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Nominally Rational: Systematic Nomenclature and the Structure of Organic Chemistry, 1889-1940

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NOMINALLY RATIONAL:
SYSTEMATIC NOMENCLATURE AND THE STRUCTURE OF ORGANIC CHEMISTRY, 1889-1940

Evan Hepler-Smith

A DISSERTATION

PRESENTED TO THE FACULTY

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Abstract

This dissertation addresses the emergence of an approach to naming chemical compounds that became a pivotal instrument for the representation of chemicals, the development of the information technologies of chemistry, and the national and international organization of chemists. At the turn of the twentieth century, tens of thousands of synthetic compounds were transforming chemistry and commerce. European and American chemists sought to impose order on their field and its objects of inquiry by making rules for naming these chemicals—or rather, rules for converting chemical diagrams into names.

Chapters 1 and 2 recount the foundations of this approach to chemical nomenclature and its first articulation at the 1892 Congress for the Reform of Nomenclature in Geneva. The Alsatian chemist Charles Friedel convened this congress to advance the interests of the French within European chemistry and of Friedel's faction within Parisian science; congress delegates designed rules of nomenclature specifically and exclusively for use in chemical indexes. But names would not stay where they were put. Chapters 3 and 4 reconstruct how chemists throughout Europe and America adopted and adapted Geneva names in laboratories, classrooms, and patent offices, while index-makers avoided them. Instead, chemical editors created their own nomenclature schemes not only *for* but *through* the work of compilation. As Chapter 5 shows, these chemical reference works became a vital part of the political and intellectual landscape of chemistry. Indeed, during World War I, Entente chemists singled out the authoritative German reference literature as the Teutonic trick behind

that nation's dominance of chemical science and industry. Chapter 6 addresses how, after the war's end, reformers who sought to situate science within a new international order took up nomenclature less as a problem of organizing chemicals than as an opportunity to organize chemists. They resolved fundamental conflicts over both *how* chemicals should be named and *who* should get to decide by vesting authority in a cadre of technical experts. Nomenclature debates receded to irrelevance for the vast majority of chemists, even as systematic names gave form to the epistemic shifts and transformations of the material world effected by twentieth-century chemistry.

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I think of academic life in theatrical terms – teaching is performance, writing is rehearsal, job searches are casting, and so on. Writing acknowledgements feels a bit like a curtain call: abrupt, earnest, grateful, kind of strange (as characters from act 1 and act 3 stand cheek to jowl), a little awkward. Best just to go with it.

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Three other committee members have contributed valuable perspectives to this project. Angela Creager has been an unfailingly thoughtful and supportive teacher, reader, and advocate from the beginning of my time at Princeton. Both her advice and her own scholarship have shaped this dissertation and, especially, the directions in which I hope to take it. Phil Nord has pushed me to situate chemical science and industry in the political and social transformations of Europe in the late nineteenth and early twentieth centuries. Carsten Reinhardt has helped me to think of this dissertation as part of a much broader story of the development of tools and methods in chemical science and industry. Though not members of my committee, Robert Fox and Roald Hoffmann have generously read and commented on substantial portions of this dissertation and sundry other writings and notions. It has been a great pleasure to exchange thoughts with both of them.

I conceived of this dissertation as a graduate student at Princeton, wrote a substantial portion of it at the Chemical Heritage Foundation, and finished it at the Max Planck Institute for the History of Science. I am grateful to all three institutions for their support, as well as to the Society for the History of Alchemy and Chemistry and the Chemical Heritage Foundation for additional funding in support of research travel. A portion of the material addressed in Chapters 1 and 2 appeared in *Ambix* 61, no. 1 (2015), 1–28 as Evan Hepler-Smith, "'Just as the Structural Formula Does': Names, Diagrams, and the Structure of Organic Chemistry at the 1892 Geneva Nomenclature Congress." I thank the editors for permission to use this material here.

In Princeton's Department of History and Program in History of Science, I had the great good fortune to spend six years with a wonderful group of faculty and fellow graduate students who taught me how to read, talk, write, and think like a historian. Graham Burnett and Tony Grafton exemplified how fruitful it can be to push the

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In closing, I would like to offer three sets of special thanks. The first is to Will Deringer, who quite literally got me into this mess. Without Willy's encouragement, I would not have found my way to the history of science nor, perhaps, to academia, the only sort of job I have so far managed to hold down for more than a year. He has been a colleague, mentor, interlocutor, collaborator, cheerful recipient of unsolicited feedback, chef, once and future roommate, butler, and, most of all, a dear friend. I am deeply in his debt, intellectual and otherwise, and I will strive to keep him in mine.

Rebecca O'Brien became my closest companion just about the same time I decided I wanted to become a historian of science. In rapid succession, she became a journalist (and a megacommuter), I began this dissertation, and we got married. For

tolerating me, reassuring me, making me laugh, making me think, getting me out of the house, deleting my modifiers, humoring my trains of thought, all with love – and for letting me do the same for her – I am grateful beyond words.

When organic chemists wish to refer to a compound consisting of two distinct chemical subunits without giving one moiety priority over the other, they simply combine the names of the two constituent compounds – a method of naming called "conjunctive nomenclature." Forty-odd years ago, Michael Smith sat down behind Beth Hepler in – of all places – an organic chemistry class. Shortly thereafter, they both decided to start calling themselves "Hepler-Smith," a surname that, while not entirely rational and certainly unsystematic, does express their characteristic properties: deliberateness and whimsy, empathy and imagination, humility and curiosity, deep and abiding thoughtfulness. If I have managed to display some measure of any of these attributes, it is their doing. This dissertation is for them.

June 23, 2016

Note on Sources and Abbreviations

During the 1920s and 1930s, the International Union of Pure and Applied Chemistry published proceedings of its annual (as of 1928, biannual) conferences under the title *Comptes rendus de la première [deuxième, troisième, etc.] conference international de la chimie* (Paris: Jean Gérard). I have abbreviated these citations to "IUPAC Comptes Rendus [conference number] ([year])."

From the 1900s through the 1920s, the American Chemical Society published its monthly *Proceedings* as an annex to the *Journal of the American Chemical Society*, paginated separately from the *Journal* and continuously for each year. I have cited these as *Proceedings of the American Chemical Society* ([year]).

In citations to archival materials, I use the following abbreviations:

Agenda	Agenda books of Amé Pictet, Z 306, Musée d'histoire des sciences, Geneva.
Austin Patterson IEB file	International Education Board, RG IEB 226, Series 1, Box 13, Folder 197: Austin Patterson 1924-1926, Rockefeller Archive Center, Sleepy Hollow, NY.
BGE	Bibliothèque de Genève, Geneva.
Emil Fischer papers	BANC MSS 71/95 z, The Bancroft Library, University of California, Berkeley, Berkeley, CA.
IUPAC papers	International Union of Pure and Applied Chemistry (IUPAC) Archive, Othmer Library of Chemical History, Chemical Heritage Foundation, Philadelphia, PA.
Paul Ehrlich papers	Rockefeller University Archives, RG 650 Eh89, Rockefeller Archive Center, Sleepy Hollow, NY.
RSC	Royal Society of Chemistry Library, Burlington House, London.
W. A. Noyes papers	William A. Noyes Papers, 15/5/21, University of Illinois Archives, Urbana, IL.

Introduction

The Things of Order

What kind of a thing is *vitamin C*? Not the biological factor that plays a crucial role in plant and animal metabolism, nor the tablets found on drugstore shelves. Not the pure chemical substance, a crystalline white powder, nor the particular arrangement of six carbon, eight hydrogen, and six oxygen atoms taken to constitute it. Rather, what kind of a thing is this name, "vitamin C"?

"Vitamin," a term for a class of essential nutrients, was derived from the conjunction of "vital" and "amine," the name of a chemical family to which one of these substances belonged. As it turned out, many others were not amines at all, including the substance that was third in line when biochemists started assigning letters to these factors; thus, "C." Haphazard etymology notwithstanding, "vitamin C" does a reasonably good job of referring to powder, tablet, and molecule alike, and of tying these objects together. If "vitamin C" seems unsuitable, however, an alternative name is "ascorbic acid," as this substance was christened in reference to its "anti-scorbutic" (scurvy-preventing) function. To get a bit more specific, add a prefix, "L-ascorbic acid," to refer to one of the three-dimensional variants of this compound – the version responsible for most of its functions in biological processes – or change the suffix, "ascorbate," to indicate the conjugate base of the acid, the form in which the compound does its biochemical work. For specificity of a different sort, there is "Redoxon," a brand name under which vitamin C supplements have been sold since 1934. Each of these terms displays qualities normally taken to make a good name. They are short, memorable, legible, pronounceable, and distinguishable from other names.

And then there is the systematic name for this chemical compound: "3,4-dihydroxy-5-(1,2-dihydroxyethyl)furan-2(5*H*)-one." This name is a different sort of thing entirely. Long, cumbersome, forbidding to the eye and the ear, it is an affront to the discursive order. It is a lexical monster, making its home in the attics and cellars of chemical language – lists, indexes, and databases. It is a peculiar thing, a name in name only. In other words, it is systematic organic chemical nomenclature.

Organic chemical compounds – compounds containing the element carbon – constitute the better part of the molecular world. They include pharmaceuticals, plastics and other petrochemicals, environmental toxins, high-tech nanomaterials, and the molecules of life. For nearly two centuries, the study of these substances has been a cornerstone of the chemical sciences. There are more than one hundred million such compounds known today, and all of them have names like *3,4-dihydroxy-5-(1,2-dihydroxyethyl)furan-2(5H)-one*. The preamble to the nomenclature guidelines published by the International Union of Pure and Applied Chemistry (IUPAC) explains: "To be useful for *communication* among chemists, nomenclature for chemical compounds should additionally contain within itself an explicit or implied relationship to the structure of the compound, in order that the reader or listener can deduce the structure (and thus the identity) from the name."[1]

Each systematic organic chemical name expresses this relationship through its precise correspondence to a chemical diagram known as a "structural formula" (*Fig. 1*). Chemists have long employed such graphically-suggestive chemical formulas as valuable tools for theory and experiment.[2] They are the written trace of the imaginative visual thinking with which, since the

[1] Robert Panico et al., *A Guide to IUPAC Nomenclature of Organic Compounds: Recommendations 1993* (Boston: Blackwell Scientific Publications, 1993), xiii. Emphasis in original.

[2] Ursula Klein, *Experiments, Models, Paper Tools: Cultures of Organic Chemistry in the Nineteenth Century* (Stanford: Stanford University Press, 2003).

mid-nineteenth century, chemists have sounded the structure of the microworld.[3] Systematic names, in contrast, have been considered (when considered at all) as the denouement or *reductio ad absurdum* of a centuries-long project to make words reflect the chemical order of things.[4]

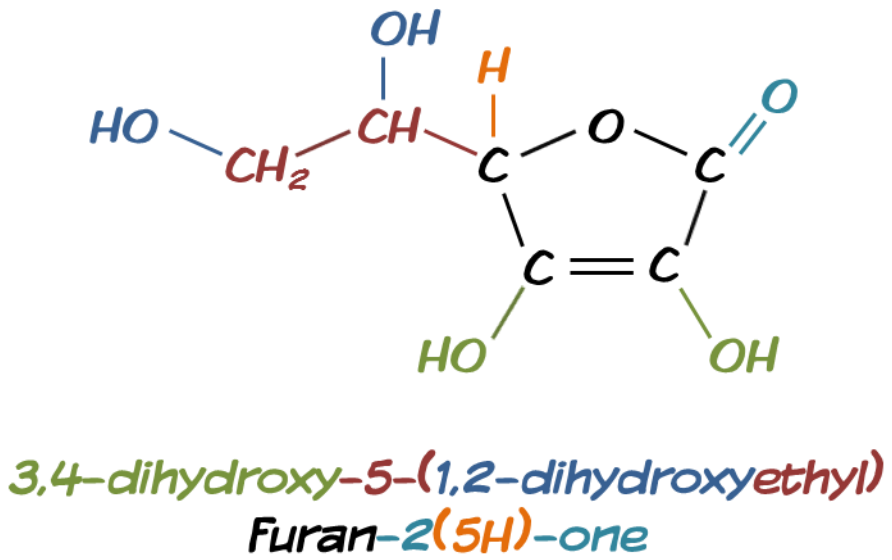

Figure 1. A structural formula and systematic name for vitamin C.

This dissertation is about the things that put the chemical world in order: systematic names for organic chemical compounds and chemical reference works compiled and searched by means of these names.[5] At the turn of the twentieth century, across Europe and America,

[3] Alan J. Rocke, *Image and Reality: Kekulé, Kopp, and the Scientific Imagination* (Chicago: University of Chicago Press, 2010).

[4] The 1892 Geneva Nomenclature Congress, conventionally considered the origin of systematic organic chemical nomenclature, is the ending point of the standard history of chemical nomenclature and notation, Maurice P. Crosland, *Historical Studies in the Language of Chemistry* (New York: Dover Publications, 1978). The philosopher François Dagognet saw systematic organic chemical nomenclature as the unraveling of a dream of perfect correspondence between chemical words and chemical things; François Dagognet, *Tableaux et langages de la chimie: Essai sur la représentation* (Seyssel: Champ Vallon, 2002). P. E. Verkade, longtime chairman of the organic nomenclature commission of the International Union of Pure and Applied Chemistry, provided an insider's view of the development of international nomenclature rules; Pieter Eduard Verkade, *A History of the Nomenclature of Organic Chemistry*, trans. S. G. Davies (Boston: Reidel, 1985).

[5] Ann Blair has described premodern scholarly reference books as resources for selecting, sorting, storing, and summarizing information; this is a reasonably good description of the chemical reference works that this

chemists were deeply engaged with the question of how best to name the rapidly accumulating objects of their discipline. These were not just debates about nomenclature. They were debates over what areas of the chemical sciences most urgently needed to be set in order: pedagogy or bibliography, the chemical laboratory or the chemical library. They were efforts to compile new information resources and to consolidate chemical science and industry on national and international levels. They were the foundation of the systems and institutions that have maintained order in the molecular world ever since. The story of why naming mattered then is the story of how naming matters today.

This dissertation advances three principal arguments concerning the development of the systematic nomenclature of organic chemistry from the 1880s through the 1930s. First, I argue that these names were (and are) not simply a transcription of ideas about the structure of chemical compounds. The notion that chemical names should correspond to chemical structure via chemical diagrams was not the starting point for systematic nomenclature. Rather, this guiding principle of ontology and information management in the chemical sciences was the outcome of the development of systematic nomenclature at the turn of the twentieth century. Over the five decades that I address, through debates over which users and uses of chemical names to prioritize and which to compromise, chemists converged upon a shared assumption about how chemical names should work. Regarded by most as practically unattainable, honored more often than not in the breach, this ideal nevertheless became well established: a chemical name should uniquely express the structure of a chemical compound by transcribing a diagrammatic representation of the compound into words.

Second, I argue that systems of nomenclature gained wide purchase in chemical practice not through the dictates of elite committees but through a new class of professional

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dissertation addresses. Ann Blair, *Too Much to Know: Managing Scholarly Information before the Modern Age* (New Haven: Yale University Press, 2010).

chemical editors and the chemical reference works that they compiled. Arranged and indexed according to the (putatively) timeless units of individual chemical compounds, comprehensive chemical handbooks and abstract journals were powerful information technologies. They enabled organic chemists to survey their discipline and gain access to its collective knowledge. Editors refashioned systems of nomenclature and notation to manage the considerable labor that it took to compile and to read these reference works. Systematic nomenclature was institutionalized through the inertia of this labor and through commissions less occupied with rationalizing chemical nomenclature than with protecting it from the rationalizing efforts of others.

Third, I argue that nomenclature reform was as much an exercise in organizing chemists, national industries, and international science as organizing chemicals. The reform and standardization of nomenclature was a primary motive for efforts to formalize international cooperation in chemistry. Such efforts proceeded alongside increasingly intensive economic and intellectual competition; at the outset of World War I, the cataclysmic conflict dubbed the "Chemists' War," international cooperation ground to a halt. Before and during the war, leaders of national chemical societies made reference works the keystone of projects to consolidate and strengthen the chemical sciences and industry of their nations. After the war, the entrenchment of nomenclature practices in tens of thousands of pages of indispensable handbooks was a crucial driver of rapprochement. Around 1930, briefly, chemical names helped to reunite the scientific world that World War I had torn asunder.

These are arguments about scientific representation and classification, the national and international organization of science, and the information infrastructure of modern science and industry. This dissertation might therefore seem to comprise three separate histories: an intellectual history, a political history, and a history of technology. It does not. Rather, the

chapters that follow recount a history of chemical names, diagrams, indexes, handbooks, committees, enterprises, and international ideals that emerges when, and only when, all of these matters are brought into a common frame.

This introduction will distinguish a few prominent threads in this skein. "Nominally Rational" contributes to the history of international scientific cooperation and competition, the history of visual and verbal representation in science, the history of classification, and the history of scientific publications as instruments of social order and economic power. Before turning to these topics, however, it will be helpful to describe what I mean by "systematic nomenclature."

Systematic nomenclature

Naming is a protean subject. For scientists – indeed, for practically everyone – names are objects of constant unreflective use, occasional ritualized complaints, and relatively rare episodes of intense deliberation. No field of inquiry can do without names of some sort, traditional or neologism, rule-governed or undisciplined, or, perhaps most often, all of the above. One of the main lessons of this dissertation is the fruitlessness of attempts to demarcate exactly what counts as a name, where it can be found, or how it is supposed to behave. Names tend not to stay where they are put. Nevertheless, I will hazard an approximate outline of the sorts of names that this dissertation addresses, and the sense in which it takes "systematic nomenclature" as its subject. By way of comparison, I begin with a sketch of the approaches to scientific naming most familiar to the chemists in this story and, most likely, to readers today.

The power of naming is deep, historical, and cross-cultural. In ancient Egyptian myth, Ra calls his fellow gods into being by speaking their names. One of them, Isis, overthrows him by learning Ra's own secret name. In Genesis, Adam asserts his dominion over the beasts of the earth and the birds of the air in the act of naming them; thereafter, natural beings carried their

names within them, as signatures of their essence. So claimed Michel Foucault, at any rate. Foucault defined the dawn of modernity as a disenchantment of this mysterious power of nomenclature; similarities between names and things were "no longer the form of knowledge but rather the occasion of error."[6] Francis Bacon, the best known historical proponent of this view, advised in his 1620 *Novum Organum* that names ought to relate to beings through definite correspondences, not loose associations. Otherwise, one risked falling victim to the "idols of the forum," a form of faulty reasoning in which the imprecisions of common language led discussions of the natural world astray into disputes over names.[7] Taken up by subsequent thinkers on language and science – the Abbé de Condillac, William Whewell, the logical positivists, to name a few – Bacon's view has become the default assumption about names in modern science.[8] Names, arbitrary in themselves, are vested with scientific meaning through the act of attaching them to particular objects in some socially sanctioned and well-defined fashion. In the Baconian tradition, names matter, but what matters about names is that a particular choice of name not matter.[9]

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[6] Michel Foucault, *The Order of Things: An Archaeology of the Human Sciences* (New York: Vintage Books, 1994), 3–77 (quotation on 51). Brian Ogilvie, among others, has shown that Foucault's schematic account of the premodern worldview fails to account for premodern practice. As Ogilvie puts it, "The Book of Nature was a powerful metaphor in the sixteenth century, as before and after, but naturalists, as naturalists, were concerned with establishing its text, not deciphering its language." Brian W. Ogilvie, *The Science of Describing: Natural History in Renaissance Europe* (Chicago: University of Chicago Press, 2006), 15–17 (quotation on 16). Lorraine Daston and Katherine Park have presented a complementary perspective on the history of conceptions of natural and human order that does not hew to Foucault's block periodization but emphasizes "sensibilities that overlapped and recurred like waves"; Lorraine Daston and Katherine Park, *Wonders and the Order of Nature, 1150-1750* (New York: Zone Books, 1998), 11.

[7] Francis Bacon, *The New Organon*, ed. Lisa Jardine and Michael Silverthorne, trans. Michael Silverthorne (New York: Cambridge University Press, 2000), 48 (aphorism LIX).

