given to the world in a polished form which comes with at least a provisional guarantee of truth. Much work was required throughout the nineteenth century to forge an eventual—and, perhaps, temporary—synthesis between these two imperatives of communication.

Above all, I have sought to draw attention to the exceptional fluidity of scientific information media around 1800. Fluidity in terms of the boundaries between forms and genres, such as the closeness of scientific journals to the newspapers. Fluidity in terms of the ability of

different kinds of media to feed off one another—for example, in the ways practitioners of science mixed and matched letters and print when working with information. And fluidity on the level of the movement of text itself. Time after time, we have seen text reprinted, translated, excerpted—copied within and between different print forms, from journals to journals, newspapers to newspapers, newspapers to journals, letters to newspapers—and many other combinations. When we journey to the early nineteenth century, we would do well to leave behind as many of our modern assumptions about scientific publication and the workings of scientific texts as we can: our notions of originality, audience, distribution, "popular" versus "expert," authorship, trust—and the very meaning and uses of print itself in the scientific process. My aim here has been to reconstruct what it meant to encounter and deal with scientific information, in print or in manuscript, in this (to us) rather alien media world of the beginning of the nineteenth century.

Nonetheless, the fluidity of communication in the Napoleonic world in fact finds an echo in present concerns. The Internet has led some observers to conclude that we are entering a new era in scientific communications, in which a mixed system of preprint circulation, blogs, wikis, and open data will complement, and perhaps even supplant, the established medium of the peer-reviewed journal. Whatever the future may hold, careful historical study of the processes of intellectual exchanges in the past can surely help to put such issues into long-term perspective. But in such work we need not only large-scale maps of correspondence or periodical circulation, but also close studies of how communication in all sorts of media actually functioned as a tool to achieve practical scientific goals that historical actors had in mind, whether the taxonomic classification of a new species, or the synchronization of metrical standards, or the replication of laboratory experiments. It is by following information on the micro-scale that we can calibrate

our macro-level pictures of communications in the past, just as the macro picture gives a frame to each small-scale communicative act. By telling the story of galvanism and situating it within the global geopolitics of the Continental System, I have sought to illustrate how both the micro and the macro shaped the world of science in the Napoleonic age.

Note on Citations

France used the Republican Calendar from 24 October 1793 (3 Brumaire an 2) until its abolition by Napoleon with effect from 1 January 1806 (which would have been 11 Nivôse an 14). In citations to primary sources I supply the Republican Calendar date when the original source itself bears a Republican date; in such cases the Gregorian date is also given.

The format of citations to periodicals recommended by modern style guides derives primarily from the model of an article in a modern academic journal: unambiguously-titled, unambiguously-authored, and original to the periodical in which it appears. As this dissertation shows, that model is a poor fit for texts in the reprinting-based "periodical commons" around 1800. In newspapers, items of news generally bore no title—the fundamental unit being not the article but the "paragraph"—and cannot be ascribed to any particular writer. Similarly, in magazines or scientific journals items of intelligence would frequently appear only under a generic heading like "scientific news." Furthermore, a large number of my citations are to reprinted material (the prior source is always specified where known), which was frequently edited, cut, or spliced with other texts, and may appear under headings quite different from those in the original publication. For these reasons, and to reduce the volume of the notes, I usually dispense with giving an article title or author's name in citations to newspapers and periodical primary sources unless it seems particularly informative or necessary (usually the name of the writer, if known, will be clear from the narrative). Similarly, I have not listed primary-source citations to periodicals and newspapers in the Bibliography; instead, I have provided a full list of all such publications consulted.

Since I am often interested in tracking how *time* mattered in the movement and consumption of printed information, where relevant I try to provide the month or day for the specific issue of a periodical in which the information appeared. This is often partially effaced by the process of binding individual monthly issues into a full volume, but can in most cases be recovered by paging back through the volume until the first page of the month's issue is reached. With one caveat: an issue printed (and cited) under the month of, say, "July 1805" might have appeared at different times depending on the periodical in question. For example, with *Nicholson's Journal* that issue would ordinarily have been available at the beginning of the month, i.e. 1 July 1805, but for the *Philosophical Magazine* it would have been available at the *end* of the month or the beginning of the next, i.e. about 31 July/1 August. When this matters for the discussion I specify when the issue actually appeared. In some cases, such as for German monthly periodicals, it is not possible to determine precise dates of issues from the text of the periodicals themselves, though where relevant the date can often be estimated using other dated sources, such as reviews or announcements in newspapers.

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