[8] On Condillac and his contemporaries, see Avi Lifschitz, *Language and Enlightenment: The Berlin Debates of the Eighteenth Century* (Oxford: Oxford University Press, 2012). On Whewell, see Simon Schaffer, "The History and Geography of the Intellectual World: Whewell's Politics of Language," in Menachem Fisch and Simon Schaffer, eds., *William Whewell, a Composite Portrait* (New York: Clarendon Press, 1991), 201–231. On the logical positivists, see Michael Friedman, *Reconsidering Logical Positivism* (New York: Cambridge University Press, 1999).

[9] For example, biologists recently named a newly-discovered species of sea mite after the actress and singer Jennifer Lopez. Vladimir Pešić et al., "A New Species of Litarachna (Acari, Hydrachnidia, Pontarachnidae) from a Caribbean Mesophotic Coral Ecosystem," *ZooKeys*, no. 425 (2014): 89–97.

Perhaps the best-known expressions of the Baconian approach were two Enlightenment projects in scientific nomenclature. The first was the system of botanical nomenclature introduced by the Swedish naturalist Carl Linnaeus, and the second was the method of chemical nomenclature developed by the French chemists Guyton de Morveau and Antoine Lavoisier. Though genealogically related, these two systems articulated different conceptions of how names were to be made useful and reliable.

In the mid-eighteenth century, Linnaeus standardized botanical nomenclature by divorcing it from description.[10] Previous naturalists named plants according to their distinguishing characteristics, often in a half-dozen words or more. Linnaeus pared these names down to a binomial nomenclature, assigning each species a genus name that classified the plant and a short, memorable "trivial name" that identified it.[11] As with his "artificial" system of classification, Linnaeus developed this nomenclature less as an expression of the order of nature than as a convenient means of record-keeping for his collaborative, global botanical enterprise.[12]

The system had its critics. The French naturalist George-Louis Leclerc, Comte du Buffon, a contemporary of Linnaeus, believed that the Linnaean obsession with naming and classifying distracted naturalists from what Buffon felt to be the proper subjects of natural history:

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[10] On the life and work of Linnaeus, see Lisbet Koerner, *Linnaeus: Nature and Nation* (Cambridge, MA: Harvard University Press, 1999).

[11] Koerner, *Linnaeus*, 14–55; Paul Lawrence Farber, *Finding Order in Nature: The Naturalist Tradition from Linnaeus to E.O. Wilson* (Baltimore: Johns Hopkins University Press, 2000), 8–13.

[12] This reading follows the recent work of Staffan Müller-Wille and Isabelle Charmantier on Linnaeus. On the distinction that Linnaeus drew between artificial and natural systems, see Staffan Müller-Wille, "Systems and How Linnaeus Looked at Them in Retrospect," *Annals of Science* 70, no. 3 (2013): 305–317. On Linnaeus' interleaved botanical notebooks as "boxes on paper," see Staffan Müller-Wille and Isabelle Charmantier, "Natural History and Information Overload: The Case of Linnaeus," *Studies in History and Philosophy of Science Part C* 43, no. 1 (2012): 4–15. Along the same lines, Koerner argues that, for Linnaeus, nomenclature was "not a complex theory" but rather "a useful technology," designed to enable his students to participate more effectively in his cameralist project of "collaborative economic botany"; Koerner, *Linnaeus*, 43, 55.

particular individuals and universal forces.[13] Such critiques, the persistence of vernacular classifications, the emergence of evolutionary theories, and doubts about the very existence of species all chipped away at the social and epistemic foundations of Linnaean classification.[14] However, Linnaean names were another matter; through the turn of the twentieth century and beyond, new rules of nomenclature tended to maintain the Linnaean approach.[15] The principle of *priority* decided nomenclature conflicts in favor of the name given in the first published description of the species.[16] The type specimen, a single physical sample, replaced the ideal "species" as the referent of a name, freeing naturalists to shift the corresponding taxon to different levels in the biological family tree.[17] Such rules kept order by keeping names arbitrary and unequivocal, liberating them to identify by barring them from describing or classifying.

The system of nomenclature developed by Guyton and Lavoisier, in contrast, tied chemical naming to both observation and classification.[18] Alongside new approaches to

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[13] Phillip R. Sloan, "The Buffon-Linnaeus Controversy," *Isis* 67, no. 3 (1976): 356–375; Farber, *Finding Order in Nature*, 13–21.

[14] For an overview of the history of nomenclature and classification in natural history from the beginning of the eighteenth century through the late twentieth century, see Farber, *Finding Order in Nature*; Robert E. Kohler, "Reflections on the History of Systematics," in Andrew Hamilton, ed., *The Evolution of Phylogenetic Systematics* (Berkeley: University of California Press, 2014), 17–46.

On vernacular classifications of living things, see Ralph Bulmer, "Why Is the Cassowary Not a Bird? A Problem of Zoological Taxonomy Among the Karam of the New Guinea Highlands," *Man* 2, no. 1 (1967): 5–25; Harriet Ritvo, *The Platypus and the Mermaid, and Other Figments of the Classifying Imagination* (Cambridge, MA: Harvard University Press, 1997); D. Graham Burnett, *Trying Leviathan* (Princeton: Princeton University Press, 2007).

On philosophy, rhetoric, and scientific practices pertaining to the question of the real existence of species, see Müller-Wille, "Systems and How Linnaeus Looked at Them in Retrospect" (and the works cited therein on 307 n8); Gordon McOuat, "The Origins of 'Natural Kinds': Keeping 'Essentialism' at Bay in the Age of Reform," *Intellectual History Review* 19, no. 2 (2009): 211–230; Kristin Johnson, *Ordering Life: Karl Jordan and the Naturalist Tradition* (Baltimore: Johns Hopkins University Press, 2012).

[15] Indeed, Linnaean binomial nomenclature also became the basis of most prominent modern codes of nomenclature for animals, bacteria, and viruses; Koerner, *Linnaeus*, 16, 87.

[16] Geoffrey C. Bowker, "The Game of the Name: Nomenclatural Instability in the History of Botanical Informatics," in Mary Ellen Bowden, Trudi Bellardo Hahn, and Robert Virgil Williams, eds., *Proceedings of the 1998 Conference on the History and Heritage of Science Information Systems* (Medford, NJ: Information Today, 1999), 74–83.

[17] Lorraine Daston, "Type Specimens and Scientific Memory," *Critical Inquiry* 31, no. 1 (2004): 153–182.

[18] The best recent biography of Lavoisier is Jean Pierre Poirier, *Lavoisier, Chemist, Biologist, Economist*, trans. Rebecca Balinski (Philadelphia: University of Pennsylvania Press, 1996).

On chemical and alchemical names before Lavoisier and Guyton, see Crosland, *Historical Studies*, 3–130; Jennifer M. Rampling, "Transmuting Sericon: Alchemy as 'Practical Exegesis' in Early Modern England," *Osiris* 29 (2014): 19–

chemical theory and experimental practice, the composition-based method of nomenclature that the two chemists published in 1787 was a keystone of the so-called "chemical revolution."[19] Guyton brought to their collaboration a binomial approach indirectly derived from Linnaean botanical names.[20] Lavoisier contributed a commitment to Condillac's principle that sound reasoning relied on "well-made language." He believed that a model of chemical composition based on his oxygen theory of combustion provided the foundation for such a language.[21]

For elements – defined empirically as substances that chemists could not decompose – Guyton and Lavoisier retained or coined trivial names, like those that Linnaeus assigned to each species. Chemical substances that *could* be decomposed –compounds – took on binomial names formed by combining the names of their immediate constituents, as attested by experimental evidence interpreted according to Lavoisier's theories. Unlike Linnaean botanical

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34; William R. Newman, "« Decknamen or Pseudochemical Language »? Eirenaeus Philalethes and Carl Jung," *Revue d'histoire des sciences* 49, no. 2/3 (1996): 159–188.

[19] Louis Bernard Guyton de Morveau et al., *Méthode de nomenclature chimique* (Paris: Cuchet, 1787). On the *Méthode*, see Crosland, *Historical Studies*, 174–92; Poirier, *Lavoisier*, 182–190; Dagognet, *Tableaux et langages de la chimie*, 11–49.

The historiography of the chemical revolution is the subject of a book of its own; John G. McEvoy, *The Historiography of the Chemical Revolution: Patterns of Interpretation in the History of Science* (Brookfield, VT: Pickering & Chatto, 2010). On major turning points in Lavoisier's research, see Henry Guerlac, *Lavoisier—the Crucial Year: The Background and Origin of His First Experiments on Combustion in 1772* (Ithaca: Cornell University Press, 1961); Frederic Lawrence Holmes, *Antoine Lavoisier—The Next Crucial Year: Or, the Sources of His Quantitative Method in Chemistry* (Princeton: Princeton University Press, 1998). On the reception of Lavoisier's chemistry, see Bernadette Bensaude-Vincent and Ferdinando Abbri, eds., *Lavoisier in European Context: Negotiating a New Language for Chemistry* (Canton, MA: Science History Publications/USA, 1995).

[20] Guyton took many of his ideas about nomenclature from the Swedish mineralogist Torbern Bergman. Bergman, in turn, had studied with Linnaeus, and adapted his former teacher's principles of botanical nomenclature to chemistry. Crosland, *Historical Studies*, 139–167; Marco Beretta, *The Enlightenment of Matter: The Definition of Chemistry from Agricola to Lavoisier* (Nantucket, MA: Science History Publications, 1993).

[21] Jessica Riskin, "Rival Idioms for a Revolutionized Science and a Republican Citizenry," *Isis* 89, no. 2 (June 1, 1998): 203–32; Lissa Roberts, "Condillac, Lavoisier, and the Instrumentalization of Science," *The Eighteenth Century* 33, no. 3 (1992): 252–271; Crosland, *Historical Studies*, 168–171, 177–180; Beretta, *The Enlightenment of Matter*; Wilda C. Anderson, *Between the Library and the Laboratory: The Language of Chemistry in Eighteenth-Century France* (Baltimore: Johns Hopkins University Press, 1984); Trevor Harvey Levere, "Lavoisier: Language, Instruments, and the Chemical Revolution," in *Nature, Experiment, and the Sciences: Essays on Galileo and the History of Science*, ed. Stillman Drake, Trevor Harvey Levere, and William R. Shea, (Boston: Kluwer Academic Publishers, 1990).

On "compositionism" in eighteenth-century chemistry, see Mi Gyung Kim, *Affinity, That Elusive Dream: A Genealogy of the Chemical Revolution* (Cambridge, MA: MIT Press, 2003), 65–110; Hasok Chang, *Is Water H_2O?: Evidence, Realism and Pluralism* (New York: Springer, 2012), 1–70 (defined on 14).

names, made to cut across taxonomic disputes, the new chemical names carried Lavoisier's chemistry with them. Before long, the chemist Joseph Priestley lamented, Lavoisier's nomenclature was "almost in universal use; so that, whether we adopt the new system or not" – and Priestley most certainly did not – "we are under the necessity of learning the new language."[22] Furthermore, whereas Linnaean names were designed for stability, the nomenclature of Guyton and Lavoisier contained the seeds of its own revision. In 1810, the British chemist Humphry Davy showed experimentally that the compound known as "oxymuriatic acid" was actually an element. "To call a body which is not known to contain oxygen, and which cannot contain muriatic acid, *oxymuriatic acid*," he wrote, "is contrary to the principles of that nomenclature in which it is adopted."[23] Davy christened this element "chlorine"; the French chemist Joseph Louis Gay-Lussac found muriatic acid to be a compound of this element and hydrogen, and renamed it "hydrochloric acid."[24] The revision of names was the affirmation of the method of nomenclature as an expression of the results of experiment.

The nomenclature of Lavoisier and Guyton put inorganic chemical compounds in order. The periodic system, first published in its most influential form by Dmitri Mendeleev in 1869, did the same for the chemical elements.[25] However, neither of these systems was of much help

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[22] Joseph Priestley, *Experiments and Observations Relating to the Analysis of Atmospherical Air; Also Farther Experiments Relating to the Generation of Air from Water, to Which Are Added, Considerations on the Doctrine of Phlogiston, and the Decomposition of Water* (Philadelphia: Thomas Dobson, 1796), 37.

On the reception and translation of the nomenclature of Lavoisier and Guyton, see Bensaude-Vincent and Abbri, *Lavoisier in European Context*; Crosland, *Historical Studies*, 184–214; Jan Golinski, "The Chemical Revolution and the Politics of Language," *The Eighteenth Century* 33, no. 3 (1992): 238–251; Michael D. Gordin, *Scientific Babel: How Science was Done Before and After Global English* (Chicago: University of Chicago Press, 2015), 15–20.

[23] Humphry Davy, "Some Reflections on the Nomenclature of the Oxymuriatic Compounds," in *The Collected Works of Sir Humphry Davy*, ed. John Davy, vol. 5 (London: Smith, Elder, 1840), 345, quoted in Crosland, *Historical Studies*, 221.

[24] Crosland, *Historical Studies*, 215–24; Jan Golinski, *Science as Public Culture: Chemistry and Enlightenment in Britain, 1760-1820* (New York: Cambridge University Press, 1992), 218–35; Iain P. Watts, "Philosophical Intelligence: Letters, Print and Experiment during Napoleon's Continental Blockade," *Isis* 106, no. 4 (2015): 749–770.

[25] On the development of the periodic system, see Michael D. Gordin, *A Well-Ordered Thing: Dmitrii Mendeleev and the Shadow of the Periodic Table* (New York: Basic Books, 2004), 15–45. As an entry point into the substantial

in dealing with organic chemical compounds. Working in the context of organic chemistry, chemists developed ways of thinking about molecules – and of drawing and naming them – that have taken root throughout the chemical sciences.[26]

Chapter 1 situates the late nineteenth-century starting point of this dissertation in the development of organic chemistry and organic chemical nomenclature over the preceding century.[27] Of particular significance was the emergence of *structure theory*: a collection of principles for understanding the behavior and relationships of organic compounds in terms of a particular model of their inner structure or "constitution." As more and more chemists framed their research according to these principles, they began to coin *rational names*: names based on the constitution of compounds. *Systematic nomenclature* was an attempt to standardize this practice through rules for expressing chemical structure in a consistent fashion.[28] To do so, nomenclature reformers hit upon the idea of establishing a direct relationship between *structural formulas* – diagrams drawn according to the principles of structure theory – and names. This was the ideal of systematic organic chemical nomenclature:

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literature on the history and philosophy of the periodic system and its expression in the many forms of the periodic table, see Eric R. Scerri, *The Periodic Table: Its Story and Its Significance* (New York: Oxford University Press, 2007).

[26] On the emergence of biochemistry and physical chemistry, respectively, out of organic chemistry around the turn of the twentieth century, see Robert E. Kohler, *From Medical Chemistry to Biochemistry: The Making of a Biomedical Discipline* (New York: Cambridge University Press, 1982); John W. Servos, *Physical Chemistry from Ostwald to Pauling: The Making of a Science in America* (Princeton: Princeton University Press, 1990). Mary Jo Nye has argued that physical chemistry constituted a "bridge, not a wedge" between the disciplines of physics and chemistry; Mary Jo Nye, *From Chemical Philosophy to Theoretical Chemistry: Dynamics of Matter and Dynamics of Disciplines, 1800-1950* (Berkeley: University of California Press, 1993).

[27] Principal sources include Rocke, *Image and Reality*, Klein, *Experiments, Models, Paper Tools;* Alan J. Rocke, *The Quiet Revolution: Hermann Kolbe and the Science of Organic Chemistry* (Berkeley: University of California Press, 1993); Alan J. Rocke, *Nationalizing Science: Adolphe Wurtz and the Battle for French Chemistry* (Cambridge, MA: MIT Press, 2001); John Hedley Brooke, *Thinking about Matter: Studies in the History of Chemical Philosophy* (Brookfield, VT: Variorum, 1995).

[28] All systematic chemical nomenclature depends on a vocabulary of "trivial" terms and inflections. Generally speaking, the smaller this trivial vocabulary, the more "systematic" the nomenclature. A mid-twentieth century expert on chemical nomenclature put it this way: "The customary division of organic nomenclature into 'systematic' and 'trivial, or non-systematic' does not express the very considerable dependence of the former on the latter, since systematic nomenclature is largely the use of a selected group of trivial names in a systematic manner." G. Malcolm Dyson, "A Cluster of Algorithms Relating the Nomenclature of Organic Compounds to their Structure Matrices and Ciphers," *Information Storage and Retrieval* 2, no. 3 (1964): 159.

each chemical compound should have one unique chemical name, corresponding in a precise, rule-bound fashion to a chemical diagram.

However, from the late nineteenth century through the present day, the systematic names that chemists have put into practice have diverged from each aspect of this ideal. There has never been one system, but always several. Within each system, many compounds could be assigned different, equally valid names, not one unique name. Rigorously rule-abiding names sat alongside trivial and semi-trivial names sanctioned as "systematic." Ambiguities in the correspondence between name and formula constantly emerged. In contrast to its guiding ideal, the practice of systematic chemical nomenclature has been characterized by pluralism, flexibility, and exceptions.[29]

A central contention of this dissertation is that the historical significance of systematic chemical nomenclature is not to be found through studying either ideals or practices alone. Comparing the two yields a more complete picture. However, even such an approach fails to capture the history of how these ideals and these practices came to be, how each gave rise to the other, and how the making of both has shaped the chemical sciences in the twentieth and twenty-first centuries. The history of the systematic nomenclature of organic chemistry lies in the push and pull of chemists, editors, institutions, and information technologies across the gap between the ideals and practices of chemical naming.

A chemical analogy may be helpful. A chemical reaction involves the making and breaking of bonds within one set of chemical substances, the reactants. The result of the reaction is a different set of chemical substances, the products, characterized by a different

[29] A number of histories of standardization are characterized by contrasts of this sort. See, e.g. Ken Alder, *Engineering the Revolution: Arms and Enlightenment in France, 1763-1815* (Princeton: Princeton University Press, 1997); Graeme Gooday, *The Morals of Measurement: Accuracy, Irony, and Trust in Late Victorian Electrical Practice* (New York: Cambridge University Press, 2004); On Barak, *On Time: Technology and Temporality in Modern Egypt* (Berkeley: University of California Press, 2013); Vanessa Ogle, *The Global Transformation of Time: 1870-1950* (Cambridge, MA: Harvard University Press, 2015).

pattern of bonds. On the left side of a reaction equation, the structural formulas of the reactants; on the right, the structural formulas of the products. However, it is in the transformation of reactants into products – what happens in the gap separating the structures on the left and those on the right – that chemistry happens.

In order to gain analytical purchase on this process, chemists came up with something that they call a "transition state." The transition state is a representation of the point at which reactants tip into products. It is a snapshot of the dynamics of a chemical reaction at its point of greatest energy and instability. Studying transition states can be challenging, but it has also been highly rewarding. Through these studies, chemists have gained considerable insight into the foundations of chemical change.[30]

The subject matter of this dissertation might be described as a collection of historical "transition states." I address the history of systematic nomenclature as the history of how practices have become ideals, and how ideals have become practices. Attending to this gap throws light onto other spaces that characterize this history: the space between representation and classification, between members of elite committees and anonymous editorial assistants, between chemical substances and commercial products. Told in this fashion, the history of chemical naming yields insight into the foundations of the twentieth-century chemical sciences. It may serve as a model for examining how, across a broad array of subjects and fields, methods of naming, hidden in plain sight, mold the world as they put it in order.

The structure of organic chemistry

For the scientist, a systematic name is a nexus. It brings together heterogeneous sources of information, different forms of evidence, and the various contexts in which the object to

[30] On transition states, see Mary Jo Nye, "Reaction Intermediates and Transition States: States of Matter or States of Mind?," in *Objects of Chemical Inquiry*, ed. Ursula Klein and Carsten Reinhardt (Sagamore Beach, MA: Science History Publications/USA), 237–259.

which the name refers may appear. Systematic nomenclature can do a similar duty for the historian. The development of nomenclature standards winds along intersecting paths through the history of organic chemistry, connecting several subjects that might each be referred to as "the structure of organic chemistry": the institutional landscape of international chemistry, a framework for representing and classifying chemicals, and a corpus of chemical information assembled in canonical texts. Chemists shaped organic chemistry in all of these respects as they made (and unmade) systems of chemical naming. In this section, I take up each of these topics in turn, addressing the scholarship that this dissertation builds upon and bridges.

The reform and standardization of chemical nomenclature played a central role in the organization of chemistry within national and, especially, international institutions at the end of the nineteenth century. Nomenclature was a focal point, too, of the tumultuous reorganization of such institutions during World War I and its aftermath. Throughout, institution-building chemists took international congresses and associations as the sites in which nomenclature reform ought to be addressed, and nomenclature as a problem that justified establishing such organizations. Unpacking the association between nomenclature and international institutions, self-evident as it may seem, reveals that these projects for organizing chemicals and for organizing chemists were linked in several ways. This dissertation shows that, as a subject of international cooperation, nomenclature reform proved to be a fruitful medium for cultivating a wide range of forms of chemical sociability. As a subject for international rivalry, chemists made nomenclature and chemical reference works into crucial resources for intellectual and economic competition.

In science, as in culture more broadly, international congresses and associations proliferated over the second half of the nineteenth century.[31] Among the most frequent subjects for these gatherings was the pragmatic matter of facilitating international coordination and communication by establishing international standards.[32] Like other standardization projects, nomenclature reform could be an instrument for industrial competition and for the consolidation of nations and their empires.[33] But these discussions were also implicated in the many other forms of international engagement that took place within such institutions: universalist and nationalist, socialist and liberal, avant-garde and Olympic, to name a few of the genres of internationalism that historians have described.[34]

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[31] Brigitte Schroeder-Gudehus, ed., "Les congrès scientifiques internationaux," special issue of *Relations internationales* 62 (1990): 111–211; Robert Fox, *Science without Frontiers: Cosmopolitanism and National Identity in the World of Learning, 1870-1940* (Corvallis, OR: Oregon State University Press, forthcoming 2016).

For an overview of European internationalism before World War I, see Martin H. Geyer and Johannes Paulmann, *The Mechanics of Internationalism: Culture, Society, and Politics from the 1840s to the First World War* (New York: Oxford University Press, 2001); F. S. L. Lyons, *Internationalism in Europe, 1815-1914* (Leyden: A.W. Sythoff, 1963).

[32] On the role of pragmatic matters of communication and coordination in late-nineteenth century international scientific cooperation, see Anne Rasmussen, "Jalons pour une histoire des congrès internationaux au XIXe siècle: Régulation scientifique et propagande intellectuelle," *Relations internationales* 62 (1990): 115–133; Brigitte Schroeder-Gudehus, "Les congrès scientifiques et la politique de cooperation internationale des académies des sciences," *Relations internationales* 62 (1990): 135–148.

On standards and measurement in general, see M. Norton Wise, ed., *The Values of Precision* (Princeton: Princeton University Press, 1994); Martha Lampland and Susan Leigh Star, eds., *Standards and Their Stories: How Quantifying, Classifying, and Formalizing Practices Shape Everyday Life* (Ithaca: Cornell University Press, 2009); James Sumner and Graeme Gooday, eds. "By Whose Standards?: Standardization, Stability and Uniformity in the History of Information and Electrical Technologies," special issue of *History of Technology* 28 (2008): 1–168.

[33] Scholarship on the history of time standardization, the standardization of electrical units, and the standardization of nomenclature and taxonomy in systematic biology makes this point particularly effectively. On time, see Peter Galison, *Einstein's Clocks and Poincaré's Maps: Empires of Time* (New York: W.W. Norton, 2003); Ogle, *The Global Transformation of Time*; Barak, *On Time*. On electrical units, see Gooday, *The Morals of Measurement;* Simon Schaffer, "Late Victorian Metrology and Its Instrumentation: A Manufactory of Ohms," in *The Science Studies Reader*, ed. Mario Biagioli (New York: Routledge, 1999), 457–478. On systematic biology, see Johnson, *Ordering Life;* Jim Endersby, *Imperial Nature: Joseph Hooker and the Practices of Victorian Science* (Chicago: University of Chicago Press, 2008).

[34] Geert J. Somsen, "A History of Universalism: Conceptions of the Internationality of Science from the Enlightenment to the Cold War," *Minerva* 46, no. 3 (September 1, 2008): 361–379; Fox, *Science without Frontiers*.

As a point of comparison outside of the natural sciences, Samuel Moyn has addressed liberal, socialist, and other modes of internationalism in the history of international human rights advocacy; Samuel Moyn, *The Last Utopia: Human Rights in History* (Cambridge, MA: Belknap Press, 2010).

On avant-garde cosmopolitanism vis-à-vis socialist and nationalist ideologies, see Marci Shore, "On Cosmopolitanism, the Avant-Garde, and a Lost Innocence of Central Europe," in *Utopia/dystopia: Conditions of Historical Possibility*, ed. Michael D. Gordin, Helen Tilley, and Gyan Prakash (Princeton: Princeton University Press, 2010), 176–202.

Scholarship on international science has emphasized how the diversity of pre-1914 internationalism narrowed during and after World War I. International organizations became representative bodies of national organizations, hewing to (and sometimes surpassing) the political alliances and enmities of their respective nation-states, most famously in the "boycott" of the former Central Powers from the constellation of international unions founded after World War I.[35] However, recent work on the League of Nations has paved the way for a broad reconsideration of interwar international institutions as more than a means of legitimizing the interests and grudges of powerful nation-states.[36]

This dissertation builds upon such studies of international relations in science and beyond, in two ways. First, instead of simply taking nomenclature reform as typical subject matter for international cooperation, I track the shifting affinities that bound nomenclature reform and international chemical organizations together in the first place. Members of the upper echelons of European academic chemistry, dissatisfied with the fragmented, contentious character of many international chemical meetings, made nomenclature the centerpiece of a different sort of organization. Crucially, such chemists saw nomenclature as something that mattered to everyone in their field, but did not matter too much to anyone. In the decades preceding World War I, chemical naming became the central problem around which a transnational chemical elite built institutions for intimate and wide-ranging intellectual exchange. Nomenclature reform played a different role within the international chemical organizations of the 1920s. Attending carefully to the official and unofficial work of the

[35] Brigitte Schroeder-Gudehus, *Les scientifiques et la paix: La communauté scientifique internationale au cours des années 20* (Montréal: Presses de l'Université de Montréal, 1978); Elisabeth T. Crawford, *Nationalism and Internationalism in Science, 1880-1939: Four Studies of the Nobel Population* (New York: Cambridge University Press, 1992), 49–78; Michael D. Gordin, *Scientific Babel*, 173–180.

[36] Susan Pedersen, a prominent contributor to this scholarship, has surveyed it in Susan Pedersen, "Back to the League of Nations," *The American Historical Review* 112, no. 4 (2007): 1091–1117; Susan Pedersen, *The Guardians: The League of Nations and the Crisis of Empire*, (Oxford: Oxford University Press, 2015), 424 n7.

members of the nomenclature commission of the International Union of Pure and Applied Chemistry, I demonstrate that international cooperation was not a default state to which science reverted when political tensions ebbed, nor a fragile compromise shattered once and for all by war. This dissertation shows how, for some, chemical naming was a means for establishing the legitimacy of an institution riven by political divisions, while for others, it was a means of breaking down these divisions.[37]

Second, I attend to the local and national agendas of members of international congresses and committees. The chemists who were most intent on tying nomenclature reform to international organizations often had a particular stake in doing so. Often, these stakes included reckoning with or preparing for the aftermath of war. For Alsatian chemists embroiled in Parisian scientific politics in the shadow of the Franco-Prussian war, international nomenclature reform provided a basis for renovating French chemical pedagogy and the international prestige of French chemistry. During World War I, when Entente scientists sought to explain the strength of German chemistry in order to plan for postwar economic competition, they seized upon chemical reference literature as German chemistry's secret weapon. In illuminating the role of international cooperation in fostering pedagogical and information resources for geopolitical competition, this dissertation plumbs the foundations of some of the characteristic features of Cold War science in this earlier period.[38]

The various nomenclature reform committees of different international organizations took up the same central questions: how and to what degree to bring chemical names into

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[37] On the history of the technical subject matter of IUPAC work on organic chemical nomenclature, see Verkade, *A History*. On the institutional history of IUPAC, with an emphasis on the shifting structure of the organization, see Roger Fennell, *History of IUPAC, 1919-1987* (Boston: Blackwell Science, 1994); Stanley S. Brown and Roger Fennell, *History of IUPAC, 1988-1999* (Ephrata, PA: International Union of Pure and Applied Chemistry, 2001).

[38] For an overview of current scholarship on Cold War science in American and global history, respectively, see Audra J. Wolfe, *Competing with the Soviets: Science, Technology, and the State in Cold War America* (Baltimore: Johns Hopkins University Press, 2013); Naomi Oreskes and John Krige, eds., *Science and Technology in the Global Cold War* (Cambridge, MA: MIT Press, 2014).

precise alignment with chemical diagrams. If systematic nomenclature could be tied to structural formulas, so the thinking went, then the name of a compound, like the formula diagram representing it, could be interpreted according to the principles of structure theory. Such a name would express the compound's internal structure and fix its position in the structure of chemical relationships spanning the molecular world. Here, then, are two further aspects of the structure of organic chemistry: chemical representation and chemical classification. Chemists at the turn of the twentieth century sought to base both of these on chemical names chained to chemical diagrams, and to unite chemical representation and classification in the process. This dissertation explores how and why they did so.

Genres of scientific representation have their own epistemic and political cultures. Historians have shown how chemical symbols, images, and language have embodied values about what makes for reliable knowledge and a proper social order, within science and without.[39] Some have illuminated these cultures through studies of different approaches to one form of representation. In the field of nomenclature, Jessica Riskin has contrasted the political ideologies expressed in "social" and "cultural" approaches to chemical naming in the eighteenth century; Bernadette Bensaude-Vincent has followed the development of "legislative" and "conventionalist" attitudes toward nomenclature over the centuries since.[40] Others have drawn distinctions among different forms of representation. For example, the philosopher François

[39] Two useful entry points into the voluminous scholarship on representation in science are Michael Lynch and Steve Woolgar, eds., *Representation in Scientific Practice* (Cambridge, MA: MIT Press, 1990); Catelijne Coopmans et al., eds., *Representation in Scientific Practice Revisited* (Cambridge, MA: MIT Press, 2014).

On practices of representation in the chemical sciences, see Klein, *Experiments, Models, Paper Tools*; Rocke, *Image and Reality*, Natasha Myers, *Rendering Life Molecular: Models, Modelers, and Excitable Matter* (Durham: Duke University Press, 2015); Roald Hoffmann and Pierre Laszlo, "Representation in Chemistry," in *Roald Hoffmann on the Philosophy, Art, and Science of Chemistry*, ed. Jeffrey Kovac and Michael Weisberg (New York: Oxford University Press, 2012), 163–192; and chapters by Christoph Meinel, Soraya de Chadarevian, and Eric Francoeur and Jérôme Segal in Soraya de Chadarevian and Nick Hopwood, eds., *Models: The Third Dimension of Science* (Stanford: Stanford University Press, 2004).

[40] Riskin, "Rival Idioms"; Bernadette Bensaude-Vincent, "Languages in Chemistry," in *The Cambridge History of Science*, vol. 5, 174–190.

Dagognet arranged chemical representations into a hierarchy of suggestiveness and comprehensiveness, with names and symbols at the bottom and tables and diagrams at the top.[41] The focus on images as a privileged form of chemical representation – the "iconic vernacular" and "graphic language" of chemistry, as two chemists put it in 1991 – is a primary frame for the story that this dissertation will tell.[42] Alan Rocke has shown the importance of imaginative visual reasoning in the work of some of the founding figures of structure theory, even as they emphasized the danger of approaching these images without epistemic caution. [43]

This dissertation focuses not just on the properties of chemical names, diagrams, and symbols, but on how chemical representations were transformed back and forth into each of these forms, reinforcing certain conceptions of what chemical substances were like. Peter Galison has described an encounter between different forms of representation as a crucial aspect of experimental high-energy physics in the late twentieth century. Distinct "image" and "logic" cultures came together to yield a hybrid mode of representation enabling coordination among a vast, heterogeneous collection of scientific subcultures.[44] The stakes and slipperiness of such encounters within modern chemistry are suggested by the persistent tendency of

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[41] Dagognet, *Tableaux et langages de la chimie*. The conclusion of this dissertation will return to Dagognet at greater length.

[42] Hoffmann and Laszlo, "Representation in Chemistry," 176, 179.

[43] Rocke, *Image and Reality*. Peter Galison and Lorraine Daston have described broad shifts in scientific image-making over the past three centuries in terms of fears about the capacity of images to mislead and the consequent emergence of "epistemic virtues" entailing different conceptions of the ideal "scientific self" and its relation to the making and interpretation of images; Lorraine Daston and Peter Gailson, *Objectivity* (Cambridge, MA: Zone Books, 2007).

[44] Peter Galison has detailed how distinct cultures of experimental high-energy physics emerged around two forms of evidence representing events at the subatomic scale: first, images that recorded a rich trove of particle tracks overlaid on one another, and second, discrete numerical counts of particular coincident events. Within these cultures of "image" and "logic," physicists and engineers trained within ostensibly incommensurable scientific subfields managed to negotiate a provisional set of concepts and terms consistent enough for them to advance a common cause. Analyzing this model of scientific collaboration in anthropological terms, Galison describes such sites as "trading zones." Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997).

chemists to refer to chemistry itself as a language.[45] Ursula Klein has brought this perspective to bear upon Berzelian formulas – the concatenations of element symbols (H_2O, CO_2, and so forth) that are perhaps the best-known form of representation in modern chemistry. Though scholars such as Dagognet often treated Berzelian formulas as non-visual representations (i.e., names or symbols), nineteenth-century chemists themselves, Klein has shown, treated them graphically. Chemists drew and re-drew these formulas in different two-dimensional configurations in order to model chemical constitution, describe reactions, and classify compounds. Chemists even incorporated Berzelian formulas as components of their experimental systems.[46] Representations do not merely re-present; they are also, as Klein puts it, "paper tools."[47]

I build on this scholarship in two ways. First, picking up where Klein and Rocke leave off, this dissertation addresses the development of a culture of representation that runs in counterpoint to their stories: from the suggestiveness of the visual to the rigidity of the verbal. In principle, structural formulas were an ideal basis for chemical classification; in practice, they were expensive to print and nearly impossible to index or pronounce. Systematic nomenclature

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[45] See, e.g., Pierre Laszlo, "Towards Teaching Chemistry as a Language," *Science & Education* 22, no. 7 (2011): 1669–1706; Emily R. Grosholz and Roald Hoffmann, "How Symbolic and Iconic Languages Bridge the Two Worlds of the Chemist: A Case Study from Contemporary Bioorganic Chemistry," in Hoffmann, *Roald Hoffmann on the Philosophy, Art, and Science of Chemistry*, 222–241. Bernadette Bensaude-Vincent has emphasized the persistence of this metaphor; Bensaude-Vincent, "Languages in Chemistry," 189. On the various meanings that historians and other scholars have attributed to "scientific language," see Gordin, *Scientific Babel*, 1–22, 325.

[46] Klein, *Experiments, Models, Paper Tools*, 27–35 (on "graphic suggestiveness"), 118–187 (on reactions, classification, and constitution), 231–247 (on formulas and experimental systems). Klein draws upon the work of Hans-Jörg Rheinberger on experimental systems and their characteristic production of inscriptions; Hans-Jörg Rheinberger, *Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube* (Stanford: Stanford University Press, 1997). On tools and experimental systems in the modern life sciences, see also Adele Clarke and Joan H. Fujimura, eds., *The Right Tools for the Job: At Work in Twentieth-Century Life Sciences* (Princeton: Princeton University Press, 1992). On scientific instruments in general, see Bernward Joerges and Terry Shinn, eds., *Instrumentation Between Science, State and Industry* (Boston: Kluwer Academic Publishers, 2001).

As David Kaiser has shown, Feynman diagrams representing interactions among subatomic particles similarly served as both heuristic visual models and instrument of calculation. David Kaiser, *Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics* (Chicago: University of Chicago Press, 2005).

[47] Klein, *Experiments, Models, Paper Tools*; Ursula Klein, ed., *Tools and Modes of Representation in the Laboratory Sciences* (Boston: Kluwer Academic Publishers, 2001).

emerged in this gap. These names were to serve as tools for classifying chemical compounds, abstracting the properties of diagrammatic formulas deemed most useful for classification and expressing them in a fixed verbal form. Graphical formulas were the supple medium with which chemists fashioned remarkable insights about chemical identity and relationships; systematic names were to be the plaster casts that fixed these insights as a basis for chemical order.

Second, the culture of representation that grew up around systematic nomenclature was not a matter of words alone. This approach to naming also reshaped how chemists interpreted their diagrams and even how they thought about chemicals themselves. The conversion of diagram to name was effected by abstracting patterns within structural formulas. From the beginning, some chemists recognized that this marked a shift in the kind of object to which a chemical name referred. As one prominent chemist remarked in 1892, "such systematic names as had been suggested were really names of formulae rather than names of substances."[48] Systematic names reduced visual representations to data that could be stored efficiently, recovered, and reconstituted in a particular diagrammatic form. (In fact, as the conclusion will outline, systematic nomenclature became the foundation for the representation of molecular structure on computers.)[49] This dissertation will show how the development of "names of formulae" as tools for classification reinforced the use of structural formulas themselves for chemical representation. It will also show that, in bridging the gap in scale between representing one compound and classifying hundreds of thousands of them,

[48] Alexander Crum Brown, quoted in Henry Armstrong, "Contributions to an International System of Nomenclature. The Nomenclature of Cycloids," *Proceedings of the Chemical Society* 8, no. 114 (1892): 130.

[49] On the digital representation of images and the visual representation of data, see Annamaria Carusi et al., eds., *Visualization in the Age of Computerization* (New York: Routledge, 2015); Orit Halpern, *Beautiful Data: A History of Vision and Reason since 1945* (Durham: Duke University Press, 2014).

systematic nomenclature shaped chemists' default conception of the fundamental units of the chemical world.

International committees discussed how tens of thousands of chemicals might be named; chemical editors actually named them. The forms of systematic organic chemical nomenclature that became part of the practice of chemistry were created by editors and disseminated in the pages of the enormous chemical reference works that they compiled. This is one more aspect of the structure of organic chemistry: the collective knowledge of the field, gathered and ordered within enormous, canonical handbooks and indexes. During the period that this dissertation addresses, a few reference works became particularly prominent. As historical sources, these handbooks, registers, and abstract journals are easy to overlook; as historical actors, so are their editors. After all, as compilations of data and information gleaned from other publications, these texts sit at least two degrees removed from what typically counts as scientific practice. However, for chemists at the turn of the twentieth century, reference works were essential resources, and they are the foundation on which the information infrastructure of twentieth- and twenty-first-century chemistry was built.

The historiography of practices of collecting and compiling knowledge has become particularly active in recent years, spurred on in part by interest in situating contemporary characterizations of "information overload" and "big data" in historical context.[50] During the

[50] In roughly chronological order of subject matter: Blair, *Too Much to Know;* Richard R. Yeo, *Notebooks, English Virtuosi, and Early Modern Science* (Chicago: University of Chicago Press, 2014); James Delbourgo and Staffan Müller-Wille, ed., "Listmania," focus section of *Isis* 103, no. 4 (2012), 710–752; David Sepkoski, "Towards 'A Natural History of Data': Evolving Practices and Epistemologies of Data in Paleontology, 1800–2000," *Journal of the History of Biology* 46, no. 3 (2013): 401–444; Eugenia Lean, "Proofreading Science: Editing and Experimentation in Manuals by a 1930s Industrialist," in Jing Tsu and Benjamin A. Ellman, eds., *Science and Technology in Modern China, 1880s-1940s* (Boston: Brill, 2014), 185–208; Bruno J. Strasser, "Collecting Nature: Practices, Styles, and Narratives," *Osiris* 27 (2012): 303–340; Bruno J. Strasser, "Collecting, Comparing, and Computing Sequences: The Making of Margaret O. Dayhoff's Atlas of Protein Sequence and Structure, 1954–1965," *Journal of the History of Biology* 43 (2010): 623–660; Rebecca Lemov, *Database of Dreams: The Lost Quest to Catalog Humanity* (New Haven: Yale University Press, 2015); Hallam Stevens, *Life out of Sequence: A Data-Driven History of Bioinformatics* (Chicago: University of Chicago Press, 2013).

nineteenth century, in chemistry as in other disciplines, scientific journals steadily increased in quantity, size, and authority.[51] With the tandem proliferation of new chemicals and new journal articles, chemists increasingly came to rely upon resources such as *Beilsteins Handbuch der Organischen Chemie*, which incorporated data and information from a century's worth of published research, organized by chemical compound.[52] Structured in this manner, organic chemistry became a discipline of the sort that Lorraine Daston has christened the "sciences of the archive": a trans-historical imagined community engaged in "collective empiricism," connected across space and time by massive compilations of data.[53] Geoff Bowker and Susan Leigh Star have emphasized that developing a classification scheme to organize such a vast trove of information takes work, that such classification schemes perform work, and that, ultimately, particular people do the work of classification and the work that classifications do.[54]

This dissertation contributes to the historiography of scientific compilation, first, by considering this work and these people. Chemical compilations deserve attention in their own right. Organic chemists considered them absolutely indispensable, and organic chemistry was as prominent – and as prominently characterized by claims of information overload – as any field of science at the turn of the twentieth century (a high point for such claims). I show how national chemical societies took up systematic nomenclature as a means of building national

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[51] On scientific periodicals in the nineteenth century, see Melinda Baldwin, *Making "Nature": The History of a Scientific Journal* (Chicago: University of Chicago Press, 2015); Aileen Fyfe, Julie McDougall-Waters and Noah Moxham, eds., "350 Years of Scientific Periodicals," special issue of *Notes and Records* 69, no. 3 (2015): 227–352; Alex Csiszar, *The Scientific Journal: Authorship and Knowledge in the Nineteenth Century* (forthcoming).

[52] Michael D. Gordin, "Beilstein Unbound: The Pedagogical Unraveling of a Man and His Handbuch," in *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives,* ed. David Kaiser (Cambridge, MA: MIT Press, 2005), 11–39.

[53] Lorraine Daston, "The Sciences of the Archive," *Osiris* 27 (2012): 156–187.

[54] Geoffrey C. Bowker and Susan Leigh Star, *Sorting Things Out: Classification and Its Consequences* (Cambridge, MA: MIT Press, 1999). Star and Bowker describe the subject of their book as "the creation and maintenance of complex classifications as a kind of work practice, with its attendant financial, skill, and moral dimensions" (5). For an actor's perspective on the work of chemical compilation, see Janet D. Scott, "The Chemist at Work. XIV. My Work with Chemical Abstracts," *Journal of Chemical Education* 15, no. 6 (1938): 271–275.

communities of chemical science and industry.[55] Names, notation, and cross-references knit together various chemical publications, providing their readers with privileged access to the collective knowledge of chemistry. Such compilations required substantial intellectual labor from a new kind of chemical professional: the full-time chemist-editor. While international nomenclature commissions were mainly populated by the chemical elite, members of groups marginalized in other areas of science – women, Jews, provincials, the physically disabled – found or created roles for themselves in this specialization.[56]

Second, this dissertation explores how approaches to representation and classification were remade in the practice of compilation. Finding a system of nomenclature handed down by international committees to be a poor tool for their purposes, chemical editors adapted it according to their needs or traded it for other forms of notation. As Andrew Warwick and David Kaiser have pointed out, the effective manipulation of scientific representations constitutes a "craft skill," developed, like skills of experiment and observation, through practice.[57] The same went for systematic nomenclature. It was only through the daily labor of compiling reference works that a cadre of editors fashioned systems of nomenclature that were

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[55] There is a substantial historiography of the modern chemical industry. As a start, see Anthony S. Travis, *The Rainbow Makers: The Origins of the Synthetic Dyestuffs Industry in Western Europe* (Bethlehem, PA: Lehigh University Press, 1993); Kathryn Steen, *The American Synthetic Organic Chemicals Industry: War and Politics, 1910-1930* (Chapel Hill: University of North Carolina Press, 2014); Alfred D. Chandler, *Shaping the Industrial Century: The Remarkable Story of the Evolution of the Modern Chemical and Pharmaceutical Industries* (Cambridge, MA: Harvard University Press, 2005); Fred Aftalion, *A History of the International Chemical Industry: From the Early Days to 2000*, 2nd ed. (Philadelphia: Chemical Heritage Press, 2001).

Studies of individual chemical firms include David A. Hounshell and John K. Smith, *Science and Corporate Strategy: Du Pont R&D, 1902-1980* (New York: Cambridge University Press, 1988); Carsten Reinhardt and Anthony S. Travis, *Heinrich Caro and the Creation of Modern Chemical Industry* (Boston: Kluwer Academic Publishers, 2000); Kenneth Bertrams et al., *Solvay: History of a Multinational Family Firm* (New York: Cambridge University Press, 2013).

[56] Margaret Rossiter has addressed the opportunities for American women (and the limits of these opportunities) afforded by the closely related field of chemical librarianship; Margaret W. Rossiter, "Chemical Librarianship: A Kind of 'Women's Work' in America," *Ambix* 43, no. 1 (1996): 46–58. A parallel trend in contemporaneous theoretical physics became more apparent because of the sudden rise of the latter field to prominence and prestige; Christa Jungnickel and Russell McCormmach, *Intellectual Mastery of Nature: Theoretical Physics From Ohm to Einstein*, vol. 2 (Chicago: University of Chicago Press, 1986).

[57] Andrew Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (Chicago: University of Chicago Press, 2003), esp. 1–48; Kaiser, *Drawing Theories Apart*.

useful – or, more to the point, that were used. In doing so, they entrenched the diagram-based definition of chemical identity within the ever-growing edifice of chemical information.

Once again: these accounts of the structure of organic chemistry are not separate histories, but interconnected and mutually dependent aspects of one story. A French chemist's xenophobic fear of Anglo-Saxon conspiracies hinged on the logic of chemical representation. The ebb and flow of principled resistance to a nomenclature proposal depended on the editorial affiliations of those supporting it. The advent of interest in systematic nomenclature was a function of Parisian intellectual politics. These links and those noted above are among the many that emerge through close reading of congress minutes, committee reports, chemists' diaries and correspondence, and the inner recesses of sprawling reference works, among other sources. Social and epistemic, technological and political, the values and assumptions articulated in the development of systematic nomenclature at the turn of the twentieth century have shaped the structure of organic chemistry – and the myriad fields of science, technology, and medicine that draw upon it – through the present day.

On tedium

One final aspect of the systematic nomenclature of organic chemistry bears mentioning: by and large, organic chemists themselves have done their best to avoid it. The opening sentences of a 500-page history of the subject acknowledge that organic nomenclature "does not generate much interest among practicing organic chemists" and "is frequently regarded as a necessary evil."[58] Of course, a large proportion of scientific practice is mundane: routine, invisible, or crushingly repetitive.[59] The tedium of systematic nomenclature is something

[58] Kurt L. Loening, Foreword to Verkade, *A History*, ix.

[59] Classic treatments of "normal science," "invisible technicians," and repetitive scientific labor are, respectively, Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 4th ed. (Chicago: University of Chicago Press, 2012), 23–51;

different. Whenever they could, most chemists have ignored systematic nomenclature. When they could not, as in preparing manuscripts for publication, chemists have found navigating thickets of nomenclature rules to be "'necessary torture' or 'torturous necessity,'" as one journal editor expressed the attitudes of his authors.[60] And that was a relatively positive response. Another group of authors griped in a footnote that the systematic names forced upon them by a referee were nothing but "useless obfuscation."[61] Even the designers of a commemorative stamp celebrating the centenary of systematic organic chemical nomenclature elected to steer clear of systematic organic chemical names (*Fig. 2*).

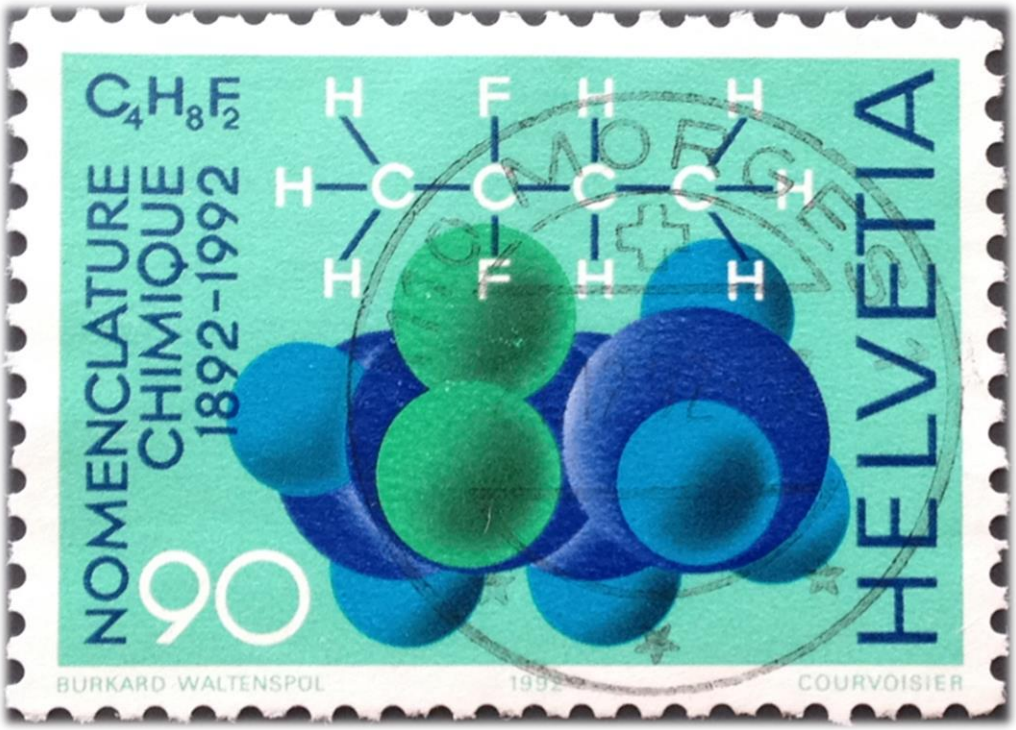


Figure 2. The Swiss Post celebrated the centenary of systematic nomenclature with a molecular formula, structural formula, and space-filling model – but no name – for 2,2-difluorobutane. (Author's photograph. Stamp © Swiss Post.)

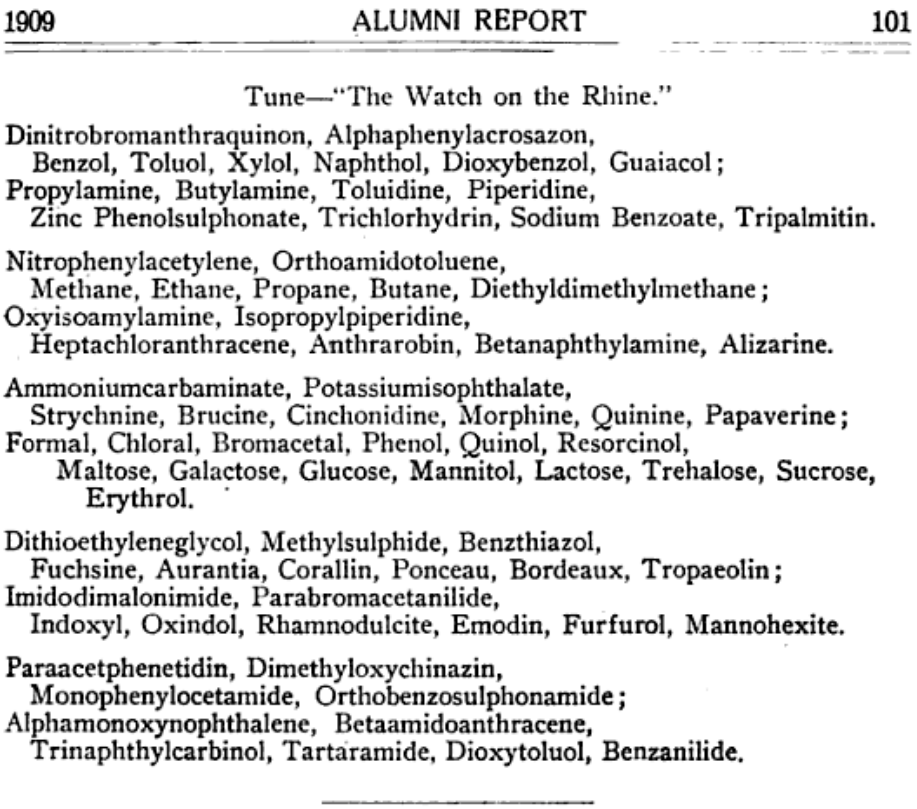
Steven Shapin, "The House of Experiment in Seventeenth-Century England," *Isis* 79, no. 3 (1988): 373–404; Simon Schaffer, "Astronomers Mark Time: Discipline and the Personal Equation," *Science in Context* 2 (1988): 115–145.

[60] M. Volkan Kısakürek, "Chemistry Journals and Nomenclature, 1892-1930," in *Organic Chemistry: Its Language and Its State of the Art*, ed. M. Volkan Kısakürek, (New York: VHCA, 1993), 75.

[61] Philip E. Eaton et al., "Synthesis of Peristylane and the Acs-(Cs)-C14-Tetraquinane, C14-Pentaquinane, and Norperistylane Systems," *Journal of the American Chemical Society* 99, no. 8 (1977): 2767 n2.

Chemical naming was not always so despised. Around 1900, chemists celebrated the unsystematic but nevertheless jawbreaking trivial and semitrivial names of organic compounds as shibboleths affirming their distinctive professional identity (*Fig. 3*). Systematic names, in contrast, have been omnipresent but studiously ignored, hidden in plain sight.

This dissertation is, in part, about how systematic chemical nomenclature became tedious. It is a story of illegible and unpronounceable names, the arcane rules that fill the 1,568 pages of the latest IUPAC *Nomenclature of Organic Chemistry*, the aura of technicality that casts matters of nomenclature as unfit for reflection, the nagging sense that, despite it all, it would be unwise to ignore systematic names completely.



1909 ALUMNI REPORT 101

Tune—"The Watch on the Rhine."

Dinitrobromanthraquinon, Alphaphenylacrosazon,
Benzol, Toluol, Xylol, Naphthol, Dioxybenzol, Guaiacol;
Propylamine, Butylamine, Toluidine, Piperidine,
Zinc Phenolsulphonate, Trichlorhydrin, Sodium Benzoate, Tripalmitin.

Nitrophenylacetylene, Orthoamidotoluene,
Methane, Ethane, Propane, Butane, Diethyldimethylmethane;
Oxyisoamylamine, Isopropylpiperidine,
Heptachloranthracene, Anthrarobin, Betanaphthylamine, Alizarine.

Ammoniumcarbaminate, Potassiumisophthalate,
Strychnine, Brucine, Cinchonidine, Morphine, Quinine, Papaverine;
Formal, Chloral, Bromacetal, Phenol, Quinol, Resorcinol,
Maltose, Galactose, Glucose, Mannitol, Lactose, Trehalose, Sucrose,
Erythrol.

Dithioethyleneglycol, Methylsulphide, Benzthiazol,
Fuchsine, Aurantia, Corallin, Ponceau, Bordeaux, Tropaeolin;
Imidodimalonimide, Parabromacetanilide,
Indoxyl, Oxindol, Rhamnodulcite, Emodin, Furfurol, Mannohexite.

Paraacetphenetidin, Dimethyloxychinazin,
Monophenylocetamide, Orthobenzosulphonamide;
Alphamonoxynophthalene, Betaamidoanthracene,
Trinaphthylcarbinol, Tartaramide, Dioxytoluol, Benzanilide.

Figure 3. A chemists' drinking song, c. 1900.[62]

[62] *Philadelphia College of Pharmacy Alumni Report* 46, no. 4 (1909), 101.

A history of scientific tedium is a quixotic enterprise. This dissertation focuses on precisely the points at which the eyes of chemists glazed over. It is founded on close readings of texts that were built specifically and exclusively to be searched and browsed. It treats names not as signifiers but as actors, wandering through dramas of epistemology, technology, and international politics, playing crucial but unobtrusive roles. It is an exercise in catching glimpses of the shadow life of scientific representations: how names and formulas behaved when, after the scientist turned off the lights, they left the bench or blackboard and crept back to their places in indexes, nestled among hundreds of thousands of their peers.

This dissertation, then, is written in something like the spirit in which David Foster Wallace explored the fictional possibilities of tax-return examinations. In his unfinished novel set in an IRS facility near Peoria, Illinois, Wallace wrote:

> Living people do not speak much of the dull. Of those parts of life that are and must be dull. Why this silence? Maybe it's because the subject is, in and of itself, dull… only then we're again right back where we started, which is tedious and irksome. There may, though, I opine, be more to it…as in vastly more, right here before us all, hidden by virtue of its size.[63]

Plan of dissertation

Chapter 1 will address the ferment of chemical theory, entrepreneurial ambitions, international rivalries, and local scientific politics that, during the 1880s, made the reform of the nomenclature of organic chemistry seem both possible and urgent. As individual chemists developed their own conventions for naming new synthetic compounds according to their structural formulas, a pernicious synonymy quickly spread across the field of organic chemistry. Many chemists complained that someone would have to do something to rein in these confusing synonyms; few were willing to take up the task themselves. However, Charles

[63] David Foster Wallace, *The Pale King: An Unfinished Novel* (New York: Back Bay Books, 2012), 87 (ellipses in original).

Friedel, professor of organic chemistry at the Sorbonne, saw chemical naming as an opportunity as well as a challenge. Friedel and a group of French colleagues took up nomenclature reform as a means of advancing French chemical science and industry and furthering the cause of their own faction within Parisian intellectual politics.

Chapter 2 addresses the result of Friedel's reform effort: the Geneva Congress for the Reform of Chemical Nomenclature. In April of 1892, Friedel gathered about three dozen of Europe's most prominent organic chemists in Geneva for four days of intensive discussion of chemical nomenclature. Friedel's goal was to develop a flexible system of nomenclature especially suited to the needs of chemical pedagogy. At the Geneva Congress, however, the German chemist Adolf von Baeyer contended that reform was most urgently needed in the compilation of chemical indexes and reference works. To this end, Baeyer advocated developing a set of rules to map the pattern of atoms and bonds represented in a structural formula into verbal form. This novel approach to chemical naming, forged in the intimate and collegial setting of the congress, set the model for nomenclature and identity in the molecular world.

Chapter 3 turns to chemical reference publications and to the editors whose job it was to channel the flood of chemical substances and information into a form tractable for compilation. The Geneva nomenclature was designed for just this task, but influential editors such as the German Paul Jacobson and the American Austin Patterson found it to be the wrong tool for the job. Instead, they turned to molecular formulas, numbering systems, and naming schemes riddled with exceptions. In very different contexts – Jacobson at the nerve center of German chemistry in Berlin, Patterson from his home in the hamlet of Xenia, Ohio – these editors sought to establish the literature of their respective national chemical societies as an authoritative guide to chemistry for the entire world.

Chapter 4 surveys how chemists across Europe and America adopted, adapted, or avoided systematic nomenclature outside of the pages of reference works. The qualities that went into making a good systematic name often diverged from those that were valued when it came to chemical pedagogy, patents, and products. However, from textbooks to laboratories to lawsuits, this divergence was itself productive. The chemotherapy pioneer Paul Ehrlich, executives at the chemical firm Bayer, and a wide variety of chemical instructors all used systematic nomenclature as a counterpoint against which they defined alternative approaches to naming, invested with their own epistemic, ethical, and economic values.

Chapter 5 addresses the emergence of new international chemical organizations concerned with nomenclature and index-making, and how these organizations transformed during World War I and its aftermath. The International Association of Chemical Societies came together in 1910 around nomenclature reform as a challenge best handled by a transnational elite. The events and rhetoric of the opening months of World War I shattered the notion of such a community. Over the course of the war, chemists from the nations of the Entente decided that the prodigious German chemical reference literature was the Teutonic trick behind the prestige of German chemistry. Wartime inter-allied cooperation laid the groundwork for a postwar organization, the International Union of Pure and Applied Chemistry (IUPAC). Once again, nomenclature and compilation were among the organization's signal tasks, but this time, the former Central Powers were not invited.

Chapter 6 follows the work of the nomenclature commissions convened by the fledgling IUPAC during the 1920s. Internationalists such as Patterson and his mentor William A. Noyes insisted that the Union's exclusion of Germany was incompatible with the effective development of systematic nomenclature. They also contended that nomenclature reform could provide the basis for rapprochement that would help prevent a future war. Under the

leadership of the Dutch chemist Arnold Holleman, IUPAC's commission on organic chemical nomenclature sought to establish a mechanism for negotiation among the German and American institutions that dominated the field of chemical information. The systematic nomenclature of organic chemistry that took root during the 1930s was less a matter of ordering molecules and more a means of maintaining a particular order among chemists. Nomenclature became the keystone of an ever-broader and ever-less-visible infrastructure of chemical information, built and maintained by a new kind of chemical expert.

Chapter 1

"The language itself must be transformed": Nomenclature Reform between German Chemical Synonyms and French Chemical Theories

When the Dutch chemist P.E. Verkade, longtime chair of an international commission on organic chemical nomenclature, decided to write a series of essays on the historical development of this field, he began his account in the year 1889. The eight years beginning in 1889, he explained, were "a very important period, since the foundations of our current nomenclature were laid down during this epoch."[1] But why *this* epoch? Why was it that an international effort to reform the nomenclature of organic chemistry arose in the late 1880s, and not before or after?

During the late nineteenth century, European scholars and scientists in a wide variety of fields organized congresses to address terminology, measurement practices, and other challenges of coordination across national and linguistic boundaries. However, this does not explain why organic chemical nomenclature became the subject of such a congress. Chemists had been complaining about the confusing, inconsistent names of organic compounds for nearly a century without ever regarding them as particularly vexing or proposing an international project to impose order upon them.

The disorder of organic chemical nomenclature was a problem born of scientific and commercial success. Between the 1860s and the 1880s, the science of organic chemistry and the synthetic dye industry grew rapidly, especially in the German states. The interconnected development of chemical science and industry drew upon on a collection of principles for

[1] Pieter Eduard Verkade, *A History of the Nomenclature of Organic Chemistry*, trans. S. G. Davies (Boston: Reidel, 1985), 18.

explaining chemical phenomena, structure theory, and a method of chemical experiment, the synthesis of long series of related compounds. Chemists engaged in such experimental programs developed ad hoc conventions for naming new synthetic compounds in keeping with the principles of structure theory. The result was a proliferation of chemical synonyms, each purporting to express a shared conception of chemical structure, but all doing so in different and often inconsistent ways. Organic nomenclature came to be seen as an urgent, solvable problem through the naming practices of German chemists who treated nomenclature as if it were no problem at all.

Unlike many of his German colleagues, the Alsatian chemist Charles Friedel did not carry out synthetic studies on retainer to the firms of a robust and growing dye industry. Rather, he labored in Paris to make a place for structure theory within French science and industry, whose stagnation many blamed for the nation's disastrous defeat at the hands of the Germans in the 1870-71 Franco-Prussian War. Friedel saw the confusion of German chemical synonyms as an opportunity to facilitate the translation of German chemical names into French, and thus German chemistry into France.[2] The habits Friedel formed and the institutions he built over three decades in Paris gave him the means and motivation to launch an international project to reform organic chemical nomenclature.

German synonyms made the reform of organic chemical nomenclature conceivable and desirable; Parisian scientific politics set the stage for reform on an international scale. This dual heritage shaped the events of the Geneva Congress, the subject of the next chapter. It also sheds

[2] For the sake of intelligibility, I have translated French and German chemical names, affixes, roots and orthography into the corresponding forms used in contemporaneous English. In doing so, I do not mean to suggest that chemical names – even unique, systematic ones – function equivalently in French, German, English, and other linguistic forms and contexts. In my judgment, the benefits of working within a single language outweigh the mediating effects of translation. I have avoided updating period terminology, except in the case of *corps*, which I have translated as "compound" or "substance" rather than "body," which suggests a less chemical and more antiquated concept in modern English than it would have in period French, and *noyau*, which I have translated alternatively as "ring" or "core" rather than the literal "nucleus," to avoid confusion with the atomic nucleus, a concept which had yet to emerge in 1892.

light on the nature of late nineteenth-century scientific internationalism, the standardization efforts central to it, and the local intellectual and institutional conditions out of which international standardization projects emerged.

Waiting for Lavoisier

Though chemists began to consider the state of organic chemical nomenclature as a serious problem during the 1880s, they had long perceived this as a particularly disorderly area of their science. The sense that something was awry in the naming of organic chemical substances was founded on a comparison with practices of nomenclature in the other branch of their science, inorganic chemistry.

Inorganic compounds were named according to the composition-based method introduced by Lavoisier and Guyton.[3] Experiments (conducted according to Lavoisier's method and interpreted according to Lavoisier's theory) showed that the compound known as "oil of vitriol" was a combination of sulfur and oxygen, so it was renamed "sulfuric acid." "Glauber's salt," a combination of sulfuric acid and soda, was renamed "sulfate of soda." By indicating composition in this way, such names also expressed the position of each chemical substance within the composition-based classification system of Lavosier's new chemistry.

As chemists distinguished multiple compounds made up of different proportions of the same constituent elements, they introduced additional prefixes – "hypo," "per," "bi," "super" and "sub," along with Lavosier's suffixes "–ous" and "–ic" – to distinguish such compounds while retaining Lavoisier's binomial method.[4] In the 1810s, as accumulating experimental

[3] Louis Bernard Guyton de Morveau et al., *Méthode de nomenclature chimique* (Paris: Cuchet, 1787). Their fellow-travelers Claude-Louis Berthollet and Antoine de Fourcroy also wrote portions of the volume, but the bulk of the system was based on Morveau's previous efforts in binomial nomenclature, recast in terms of Lavoisier's oxygen theory; Maurice P. Crosland, *Historical Studies in the Language of Chemistry* (New York: Dover Publications, 1978), 133–224.

[4] On the elaboration of Lavoisier and Guyton's method of nomenclature during the nineteenth century, see Crosland, *Historical Studies*, 215–224.

evidence suggested that these combining proportions related to each other by whole-number ratios, the Swedish chemist Jöns Jacob Berzelius introduced notation for expressing such distinctions quantitatively. Assigning each element a one- or two-letter symbol (H for hydrogen, O for oxygen, and so on), Berzelius represented each compound by concatenating the symbols of its constituent elements, paired with numbers indicating their relative abundance. Though not universally accepted – William Whewell, a mineralogist as well as a mathematician, complained that they played fast and loose with the conventions of algebra – Berzelian formulas such as H_2O (water), H_2SO_4 (sulfuric acid), Na_2SO_4 (sodium sulfate), and NaCl (sodium chloride) soon became ubiquitous.[5]

Like Lavoisier's method of nomenclature, Berzelian formulas expressed chemical identity as a function of composition. They did so in a synoptic manner especially convenient for describing and comparing compounds, as well as for calculating the identity and quantity of substances generated during an experiment but difficult to measure directly.[6] However, there was no agreed-upon way of determining the *absolute* proportions in which elements combined to make up each compound.[7] For example, following different empirical and philosophical considerations, some chemists took the ratio of the masses of the simplest combining units of hydrogen, oxygen, and carbon to be 1:6:8; others assumed relative masses of H=1, C=12, and O=16. Chemists who adopted these different systems, known as "equivalent weights" and "atomic weights," respectively, wrote different formulas for the same substance. To the latter,

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[5] Ursula Klein, *Experiments, Models, Paper Tools: Cultures of Organic Chemistry in the Nineteenth Century* (Stanford: Stanford University Press, 2003), 86–117.

[6] Ibid., 118–148.

[7] To determine the relative weight of two different elements, it was necessary to compare equal amounts of each element. However, the most reliable way to determine the relative amounts of samples of two elements was to weigh them. This circular reasoning could be escaped by means of various assumptions about the relationship between quantity and volume in a gas, about the ratios in which elements combined to form compounds, and about the physical micro-structure of certain elements, but convincing experimental tests of these assumptions were hard to come by. See Alan J. Rocke, *Chemical Atomism in the Nineteenth Century: From Dalton to Cannizzaro* (Columbus: Ohio State University Press, 1984).

water was H_2O; to the former, it was HO.[8] Alongside their Berzelian formulas, therefore, chemists continued to rely on binomial names – and on Lavoisier's method for generating them – to reliably identify inorganic compounds.

Guyton and Lavoisier had not attempted to apply their method to chemical substances derived from plants and animals. Though Lavoisier and some of his contemporaries did study such substances, they were generally most interested in their behavior within living organisms. In any case, these compounds stubbornly resisted the methods of isolation and analysis by which chemists determined the composition of inorganic substances.[9]

Between 1820 and 1840, this changed. Armed with more precise instruments, methods, and comparative data, chemists began to turn their techniques for isolating and analyzing inorganic chemical substances upon materials derived from plants and animals. With two ambitious young chemists leading the way – the German Justus Liebig and the Frenchman Jean-Baptiste Dumas – chemists began to study organic chemical substances in the same manner as mineral compounds. The laboratory synthesis of relatively simple organic compounds indistinguishable from materials derived from nature confirmed the fruitfulness of this unified approach. By 1840, organic chemistry, recast as the chemistry of carbon compounds, was established as the fastest-growing field of chemical scholarship in Europe.[10]

Berzelian formulas proved extremely useful in planning organic chemical research and in interpreting its results in creative and sometimes speculative ways.[11] The binomial nomenclature, however, offered little help in naming organic chemical substances. While

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[8] Ibid.; Hasok Chang, *Is Water H_2O?: Evidence, Realism and Pluralism* (New York: Springer, 2012).

[9] Klein, *Experiments, Models, Paper Tools*, 41–65. On Lavoisier's analytical method, see Frederic Lawrence Holmes, *Antoine Lavoisier—The Next Crucial Year: Or, the Sources of His Quantitative Method in Chemistry* (Princeton: Princeton University Press, 1998).

[10] Klein, *Experiments, Models, Paper Tools*, 66–85; Alan J. Rocke, *Nationalizing Science: Adolphe Wurtz and the Battle for French Chemistry* (Cambridge, MA: MIT Press, 2001), 11–71; J. B. Morrell, "The Chemist Breeders: The Research Schools of Liebig and Thomas Thomson," *Ambix* 19, no. 1 (1972): 1–46.

[11] Klein, *Experiments, Models, Paper Tools*.

inorganic compounds could be neatly classified according to which among the of dozens of simple substances they contained, organic compounds were all composed of the same few elements – mostly carbon and hydrogen, some oxygen and nitrogen, and occasionally sulfur. This made their composition both more difficult to determine and less informative than that of an inorganic compound like sodium chloride. Even worse, some substances with different chemical properties had precisely the same composition: *isomers*, Berzelius termed such chemical twins. For organic chemical substances, a method of nomenclature based on composition alone would not do. Chemists accordingly retained and coined names for organic compounds based on their sources or properties – the sort of trivial names that Lavoisier had banished from the lexicon of inorganic chemistry. A substance isolated from ants (Latin: *formica*) became "formic acid"; a reddish-brown oil (Greek: *pyros*, ruddy + *oleum*, oil), "pyrrol"; the product of the *dehyd*rogenation of *al*cohol, "aldehyde."[12]

Seeking to explain isomerism, the relationship between starting materials and products of chemical reactions, and patterns of physical and chemical properties among different organic compounds, chemists turned from composition to constitution. From 1830 through 1860, Berzelius, Liebig, Dumas, and younger colleagues including Auguste Laurent and Charles Gerhardt developed various theories of chemical constitution, seeking a unified explanation of organic chemical phenomena.[13] All of these chemists adapted Berzelian formulas to express and in some cases to help formulate their conceptions of the inner organization of compounds and of relationships between them. Few claimed that their formulas were literal representations of the physical structure of compounds at the micro-scale, but most

[12] Crosland, *Historical Studies*, 285–298. For more such etymologies, see W. E. Flood, *The Origins of Chemical Names* (London: Oldbourne, 1963); Edmund O. von Lippmann, *Zeittafeln zur Geschichte der Organischen Chemie* (Berlin: J. Springer, 1921).

[13] Alan J. Rocke, *The Quiet Revolution: Hermann Kolbe and the Science of Organic Chemistry* (Berkeley: University of California Press, 1993), 49-54; Alan J. Rocke, *Image and Reality: Kekulé, Kopp, and the Scientific Imagination* (Chicago: University of Chicago Press, 2010), 1-37.

nonetheless acted as if compounds were composed of chemically indivisible units clustered and connected in patterns to which an adequate formula should correspond.[14]

Some chemists experimented with schemes of rational nomenclature that reflected their theories as well. Laurent's theory of substitution located chemical identity primarily in a compound's "fundamental radical," the hydrogen atoms of which might be replaced by any number of other atoms or groups. He developed a corresponding method of nomenclature in which the root of a compound's name indicated its nucleus, each substituent was identified by a prefix, and the degree of substitution was expressed by the vowel of a suffix. Gerhardt used his classification of compounds in "homologous series" differing by units of CH_2 as the basis of a binomial nomenclature, appending a prefix indicating the number of CH_2 units in a particular compound to the name of the chemical "type" that defined the series.[15] Notwithstanding their differing epistemic commitments – Laurent considered his formulas representations of the real structure of compounds, whereas Gerhardt treated them as bookkeeping devices for empirical chemical reactions – both turned to rational names as a means of classifying compounds according to their theories of constitution.[16]

However, the general disagreement among organic chemists over matters of constitution and classification during this period discouraged chemists from proposing such conventions, or from adopting the ones that were proposed. By the late 1850s, the divergence among systems of element weights, conventions of formula notation, and nomenclature was so dire that chemists could barely understand, let alone adjudicate, their varying claims regarding

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[14] Alan Rocke has termed this mode of analogical reasoning "chemical atomism." Rocke, *Chemical Atomism in the Nineteenth Century,* 12. On chemical and physical atomism, see also Hans-Werner Schütt, "Chemical Atomism and Chemical Classification," in *The Cambridge History of Science*, ed. Mary Jo Nye, vol. 5 (Cambridge: Cambridge University Press, 2003), 237–254; Mary Jo Nye, ed., *The Question of the Atom: From the Karlsruhe Congress to the First Solvay Conference, 1860-1911: A Compilation of Primary Sources* (Los Angeles: Tomash Publishers, 1984).

[15] Crosland, *Historical Studies*, 308–314.

[16] John Hedley Brooke, "Laurent, Gerhardt, and the Philosophy of Chemistry," in *Thinking about Matter: Studies in the History of Chemical Philosophy* (Brookfield, VT: Variorum, 1995), chapter 7.

chemical composition, structure and behavior. The young German chemist August Kekulé illustrated the confusing state of affairs with a table listing eighteen formulas for one of the simplest and best-known organic compounds, acetic acid (*Fig 1*).

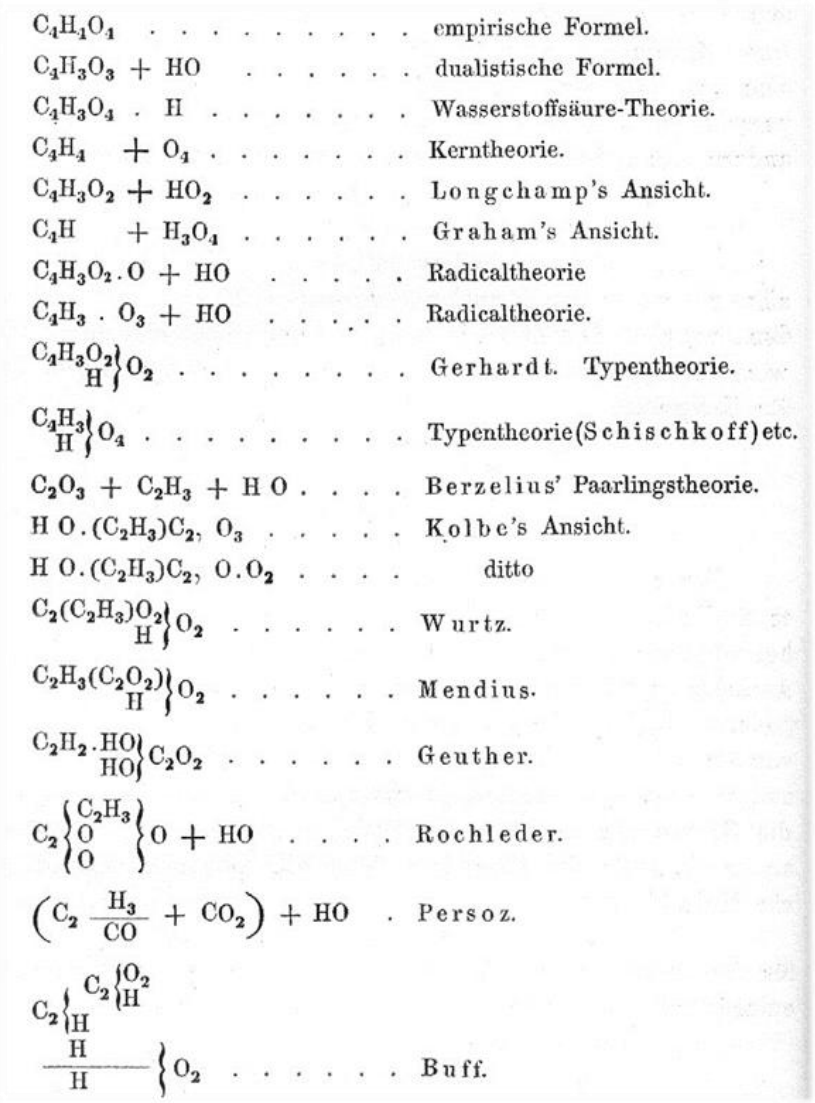

Figure 1. Kekulé's table of nineteen competing formulas for acetic acid (vinegar).[17]

Kekulé felt the time was ripe to try to do something about his disorder. Aided by his French colleague Adolphe Wurtz, he organized an international congress in Karlsruhe in 1860 to try to address some of the most vexing sources of confusion. Nomenclature was on the

[17] August Kekulé, *Lehrbuch der organischen Chemie* (Erlangen: F. Enke, 1861), 58.

agenda, but the 140 attendees of the Karlsruhe Congress were more concerned about establishing definitions for terms like "atom" and "molecule," and adopting a standard system of element weights (or at least adopting different symbols for atomic weights and equivalents). Even in this limited area, the Congress was only moderately successful. The Italian delegate Stanislao Cannizzaro delivered a magisterial address in favor of the system of atomic weights favored by Kekulé and Wurtz; this may have accelerated a general shift toward the atomic weights, especially among the younger chemists present at Karlsruhe.[18] But with many delegates skeptical that chemical usage could be legislated, and without a mechanism for enforcing any decisions that they decided to take, the Congress ended up agreeing upon only a few limited resolutions that had little direct influence on practice.[19]

The Karlsruhe Congress never got to nomenclature at all. Though rational conventions such as those of Laurent and Gerhardt did catch on in certain limited cases, in 1860, the nomenclature of organic compounds was by and large still stocked with names based on the origin or an arbitrary property of a compound. Chemists continued to worry more about formula notation, which could be actively misleading, more than idiosyncratic names, which at least generally sufficed to identify the compound to which they referred.[20] In the area of formula representation even more than in that of nomenclature, the statement of the author of

[18] Dmitri Mendeleev, for example, cited Cannizzaro's speech as a formative event for his eventual formulation of the periodic table nine years later; Michael D. Gordin, *A Well-Ordered Thing: Dmitrii Mendeleev and the Shadow of the Periodic Table* (New York: Basic Books, 2004), 4.

[19] On the Karlsruhe Congress, see Rocke, *Nationalizing Science*, 226–233; Bernadette Bensaude-Vincent, "Karlsruhe, Septembre 1860: L'atome en congrès," *Relations internationales* 62 (1990): 149–169; Bernadette Bensaude-Vincent, "Languages in Chemistry," in Nye, *The Cambridge History of Science*, vol. 5, 174–190.

For an English translation of the congress minutes, see "Carlsruhe Compte Rendu [1860]," in Nye, *The Question of the Atom,* 5-28.

[20] As Crosland puts it, "although chemical names were often badly chosen, their meanings were not in dispute." Crosland, *Historical Studies*, 296.

a French chemical lexicon published in 1845 remained true: "Organic chemistry still awaits its Lavoisier."[21]

Structural organic chemistry in Germany

Beginning in the mid-1850s, Kekulé, Wurtz, and a few other imaginative chemists drew together some of the experimental findings and conceptual insights of the preceding decade. They formulated several principles that appeared to govern the structure of organic chemical compounds in a manner that effectively accounted for many different chemical phenomena and relationships. These included *atomism*, the assumption that substances were made up of chemically indivisible atoms whose relative weights could be derived by means of certain physical measurements and laws; *substitution*, which divided molecules into a core subunit and one or more "substituent" atoms or groups of atoms in place of the hydrogen atoms of a "parent compound"; *valence*, the concept that atoms of each element formed a characteristic number of bonds to other atoms within compounds; *the tetravalence of carbon*, the rule that each carbon atom formed four bonds; and *self-linking*, the ability of atoms of the same element – especially carbon – to bond to one another. As more and more chemists adopted an approach to chemical research founded in this collection of principles, they gave it a name: structure theory.[22]

Structure theory offered numerous advantages. It provided an explanation for the number of isomers observed in certain cases, and predicted isomers yet to be discovered. It

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[21] Jean Chrétien Ferdinand Hoefer, *Nomenclature et classifications chimiques* (Paris: Baillière, 1845), vii.

[22] The allocation of credit for the origins of structure theory among Kekulé, the Scot A.S. Couper, the Russian Alexander Butlerov, and several other chemists has been the subject of much debate. Alan Rocke has explored this history and historiography in particularly illuminating detail. Rocke argues convincingly that structural organic chemistry is best understood as the collective product of the experimental demonstrations and conceptual insights of a number of imaginative chemists (among whom Kekulé looms large) who drew on each other's ideas to construct a productive vision of the chemical micro-world during the 1850s and 1860s. Alan J. Rocke, "Kekulé, Butlerov, and the Historiography of the Theory of Chemical Structure," *British Journal for the History of Science* 14, no. 1 (1981): 27–57; Alan J. Rocke, "The Theory of Chemical Structure and Its Applications," in Nye, *The Cambridge History of Science*, vol. 5, 255–271; Rocke, *The Quiet Revolution*; Rocke, *Nationalizing Science,* 155-212; Rocke, *Image and Reality*, 1–66. See also Brooke, "Doing Down the Frenchies: How Much Credit should Kekulé Have Given?," in Brooke, *Thinking About Matter*, chapter 10.

unified two sorts of chemical relationship, substitution and homologous series, into a single mode of chemical classification.[23] And it provided the basis for a particularly effective genre of formula representation. In the early 1860s, Scottish chemist Alexander Crum Brown adopted a style of graphic notation that expressed the inner chemical structure or "constitution" of compounds in accordance with these principles.[24] Representing atoms with element symbols like C, H, and O and chemical associations between them with lines, these "structural formulas" soon caught on among chemists sympathetic to structure theory. Some chemists criticized the use of such diagrams as "pictorial" chemistry that suggested arrangements of atoms within compounds in an illegitimate or inaccurate manner.[25] Even the proponents of structural formulas were careful to note that they should not be interpreted as suggesting the physical positions of atoms in molecules, but only empirical relationships among chemical subunits.[26] Nevertheless, they employed the formulas as a productive aid to chemical reasoning and

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[23] Broadly speaking, chemists in the second half of the nineteenth century conceived of two sorts of relationship among chemical substances. The first, associated with the French chemist Auguste Laurent, were relations of *substitution*: substances that shared a common *noyau* or internal structure to which different functional units or "substituents" were attached. The second, associated with Laurent's collaborator Charles Gerhardt, were relations within a *homologous series*: compounds that featured the same functional group, but whose *noyaux* contained different numbers of carbon atoms. Homologues had similar chemical behavior; compounds related by substitution could be transformed into each other relatively easily. Together, these two relations formed a sort of two-dimensional grid of organic chemical classification; this made systematic organic chemical nomenclature a different enterprise than nomenclature built around a hierarchical classification, as in biological systematics. See Brooke, "Laurent, Gerhardt, and the Philosophy of Chemistry"; Satish C. Kapoor, "The Origins of Laurent's Organic Classification," *Isis* 60, no. 4 (1969): 477–527; Rocke, *The Quiet Revolution*, 84–107.

[24] Crum Brown likely drew inspiration from the diagrams used by his countryman A.S. Couper in 1858; many, including Wurtz, saw them as extensions of the type formulas in use for about a decade. On the graphical formulas of Couper, Crum Brown, and others, see Rocke, *Image and Reality,* 118–160.

[25] Some critics, like the British chemist Benjamin Brodie, rejected the use of graphical formulas entirely, while others, like the German Hermann Kolbe, rejected some of the principles of structure theory, and thus found structural formulas misleading. On Kolbe, see Rocke, *The Quiet Revolution*. On Brodie, see Stephen T. Irish, "Brodie's Calculus and Chemical Classification," *Ambix* 60, no. 3 (2013): 234–254.

[26] As Alan Rocke has argued, most chemists who used structural formulas argued publicly that they were justified by their heuristic usefulness in expressing relationships among compounds and should not be taken to suggest the real micro-structure of substances. Nevertheless, many of the most productive chemists of the second half of the nineteenth century did seem to have used visualizations of their formulas as three-dimensional physical entities in their chemical reasoning; Rocke, *Image and Reality*.

On the development of stereochemistry – the explicit study of chemical substances in terms of their physical, three-dimensional shape – see Peter J. Ramberg, *Chemical Structure, Spatial Arrangement: The Early History of Stereochemistry, 1874-1914* (Burlington, VT: Ashgate, 2003).

teaching. Chemists drew structural formulas in different ways; interpreted according to the principles of structure theory, however, they were mutually comprehensible. (*Fig. 2*).

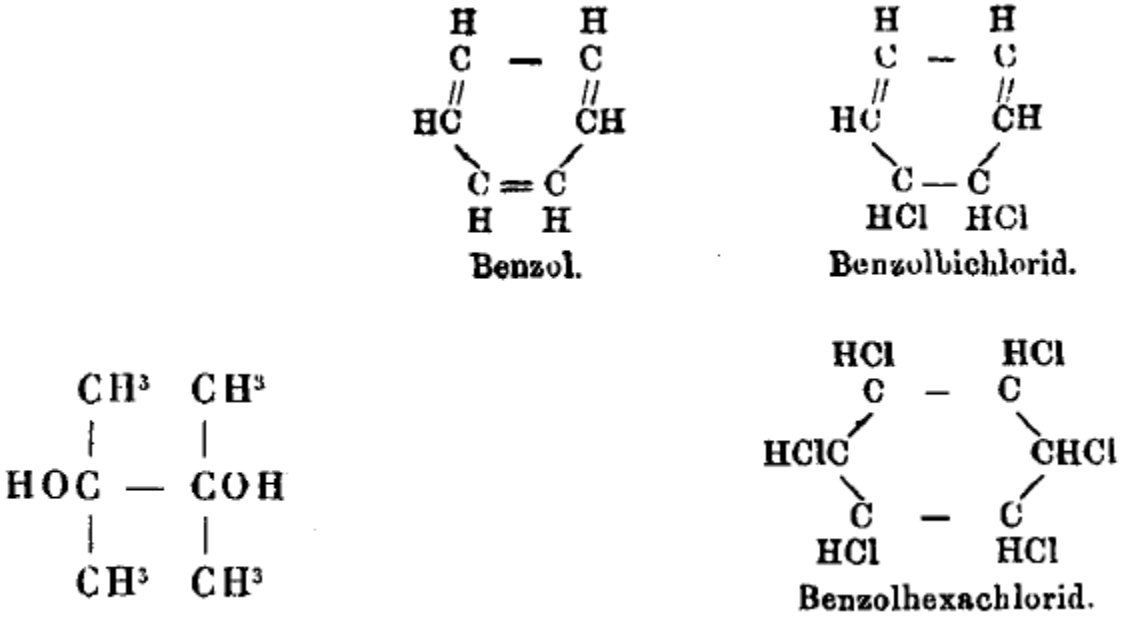

Figure 2. Structural formulas used by Geneva delegates Charles Friedel and Carl Graebe in articles of 1869 and 1868, respectively.[27]

Structure theory also offered plenty of open questions, many of which emerged through the study of valuable natural products. Did elements have a fixed or a variable valence? Was benzene, the C_6H_6 moiety found so frequently in organic compounds, structured as a hexagon, as Kekulé hypothesized in 1865, or in some other manner?

Structure theory developed in large part through, and in the service of, experiments in which chemists explored such constitutional questions using techniques of synthesis. The chemists who carried out such investigations, such as August Wilhelm Hofmann, director of the Royal College of Chemistry in London, hoped eventually to discover artificial methods of

[27] Charles Friedel, "Recherches sur les acétones et sur les aldéhydes," *Annales de la chimie et la physique* 16 (1869): 397; Carl Graebe, "Ueber die s. g. Additionsproducte der aromatischen Verbindungen," *Annalen der Chemie* 146, no. 1 (1868): 68. Those drawing structural formulas often grouped a carbon atom with the atoms bonded directly to it, e.g. CH_3, CO, or CO_2H, or omitted the carbon "C" in rings.

producing such compounds as indigo, morphine, and quinine.[28] However, the proximate goal of these "synthetical experiments" was not typically to prepare a singular target molecule, but to solve particular constitutional puzzles or determine the transformations effected by specific reactions.[29] This involved a shotgun approach: submitting numerous compounds to parallel reactions, and examining the myriad synthetic products that resulted. Some of these novel substances – signally, Hofmann student William Perkin's mauve – turned out to be valuable dyes in their own right, and the synthetic dye industry that sprang up around these discoveries generated new constitutional puzzles for organic chemists to tackle.[30] Such developments encouraged chemists to expand their synthetic studies, panning the byproducts of their studies of reactions and constitution for chemically interesting or commercially valuable synthetic nuggets.[31] As early as 1864, even a chemist skeptical of the trend could write, "It cannot be denied that, by the acceptance and the development of the atomic theory...[i]t was now most important for chemists to prepare, study, and classify as large a proportion of those compounds the existence of which was predicted by the atomic theory."[32]

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[28] In 1868, Carl Graebe and Carl Liebermann achieved the first commercially significant natural product synthesis. Their preparation of alizarin, the primary colorant in madder dye, was a pivotal episode in the development of the synthetic dye industry. Anthony S. Travis, *The Rainbow Makers: The Origins of the Synthetic Dyestuffs Industry in Western Europe* (Bethlehem: Lehigh University Press, 1993), 163–90.

[29] As Catherine Jackson has shown, Hofmann introduced "synthetical experiments" of this sort during the early 1840s and continued to rely on this mode of research for the rest of his career. Jackson argues that chemists who first succeeded in "constructive synthesis" – the deliberate construction of complex target molecules – during the 1880s did so by means of skillful and innovative laboratory practice, not because of special theoretical insights or particular uses of formula diagrams. Catherine M. Jackson, "Synthetical Experiments and Alkaloid Analogues: Liebig, Hofmann and the Origins of Organic Synthesis," *Historical Studies in the Natural Sciences* 44, no. 4 (2014): 319–363; Catherine M. Jackson, "The Curious Case of Coniine: Constructive Synthesis and Aromatic Structure Theory," in *Objects of Chemical Inquiry*, ed. Ursula Klein and Carsten Reinhardt (Sagamore Beach, MA: Science History Publications/USA, 2014), 61–102.

[30] Anthony S. Travis, "Science's Powerful Companion: A. W. Hofmann's Investigation of Aniline Red and Its Derivatives," *British Journal for the History of Science* 25 (1992): 27–44.

[31] Beginning in the 1870s, dye firms began to establish their own research laboratories where they specifically pursued the discovery of such compounds; Travis, *The Rainbow Makers*, 209–230. Physical chemists invoked the prospecting metaphor in critiquing this mode of organic chemical research; John W. Servos, *Physical Chemistry from Ostwald to Pauling: The Making of a Science in America* (Princeton: Princeton University Press, 1990), 64.

[32] Lothar Meyer, quoted in Ibid., 10.

These broad, comparative synthetic programs demanded considerable material resources, organization, and skilled chemical labor. From the 1860s through the 1880s, the conditions for such studies were by far most favorable in German universities, where professors of chemistry worked in well-equipped new laboratories flush with students ready to take up research projects.[33] With no central authority setting curricular standards, these professors could frame their research and instruction as they saw fit; most chose to predict, assess, and express the results of their synthetic studies using structural formulas. The diagrams were at once an expedient means for making claims regarding the real constitution of a new synthetic product – chemists often simply combined the structural formulas of starting materials to illustrate the constitution of such compounds – and convenient bookkeeping devices for keeping track of the expanding taxonomy of organic chemistry.

Adolf Baeyer was one of the beneficiaries of the expansion of German chemistry. In 1860, after completing a PhD with Kekulé, Baeyer began teaching chemistry at the *Königliches Gewerbe-Institut* (Royal Trade Institute), Berlin's primary site of chemical research until the University of Berlin convinced Hofmann to return to Germany in 1865, in part through the construction of a new chemical institute.[34] In 1872, Baeyer was called to the University of Strasbourg, which German authorities had reorganized after the annexation of the Alsatian city the year before. Baeyer set his sights on the valuable natural dyestuff indigo, synthesizing the closely related compound indole in 1866 and determining the structure of this subunit over the

[33] Jeffrey A. Johnson, "Academic Chemistry in Imperial Germany," *Isis* 76, no. 4 (1985): 500–524; Peter Borscheid, *Naturwissenschaft, Staat und Industrie in Baden (1848-1914)* (Stuttgart: Klett, 1976), 16–82; Rocke, *The Quiet Revolution*, 270–286; Rocke, *Nationalizing Science*, 392–397. Both before and after German unification, the chemical institutes of many Austrian and Swiss universities partook of the same academic culture, if not always such generous state sponsorship. Though most of these institutes focused on organic chemistry, Robert Bunsen build a research school at Heidelberg focused on inorganic topics; Christine Nawa, "A Refuge for Inorganic Chemistry: Bunsen's Heidelberg Laboratory," *Ambix* 61, no. 2 (2014): 115–140.

[34] Hofmann's move from London to Berlin marked a pivotal moment in the rise of German academic and industrial chemistry. Rocke, *The Quiet Revolution*, 272–273; Travis, *The Rainbow Makers*, 136–137.

next few years.[35] As he and his colleagues expanded their synthetic studies to elucidate the constitution of such compounds and elaborate the principles of structure theory, they came once again upon the problem of nomenclature.

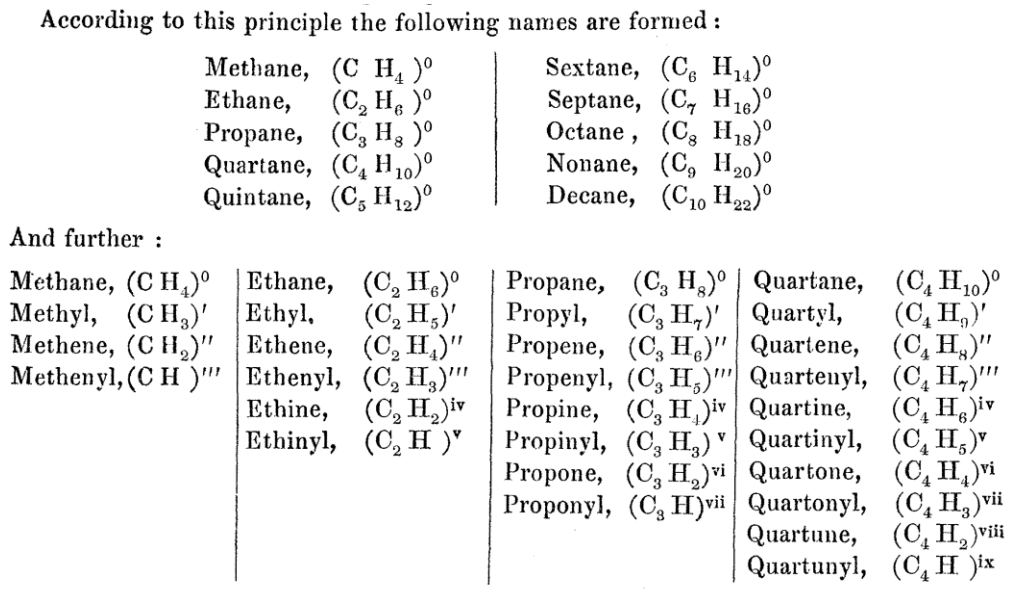
Chemical synonyms and the institutional ecology of German chemistry

During the 1860s, chemists continued sporadically to propose new conventions of rational nomenclature. Unlike those of their peers in preceding decades, the spread of the principles of structure theory provided a relatively consistent understanding of chemical constitution and classification by which these conventions could enter broad use. During the 1870s and 1880s, chemists grew more confident in deploying both structural formulas and conventions of rational nomenclature, turning to the latter particularly for compounds conceived as structural formulas before they were ever found in a test tube. As chemists in the decentralized, competitive field of German chemistry raced each other to synthesize new series of compounds in the decentralized, the practices of representation that each conceived and adjusted on the fly diverged. The shared graphical patterns, spare symbolic vocabulary and morphology, and restrictive typesetting requirements of structural formulas kept these visual representations mutually intelligible. The proliferation of different conventions for naming organic compounds, however, led by the 1880s to a serious state of confusion, only made worse by the basis of all of these names in a shared understanding of chemical constitution.

At first, conventions of rational nomenclature focused on simple compounds and chemical subunits. In an extended footnote to an 1865 article, Hofmann described a system of nomenclature that he had developed in order to communicate more clearly with his students, in the face of the "boundless confusion of names now prevailing in organic chemistry" (*Fig. 3*).

[35] Baeyer was compelled to put his research on hold in the early 1870s in deference to his former teacher Kekulé, who felt that he was on the verge of synthesizing the dye and determining its structure. In 1880, he synthesized indigo itself. Travis, *The Rainbow Makers*, 168–169, 220–227.

He focused on the hydrocarbons: chains of carbon units bonded to hydrogen. Because these compounds figured as central subunits in structural formulas, persisting across many chemical reactions, chemists often assigned them an important classificatory role in studies guided by structure theory. Hofmann combined elements of Gerhardt's and Laurent's nomenclature schemes, using numerical prefixes to indicate the number of carbon atoms in the compound and suffixes indicating its valence (the number of additional bonds it could form).[36] Few chemists adopted his names in a thoroughgoing manner, but the method slowly made its way into common use.[37]



According to this principle the following names are formed :

Methane,	$(C\ H_4)^0$	Sextane,	$(C_6\ H_{14})^0$
Ethane,	$(C_2\ H_6)^0$	Septane,	$(C_7\ H_{16})^0$
Propane,	$(C_3\ H_8)^0$	Octane ,	$(C_8\ H_{18})^0$
Quartane,	$(C_4\ H_{10})^0$	Nonane,	$(C_9\ H_{20})^0$
Quintane,	$(C_5\ H_{12})^0$	Decane,	$(C_{10}\ H_{22})^0$

And further :

Methane, $(C\ H_4)^0$	Ethane, $(C_2\ H_6)^0$	Propane, $(C_3\ H_8)^0$	Quartane, $(C_4\ H_{10})^0$
Methyl, $(C\ H_3)'$	Ethyl, $(C_2\ H_5)'$	Propyl, $(C_3\ H_7)'$	Quartyl, $(C_4\ H_9)'$
Methene, $(C\ H_2)''$	Ethene, $(C_2\ H_4)''$	Propene, $(C_3\ H_6)''$	Quartene, $(C_4\ H_8)''$
Methenyl, $(C\ H)'''$	Ethenyl, $(C_2\ H_3)'''$	Propenyl, $(C_3\ H_5)'''$	Quartenyl, $(C_4\ H_7)'''$
	Ethine, $(C_2\ H_2)^{iv}$	Propine, $(C_3\ H_4)^{iv}$	Quartine, $(C_4\ H_6)^{iv}$
	Ethinyl, $(C_2\ H)^{v}$	Propinyl, $(C_3\ H_3)^{v}$	Quartinyl, $(C_4\ H_5)^{v}$
		Propone, $(C_3\ H_2)^{vi}$	Quartone, $(C_4\ H_4)^{vi}$
		Proponyl, $(C_3\ H)^{vii}$	Quartonyl, $(C_4\ H_3)^{vii}$
			Quartune, $(C_4\ H_2)^{viii}$
			Quartunyl, $(C_4\ H)^{ix}$

Figure 3. Hofmann's hydrocarbon nomenclature. Superscript primes and Roman numerals refer to the valence of each chemical unit.[38]

As Hofmann's adaptation of Laurent and Gerhardt's approaches showed, nomenclature conventions did not have to have been conceived with structure theory in mind in order for

[36] August Wilhelm Hofmann, "Über die Einwirkung des Phosphortrichlorids auf die Salze der aromatischen Monamine," *Monatsberichte der königlich Preussischen Akademie der Wissenschaften zu Berlin*, 1865, 652–654. Quotations are taken from the English-language translation of the article that Hofmann published the following year; A. W. Hofmann, "On the Action of Trichloride of Phosphorus on the Salts of the Aromatic Monamines," *Proceedings of the Royal Society of London* 15 (1866): 54–62.

[37] Crosland, *Historical Studies*, 341–342.

[38] Hofmann, "On the Action of Trichloride of Phosphorus," 58.

chemists to make use of them in this way. One of the conventions taken up most widely during the 1860s and 70s was the work of Hermann Kolbe, one of the most stubborn and vocal critics of structure theory. He developed a scheme for naming alcohols according to the various hydrocarbon "radicals" that he argued they contained (*Fig. 4*). Notwithstanding his very different conception of constitution and formula notation, many chemists adopted this form of name to identify compounds in terms of the hydrocarbon subunits contained in their structural formulas.[39]

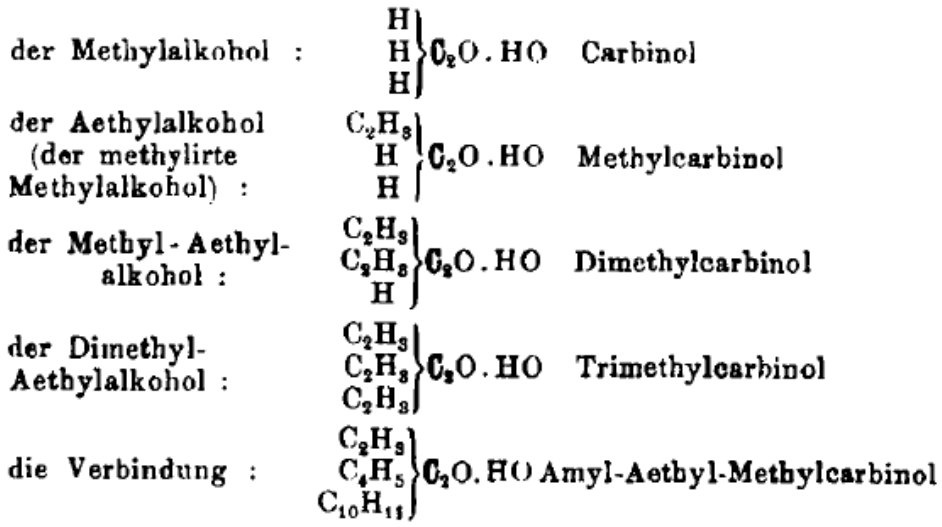


Figure 4. Kolbe's "carbinol" nomenclature.[40]

Few chemists, if any, adopted these conventions as rules governing the names that they chose. Rather, they took them up, as Hofmann wrote of his own use of his hydrocarbon scheme, as conventions to which they "occasionally resorted…as a means of communication."[41] By describing a collection of dye compounds that he investigated as derivatives of the

[39] Hermann Kolbe, "Ueber die secundären Alkohole," *Annalen der Chemie* 132, no. 1 (1864): 103–104n; Crosland, *Historical Studies*, 325–327.

[40] Kolbe, "Ueber die secundären Alkohole," 104.

[41] Hofmann, "On the Action of Trichloride of Phosphorus," 57.

hydrocarbon "triphenylmethane," for example, Emil Fischer was able to succinctly express a series of structural relationships by invoking generally-understood naming conventions.[42]

These conventions proved particularly useful in settings where structural formulas could not easily be applied. This was especially of interest to chemists seeking to take stock of the flood of new compounds emerging from German laboratories, often from positions outside of them. As the German chemical literature swelled with organic chemical research, the section of the British *Journal of the Chemical Society* dedicated to publishing abstracts of foreign articles grew. The structural formulas that filled these articles took up a lot of space and were expensive to typeset; editor Henry Watts determined that eliminating as many of them as possible provided a much-needed means of economizing. In 1879, Watts instructed his abstractors to omit the diagrams wherever they could be replaced by names formed according to a list of conventions like Kolbe's and Hofmann's.[43]

Friedrich Beilstein developed a goal more ambitious than simply keeping abreast of new research. In his position in St. Petersburg, Friedrich Beilstein was consumed by teaching duties and, unlike his equally-accomplished colleagues to the west, had no lab full of doctoral students to set to work on a research program. In response to the pedagogical challenge of his rapidly-changing field, Beilstein sought to write a textbook that would cast the heterogenous matter of structural organic chemistry into a consistent form. Determined to improve upon the incomplete and inaccurate data and citations that marred other textbooks, Beilstein exhaustively tracked down and catalogued a century's worth of literature on organic compounds. By the time Beilstein began to publish his work in 1880, the *Handbuch der organischen Chemie* had morphed into a unique reference work that spanned and delineated

[42] Emil Fischer and Otto Fischer, "Ueber Triphenylmethan und Rosanilin," *Annalen der Chemie* 194, no. 2–3 (1878): 242–303.

[43] Henry Watts, "Instructions to Abstractors," *Journal of the Chemical Society (Transactions)* 35 (1879): 276–281.

the entire material scope of organic chemistry. The *Handbuch* provided an overview and an index to the rapidly progressing field of organic chemistry, both in its accomplishments and its gaps.[44]

Nomenclature offered few accomplishments and a very large gap. Even in the case of those few conventions that had gained general purchase, Beilstein complained that the informal means by which chemists applied them had brought about untoward effects in the indexing of chemical literature. One journal's subject index, for example, contained four permutations of the name "chlornitrobenzene."[45] This was the converse of the problem that Kekulé had highlighted in his 1861 diagram *(Fig. 1)*. There, only the name "acetic acid" linked the disparate formulas by which chemists represented the constitution of this compound. Here, chemists held to the same views of a compound's constitution, but identified that constitution by means of four different rational names. The problem, Beilstein feared, would only grow worse. "With the numerous new names which are introduced into chemistry each year," he concluded, "it has become an urgent necessity to organize the nomenclature according to set rules."[46]

Rational names formed by the informal application of well-established conventions might have caused headaches for a chemist perusing and constructing indexes as closely as Beilstein, but these names were understandable. However, during the 1870s and 1880s, chemists began to deploy structural formulas to plan systematic synthetic investigations of entirely novel families of compounds, centered not on the long-familiar chemical units that had accrued rational nomenclature conventions over the preceding decades, but on exotic, newly

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[44] Michael D. Gordin, "Beilstein Unbound: The Pedagogical Unraveling of a Man and His Handbuch," in *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives*, ed. David Kaiser (Cambridge: MIT Press, 2005), 11–39.

[45] Two contained the prefix *mono-*. Friedrich Konrad Beilstein, *Handbuch der Organischen Chemie*, 1st ed., vol. 1 (Hamburg and Leipzig: Leopold Voss, 1881), vii.

[46] Ibid.

characterized structural subunits discovered in compounds used as dyes or pharmaceuticals and that, the chemists hoped, might be the key to even more valuable synthetic derivatives.

Scattered across the well-equipped new chemical institutes in universities and Technische Höchschule in Germany, Switzerland, and Austria, a growing collection of chemists raced to synthesize these series, aiming both to advance structure theory and, perhaps, to strike rich veins that would yield generous licensing fees. These compounds were structural formulas before they were substances in a test tube – they were born structural, often many at a time. Lacking familiar conventions to bring to bear on these compounds, but more confident than ever in the structural formulas that they have used to dream them up, these chemists developed their own conventions for naming these new synthetic compounds according to their structure.

For example, in 1882, a German chemist advocated naming new compounds containing the –N=N– "azo" subunit, then emerging as an important new class of dyestuffs, by inserting the term "azo" between the names of the chemical units bonded to either side of this group. Such a procedure "eases comprehension in the highest degree," he wrote, "because the names are constructed entirely according to the formulas."[47]

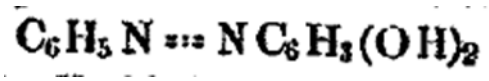

Figure 5. "Benzene-azo-resorcin."[48]

However, procedures that "eased comprehension" in relatively straightforward cases foundered when applied to more complex synthetic substances. For example, some chemists identified individual carbon atoms in the rings and chains of their structural formulas as sites

[47] K. Heumann, "Die Nomenclatur complicirter Azoverbindungen," *Berichte der Deutschen Chemischen Gesellschaft* 15, no. 1 (1882): 813–814.

[48] Ibid., 814.

of substitution by means of numbers; others used Greek letters. Either convention was comprehensible enough for a compound containing one carbon chain or ring. However, the varying application of these conventions for compounds containing multiple rings and chains led Baeyer to lament in 1884, "Over the nomenclature of organic compounds looms a confusion that makes it impossible even for the expert to understand the name of a substance without more information."

Baeyer proposed to "eliminate the greatest evils" of this confusion by recommending the assigning of numerals to rings only and Greek letters to open chains. However, he could not resist attaching yet another proposal as a rider: a collection of obscure and novel prefixes and notation to cover exceptional cases, including those in which a compound's structural formula was mostly but not entirely determined.[49] The competitive culture of German chemistry offered few laurels for a project of standardization; thus what Baeyer began as a proposal for unifying nomenclature conventions ended in a string of neologisms intended to generate rational names for a new class of compounds. It was an interesting plan, but one that tended to contribute more to the confusion in chemical naming than it solved.[50]

The most serious problems arose when procedures for naming the same series of compounds came into conflict. In 1888, Uppsala chemist Oskar Widman – another figure surveying the fruits of German chemistry from just beyond its borders – reviewed the disparate names that a dozen chemists had proposed for rings containing both carbon and nitrogen. Suspected to be important constituents of the alkaloids, a large class of naturally-occurring compounds including the valuable drugs morphine and quinine, Widman noted that these

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[49] Adolf Baeyer, "Zur chemischen Nomenclatur," *Berichte der Deutschen Chemischen Gesellschaft* 17, no. 1 (1884): 960–963.

[50] Ibid., 961. The pugnacious Hermann Kolbe, one of the last significant German chemists to oppose structure theory, minced no words in critiquing the complexity of Baeyer's proposals; Hermann Kolbe, "Adolf Baeyer, Reformator der chemischen Nomenklatur," *Journal für praktische Chemie* 29, no. 1 (1884): 501–504.

compounds were "currently studied with the greatest excitement."[51] However, Widman found it "very difficult to keep track of all of these nearly innumerable names, chosen according to different principles, for compounds that by their constitution are closely related to one another, or sometimes even identical."[52]

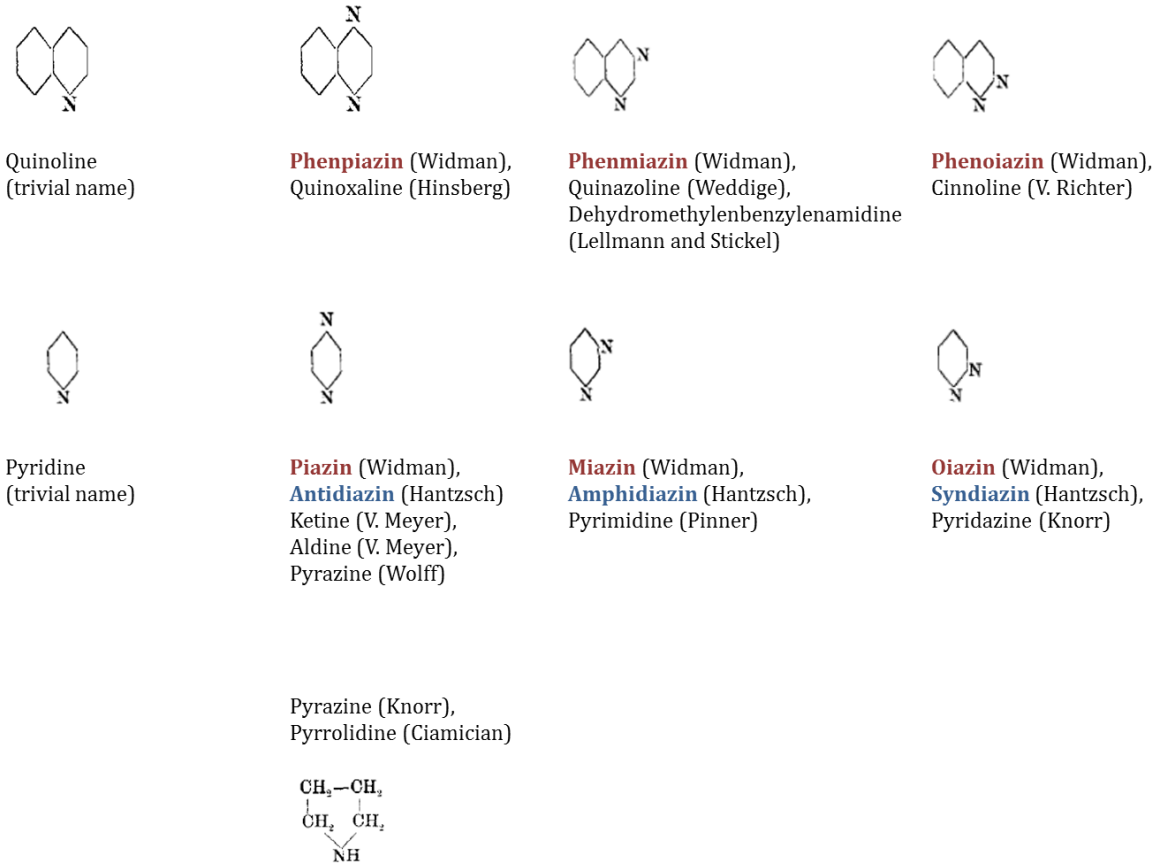

Figure 6. Competing rational names for some nitrogen-containing ring compounds, late 1880s. As was (and is) common, in all of these diagrams but one, unlabeled vertices of the structural formulas indicated carbon atoms, and hydrogen atoms bonded to carbon are omitted.[53]

[51] Oskar Widman, "Zur Nomenclatur der Verbindungen, welche Stickstoffkerne enthalten," *Journal für praktische Chemie* 38, no. 1 (1888): 186.

[52] Ibid., 186–87.

[53] Widman, "Zur Nomenclatur"; Arthur Hantzsch, "Zur Nomenclatur stereoisomerer Stickstoffverbindungen und stickstoffhaltiger Ringe," *Berichte der Deutschen Chemischen Gesellschaft* 24, no. 2 (1891): 3479–3488.

The institutional ecology of German chemistry had made it possible for a dozen chemists in different places to arrive at the same potentially valuable compounds from different directions, and to assign them names based on their own independently-formulated conventions for denoting the same structural features. Widman held a trump card favoring the names that he proposed: the authority of Beilstein, who had sanctioned his project and advised him on it.[54] Still, his scheme added one more convention of rational nomenclature to the long list that he had enumerated.

Three decades after the Karlsruhe Congress, German chemists had traded one kind of confusion in organic chemical representation for another. Using the considerable resources provided by German academic institutions and driven by the opportunities presented by the German dye industry, these chemists pursued systematic programs of structure determination and synthesis. Structure theory and structural formulas proved particularly effective at expressing what had been made and what could be made. German chemists, who enjoyed great deal of autonomy in how they pursued their teaching, research, and education in the decentralized German university system, gravitated toward this useful notation, bringing order and consistency to graphical formula representation. However, this autonomy and shared understanding of chemical constitution were the basis for the emergence of confusion in the naming of organic compounds. As chemists developed their own conventions for expressing structural relationships in words, the comprehensible disorder of trivial names gave way to the orderly confusion of competing rational synonyms, a confusion made worse by the common basis of these different names in a shared understanding of constitution. As the gap between the interrelationships elegantly revealed by structural formulas and the obscure and confusing

[54] Widman, "Zur Nomenclatur," 185, 201.

proliferation of names widened, the frustration of chemists like Widman who sought to compare different chemists' investigations of similar compounds grew.

The autonomy of chemists working in German settings and their comfort with structure theory also contributed to the character of nomenclature reform proposals. As with the chemists whose methods they critiqued, chemists like Baeyer and Widman presented their proposals as individual projects. In formulating them, they took the legitimacy of the conception of chemical constitution expressed in structural formulas for granted, focusing their efforts on the disorderly outer reaches of chemical synthesis. As a result, their nomenclature reform proposals tended to be as idiosyncratic and obscure as the practices they sought to unify.

Nomenclature and the politics of French atomism

When Charles Friedel turned his attention to nomenclature in 1888, he did so from a distinctive perspective formed by his three decades in Parisian science. The conditions of scholarly life of a chemist in Paris were very different from those of German chemists during this period. The future Geneva Congress secretary Amé Pictet, who studied both with Kekulé in Bonn and Wurtz in Paris, recalled of Wurtz's laboratory: "What a difference from Dresden, Bonn, and even Geneva! It was as dark, as dilapidated, as barren of all comforts as could be." Wurtz assigned Pictet a project akin to those of the latest German organic chemical research, involving derivatives of two nitrogen-containing ring compounds, quinoline and lutidine. But there was room for Pictet in Wurtz's crowded and poorly-equipped laboratory only during the afternoon, and he made little progress, especially in comparison to the grueling year he had spent with Kekulé. Pictet also remarked upon the factionalized landscape of Parisian chemistry. On the one hand, Pictet made the acquaintance of many other chemists who had worked in Wurtz's laboratory, including Wurtz's protégé and eventual successor, Friedel. On the other,

Pictet wrote, "I was never at the Société Chimique, for there was complete dissention between Wurtz and Berthelot. … It was established that the students of the one never went to meetings when the other occupied the chair, and as it happened it was Berthelot that year."[55]

The features of Parisian chemistry that Pictet recognized during his brief sojourn there shaped the entire careers of Wurtz, Friedel, and a tight-knit group of their students and colleagues. French science and education was highly centralized, both geographically and administratively. Savants were public servants, their teaching and research subject to the authority of superiors who regulated science on behalf of the French state.[56] The hierarchical social structure and sheer concentration of scientists in Paris made the city a hotbed of controversy over matters of scientific theory, method, and organization throughout the nineteenth century.[57] Chemistry was no exception. Beginning in the 1860s, Wurtz, Friedel and their fellow proponents of structure theory locked horns again and again with Marcellin Berthelot and his powerful faction of chemists, who grounded their opposition to atomic weights and structural formulas in the methodological precepts of Comtean positivism.[58] At the same time, Wurtz and Friedel lobbied for the construction of improved laboratory facilities and the integration of chemical training and the insights of chemical research into French industry. Both arguments took on added urgency after the shock of the Franco-Prussian war, the

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[55] Amé Pictet, *Souvenirs et travaux d'un chimiste.* (Neuchâtel: Editions de la Baconnière, 1941), 39–42, 50–52, 104.

[56] Robert Fox, *The Savant and the State: Science and Cultural Politics in Nineteenth-Century France* (Baltimore: Johns Hopkins University Press, 2012).

[57] See, e.g., Toby A. Appel, *The Cuvier-Geoffroy Debate: French Biology in the Decades before Darwin* (New York: Oxford University Press, 1987); Alex Csiszar, *The Scientific Journal: Authorship and Knowledge in the Nineteenth Century* (forthcoming).

[58] As Alan Rocke and Danielle Fauque have shown, the use of this term can be misleading. In fact, Berthelot himself was usually quite happy to admit the existence of atoms; his quibble was with chemists who unjustifiably conflated what he called "chemical atoms" and "physical atoms." Rocke, *Chemical Atomism in the Nineteenth Century*; Danielle Fauque, "La réception de la théorie atomique en France sous le Second Empire et au début de la IIIe République," *Archives Internationales d'histoire des Sciences* 53 (2003): 64–112.

disastrous outcome of which many pinned on the decline of French science and industry in comparison with their counterparts in Germany.[59]

Friedel and Wurtz saw these as related battles. They pursued them not only within the state, but also in scientific societies that they founded, part of the para-political sphere of progressive associations that flourished under the French Third Republic.[60] From the debates over atomism and structure theory, Friedel and his allies gained sensitivity to matters of chemical language and a habit of situating particular chemical compounds within claims that spanned all of chemistry. From his institution-building activities, Friedel learned to pursue goals through collective action, ideally carried out by organizations with an impressive collection of names on their letterhead. During the 1880s, German chemists engaged in many small struggles to bring order to the nomenclature of different families of compounds. In the context of their ongoing battle to promote French science and industry and structure theory within it, Friedel and his allies were prepared to see the confusion in organic chemical nomenclature as one big opportunity.

In 1854, Charles Friedel entered the new chemical laboratory of Adolphe Wurtz at the École de Médecine in Paris. Wurtz, appointed professor of chemistry in the faculty of medicine the year before, had obtained permission to set up the laboratory in a few rooms previously dedicated to anatomy. The chemists who occupied these quarters, however, were not

[59] The putative decline of French science and industry during the mid-nineteenth century has been an object of much historiographic debate. For a detailed review of literature discussing the decline of French science and the question of scientific decline in general, see Rocke, *Nationalizing Science*, 397–419. See also Harry W. Paul, "The Issue of Decline in Nineteenth-Century French Science," *French Historical Studies* 7, no. 3 (1972): 416–450; Robert Fox and George Weisz, "The Institutional Basis of French Science in the Nineteenth Century," in Fox and Weisz, eds., *The Organization of Science and Technology in France, 1808-1914* (New York: Cambridge University Press, 1980), 1–28; Mary Jo Nye, *Science in the Provinces: Scientific Communities and Provincial Leadership in France, 1860-1930* (Berkeley, Calif: University of California Press, 1986); Fox, *The Savant and the State*.

[60] Janet R. Horne, *A Social Laboratory for Modern France: The Musée Social & The Rise of the Welfare State* (Durham, NC: Duke University Press, 2002).

physicians in training but private students and associates of Wurtz. The laboratory budget was paid out of their fees, and Wurtz's pocket.[61]

Like Friedel and Wurtz, many of these students were natives of the French province of Alsace. Abutting the Rhine just across the border from Karlsruhe, Alsace was linked culturally and linguistically as much to Germany as to Paris.[62] The region was home to a number of thriving industrial enterprises, especially in the fields of textile dying, dyestuffs, and pigments.[63] Strasbourg, the largest city in Alsace, was a cosmopolitan, liberal cultural crossroads. It was a haven for Protestants like Wurtz and Friedel, as well as for Jews. During the first half of the nineteenth century, the city was also known for its outstanding gymnasium and university, both strongly influenced by German models.[64] A disproportionate number of Alsatians were thus interested in chemical training and disposed to engage with German chemical literature and a German style of laboratory education. After the untimely death of Charles Gerhardt, another Strasbourg native, Wurtz's laboratory was the prime site in France for such studies. Foreign students, too, flocked to Wurtz's lab, drawn not only by Wurtz's growing international reputation but also by the cosmopolitanism of his lab, a haven for Jewish, Protestant, and politically liberal chemists in Paris, not to mention non-French speakers.[65]

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[61] Rocke, *Nationalizing Science*, 180–186. From 1850 to 1853, Wurtz directed a similar laboratory in private quarters, but their landlord sold the building and the laboratory had to be shut down just after Wurtz joined the faculty of medicine; Ibid., 143–153. Wurtz and other struggled to obtain public funding for laboratory education during the Second Republic and Second Empire; Ibid., 269–299.

[62] Spoken Alsatian, a variant of Alemannic, is a close cousin of Swiss German. By the early nineteenth century, state authorities had managed to institute French language instruction in Strasbourg primary schools, but outside of the classroom, German predominated; Rocke, Nationalizing Science, 75.

[63] On the economic history of Alsace, see Michel Hau, *Les dynasties alsaciennes: Du XVIIe siècle à nos jours* (Paris: Perrin, 2005).

[64] Rocke, *Nationalizing Science*, 73–102.

[65] Wurtz spoke Alsatian, French, English, German, and Italian; Rocke, *Nationalizing Science*, 352.

Many contributors to structural organic chemistry from all over Europe, including Beilstein, spent time working alongside Friedel in Wurtz's lab.[66]

Friedel was Wurtz's prize student and scientific lieutenant. Recalled Albert Ladenburg, a German chemist who worked in Wurtz's lab during the mid-1860s, "His [Wurtz's] first question was always addressed to Friedel and he would never make a remark of any importance without shouting across the laboratory: Where is our friend Friedel?" Wurtz also relied on the complementary temperament of his protégé: "Because of his great excitement, Wurtz frequently had small accidents, and it was always Friedel who, with his calm and his handiness, had to repair the damage."[67]

In 1858, just as Wurtz, Kekulé, a few of their colleagues were beginning to draw together the formative principles of structure theory, Friedel, Beilstein, and a number of other young chemists in Paris organized a literature discussion group. Wurtz soon joined, and transformed this "Société Chimique" into an independent professional society where chemical questions might be debated and research might be published outside of the conservative and highly restrictive setting of the Académie des Sciences. Wurtz convinced other more senior chemists to join, and the Society and its publications became a primary site of debate between

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[66] One of Wurtz's earliest students, the Alsatian Auguste Scheurer-Kestner, was imprisoned for several months in 1862 by Louis Napoleon. As a senator under the Third Republic, Scheurer-Kestner led the movement to re-open the Dreyfus case in 1897. Soon thereafter, his former labmate Grimaux was fired from the École Polytechnique for his Dreyfusard stance, and Friedel welcomed Grimaux into his lab at the Sorbonne; Ana Carneiro and Natalie Pigeard, "Chimistes alsaciens à Paris au 19ème siècle: Un réseau, une école?," *Annals of Science* 54, no. 6 (1997): 533–546.

Of Protestants and Jews in Second Empire and early Third Republic Paris, Friedel's close friend and collaborator James Crafts wrote, "Isolation is too strong a word to be applied to Protestant society at Paris, and yet in certain ways their habits of thought are different from those of their neighbours….Those who elected French nationality after the war, or who, like Friedel, were fortunate in having already chosen their residence in Paris, have had full recognition of their patriotism and of their value as citizens; nevertheless, a mental barrier is raised to some extent between Protestants and Catholics, and curious testimony of this attitude was lately offered when the latter joined with an army of Freethinkers and affected to confound the Protestants with the Jews"; James Mason Crafts, "Friedel Memorial Lecture," *Journal of the Chemical Society (Transactions)* 77 (1900): 999.

[67] Quoted in Auguste Béhal, "Célébration du centenaire de la naissance de Charles Friedel: Notice sur sa vie et sur une partie de son œuvre en chimie organique," *Bulletin de la Société Chimique de la France* 51 (1932): 1495.

atomists and their opponents over the value and scientific legitimacy of the system of atomic weights, valency, and structural formulas.[68]

Chief among these opponents was Marcellin Berthelot, a chemist only five years older than Friedel whose precocious work on the synthesis of fats, thoroughgoing empiricist philosophy, talent for self-promotion, and political savvy catapulted him to favor in Parisian intellectual circles during the 1850s. Berthelot and allies like Henri Sainte-Claire Deville and Henry Le Châtelier had their own vision of chemical progress, grounded in thermodynamics, precise measurements, quantitative laws, and inorganic chemical industries such as the manufacture of aluminum and cement.[69] Berthelot contended that the atomic theory was filled with unjustified hypotheses, and that only quantitative generalizations founded on precise measurements constituted reliable chemical knowledge. In his 1860 opus *Chimie organique fondée sur la synthèse*, Berthelot dismissed Wurtz's and Kekulé's ideas as "theories of language rather than theories of facts," and insisted that "all that matters is the discovery of general facts and of the laws" – by which he meant quantitative relationships – "that connect each one to all the others."[70] The conservative scientific elders at the Académie des Sciences tended to sympathize.

A particularly telling topic of debate concerned isomerism: the phenomenon wherein compounds made up of the same elements combined in the same proportions displayed different properties. Since isomers differed only in the manner in which these same proportions of the same elements combined, they provided a crucial means for developing and

[68] Rocke, *Nationalizing Science*, 214–220; Carneiro and Pigeard, "Chimistes alsaciens à Paris au 19ème siècle"; Fauque, "La réception de la théorie atomique."

[69] Muriel Le Roux, "From Science to Industry: The Sites of Aluminium in France from the Nineteenth to the Twentieth Century," *Ambix* 62, no. 2 (2015): 114–137.

[70] Marcellin Berthelot, *Chimie organique fondée sur la synthèse* (Paris: Mallet-Bachelier, 1860), cxxii–cxxiv, quoted in Rocke, *Nationalizing Science*, 251.

testing ideas about the inner constitution of chemical compounds.[71] This was particularly true for structure theory, whose principles and hypotheses permitted a certain number of distinct structural formulas to be drawn for a given set of atoms or groupings of atoms.

In 1863, Berthelot delivered a lecture on isomerism before the Société Chimique, which he expanded into a 250-page monograph for a collection of such lectures published in 1866. In the first two parts of the lecture, Berthelot provided an extensive overview of the phenomenon, beginning with a definition. "Let us commence," wrote Berthelot, "by defining with rigor the terms which we will use. I call isomeric bodies: the bodies formed of the same elements, united in the same proportions, but presenting different properties, which is the proof of a different arrangement."[72] He drew upon this definition as a justification of the sweeping scope of his lecture; since it applied just as well to different forms of simple substances, such as the graphite and diamond forms of carbon, as to compounds of the same composition, he extended his treatment to all pure substances. Having established that his study spanned all of chemistry, he proceeded to subdivide it according to a classification of different types of isomerism "according to the *a posteriori* study of phenomena."[73] For Berthelot, this empirical classification consisted of the different types of chemical reaction by which isomeric compounds were produced. In the bulk of the first two parts of the lecture, he methodically discussed the general features and particular examples of each type of isomerism, first among compounds, then simple substances, relying heavily on his own experimental specialty, precise measurements of heat.

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[71] As Berthelot put it, "Indeed, the facts of isomerism permit the reduction of the comparison of bodies and reactants to only one order of facts, in eliminating the differences which correspond to the nature and weight of the elements." Marcellin Berthelot, "Sur l'isomerie," in *Leçons de chimie professées en 1864 et 1865* (Paris: Librairie Hachette, 1866), 5.

[72] Ibid., 5.

[73] Ibid., 17.

In the third part of the lecture, Berthelot drew on evidence that he had presented previously to critique the system of atomic weights advocated by Wurtz, Friedel, and their colleagues. Berthelot pointed out anomalies in measurable properties such as the specific heat of certain metals and the vapor density of arsenic and phosphorus that, he contended, undermined the atomists' claims to have established a unitary weight for individual units of each element.[74] He concluded by suggesting that his exploration of isomerism revealed the root of these anomalies: the atomists had improperly conflated what he called "physical atoms" and "chemical atoms," which were often but not always the same entities.[75]

Friedel and his colleagues responded in kind. In 1866, another Alsatian student of Wurtz, Edouard Grimaux, presented an agrégation thesis entitled "Equivalents, Atoms, Molecules."[76] The thesis was an extended exercise in defining these three terms, so crucial in attempts to articulate rules governing chemical combination and the inner constitution of chemical substances. "These words themselves," Grimaux declared, "have been employed alternately with different meanings, and have thus contributed to throwing obscurity upon these questions, the most difficult and advanced in chemistry."[77] He cast Berthelot and his ilk as the ones who were careless about language:

> One might say, it is true: "What importance is the word, if we well know what we intend by compound atom, simple atom." Nevertheless, the logical mind attaches to a word a sense derived from etymology. The science should tend to employ a precise language, free from obscurities and uncertainties.[78]

[74] Ibid., 217.

[75] Ibid., 232.

[76] Grimaux was the nephew of Gerhardt, also a Strasbourg native.

[77] Édouard Grimaux, *Équivalents, Atomes, Molecules* (Paris: F. Savy, 1866), 7.

[78] Ibid., 33.

Friedel took up the defense of structure theory in his 1869 thesis, a review of a series of painstaking experiments that had had carried out over the preceding decade.[79] Where Berthelot had set his sights on the entirety of chemistry, Friedel took up one family of compounds, the three-carbon acetones, aldehydes, and their derivatives, in exhaustive detail. In reporting the results of his experiments, along with an extensive historical and theoretical discussion, Friedel sought to gather and assess everything known about the constitution of acetone based on its various reactions. He offered this intensive discussion as a case study demonstrating the effectiveness of structure theory, and in particular Wurtz's concept of "atomicity," the principle later referred to as valence. Friedel aimed "to give an example of the clarity that the consideration of the atomicity of the elements lends, in my opinion, to the relationships that exist among a great number of organic compounds."[80] A key element of this clarity was the structural formula, which could describe compounds according to the "bonds which relate to mutual relationships of saturation among atoms" and illuminate "a certain number of their essential properties."[81]

Friedel, too, took up matters of language in his thesis; instead of definitions of terms, he turned to chemical names. Friedel took disparities among chemical names as a starting point. He framed his historical and experimental studies around the questions of isomerism and synonymy: did different chemical names that chemists had given to substances with the same composition refer to isomers, or to the same compound? He used experimental evidence to reveal which names were synonyms and which indicated isomers, and then proposed

[79] This discussion occupied half of Friedel's unusual two-part thesis. The other half was mineralogical; Friedel, "Recherches sur les acétones et sur les aldéhydes." All of Friedel's academic positions were in mineralogy until he succeeded Wurtz in the Sorbonne chair of organic chemistry in 1884.

[80] Ibid., 310–11.

[81] Ibid., 322, 328.

structural formulas that explained and were supported by the results of his experiments. Friedel presented his work as a model for a broad research program:

> The study of these isomerisms, long and complicated, once it's a matter of compounds of slightly higher order, has nevertheless at least commenced in a rather large number of cases. The results obtained so far are fully in accord with predictions, and, a capital thing, we so far have no example of isomeric substances in a number greater than what the atomic theory would suggest.[82]

Where Berthelot dismissed the principles of structure theory as word games and cited anomalies to refute them, Friedel and his colleagues marshalled precise definitions to illuminate their critics' misunderstandings and detailed studies of series of simple compounds to affirm the clarity and explanatory power that structural formulas provided.

In the midst of these debates, Wurtz was lobbying the French education ministry to emulate the considerable investment that German states had made in the organization of chemical research at their universities.[83] The Wurtz group and their inclination toward German ideas and modes of organization remained marginal in late 1860s Paris. This changed significantly, however, with the war of 1870-71, in which Prussian forces quickly and decisively defeated their French counterparts, paving the way for the unification of the German Empire and its annexation of the provinces of Alsace and Lorraine. French statesmen seeking explanations for Prussia's decisive defeat of French forces and the subsequent loss of the provinces Alsace and Lorraine looked especially to the growing industrial might of Germany and its links to German science.[84] German authorities' decision to reestablish the University of

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[82] Ibid., 330.

[83] Harry W. Paul, *The Sorcerer's Apprentice: The French Scientist's Image of German Science, 1840-1919* (Gainesville: University of Florida Press, 1972), 7–9.

[84] Fox, *The Savant and the State,* 227–41; Rocke, *Nationalizing Science,* 336–41.

Strasbourg as Kaiser Wilhelm University, envisioned as a crowning jewel among the German universities, added symbolic heft to this impression.[85]

In such circumstances, connections to German chemists and affinity for German chemistry became marks of authority for Friedel and his Alsatian allies. Their political position was bolstered by their public avowal of patriotism in 1872, as they chose to affirm their French citizenship and renounce their right of return to their birthplace, and by the favorable position of center-left politics under the Third Republic. Over the 1870s, Wurtz, Friedel and a core group of their students became the hub of a loose network of reformist savants, industrialists, and statesmen, many of them chemists, Alsatians, or both.[86]

The institution-building efforts of this group included the construction of new laboratory facilities at the Sorbonne and the promotion of institutions for the scientific education of chemists bound for jobs in industry. The most far-reaching of these institutions was the *Association française pour l'avancement des sciences* (AFAS). Its mission was the renovation of French science and industry through promoting more interchange among scientists and technical workers, especially in the provinces.[87] Though Wurtz and Friedel secured the participation of a wide range of savants, including Berthelot, in the foundation of the organization, structure theory quickly came to dominate the chemistry section of AFAS. It was not in Paris but at an AFAS Congress in Lille that Wurtz delivered an influential 1874

[85] John E. Craig, *Scholarship and Nation Building: The Universities of Strasbourg and Alsatian Society, 1870-1939* (Chicago: University of Chicago Press, 1984). Adolf Baeyer and his student Emil Fischer were among the first cohort of faculty and students at the new university.

[86] Carneiro and Pigeard, "Chimistes alsaciens à Paris au 19ème siècle."

[87] There was more autonomy to be had in the provinces. Wurtz group members including Albin Haller and Edmond Willm were able to teach structural organic chemistry unencumbered by limitations that their peers in Paris faced, though they struggled for resources. Nye, *Science in the Provinces*.

speech advocating structure theory and atomism on the basis of the role it had played in the development of the dye industry.[88]

In Paris, the debates continued. Even as atomism won an increasing number of adherents and began to gain a foothold in higher education, its opponents continued the fight. In the late 1870s, Berthelot continued to write that the "language of the atomists" consisted of "mystical hypotheses."[89] In 1880, Deville steadfastly contended: "I admit neither the law of Avodagro, nor atoms, nor molecules, nor forces, nor the particular states of matter, refusing absolutely to believe anything that I cannot see, nor even imagine."[90] Friedel and his colleagues, too, continued to make the same sort of arguments that they'd been making for twenty years. In his opening lecture in his new chair at the École Polytechnique, Grimaux returned to the topic of equivalents, atoms, and molecules, still contending that notwithstanding Berthelot's claims to have carefully kept fictive bodies out of his work, his notation was less convenient and at least as hypothetical as that of structure theory.[91] In his introduction to an 1886 fourth edition of Wurtz's *Théorie Atomique*, penned just after Wurtz died and Friedel took his place as Professor of Organic Chemistry at the Sorbonne, Friedel wrote:

> Although science advances fairly rapidly, public education, in its prudent march, struggles to follow this movement, especially in our country, bound to uniformity in its very organization. It's not only a matter of new facts and points of view that have been discovered and which have to be made known – at least in their most important aspects – to those who should be introduced to the elements of science; the language itself must be transformed to express the new ideas, and nothing is more difficult to accept than a change of language.[92]

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[88] Fauque, "La réception de la théorie atomique."

[89] Quoted in ibid., 98.

[90] Quoted in Élisabeth Bardez, "Henri Sainte-Claire Deville," in Laurence Lestel, ed., *Itinéraires de chimistes 1857-2007: 150 ans de chimie en France avec les présidents de la SFC* (Paris: EDP Sciences, 2007), 480.

[91] Édouard Grimaux, *Introduction à l'étude de la chimie, théories et notations chimiques* (Paris: Dunod, 1883), 197–198.

[92] Charles Friedel, "La vie et les travaux de Wurtz," introduction to Charles Adolphe Wurtz, *La théorie atomique*, 4th ed. (Paris: F. Alcan, 1886), xxv–xxvi.

On the surface, the concerns of the French atomists in the late 1880s were a far cry from the frustrations of German chemists struggling with the centrifugal effects of competing conventions of rational nomenclature along the leading edge of organic chemical research. Friedel and his colleagues were still much concerned with establishing a firm foundation for research and instruction in structural organic chemistry, from the bottom up, with their nation's economic future and their own position within the French scientific bureaucracy at stake. However, several distinctive features of the strategy that the group had honed over nearly three decades in pursuit of these ends contributed to making the confused German nomenclature into a French opportunity. First, Friedel and his allies focused on definitions and terminology, showing where the misunderstandings of opponents like Berthelot lay and illuminating the clarity that the terms and notation of structure theory brought to chemical investigations. Second, they carried out exhaustive studies of simple series of compounds, like Friedel's three-carbon relatives of acetone, to provide a firm empirical foundation for their claims free from anomalies that critics might exploit. Third, they built institutions outside the state bureaucracy as vehicles for the promotion of structure theory, especially as an aid to French industry and industrial science education. Fourth, they drew upon the cosmopolitanism of the Wurtz group and the network of foreign chemists that had passed through the lab as a source of domestic prestige and authority in light of concern regarding the decline of French science relative to that of its neighbors.

In this context, as he wrote in 1886, Friedel considered no aspect of this effort to reform French chemistry more important – or more difficult – than the transformation of chemical language. Drawing on the resources of the institutions that he'd helped to build and his international connections, Friedel would soon effect another sort of transformation in pursuit of this end: the conversion of individual proposals for naming complex series of novel

compounds into an international nomenclature reform effort, with himself and his students at its head.

Nomenclature reform goes international

Beginning in the summer of 1888, as contending individual proposals for the nomenclature of particular series of compounds appeared in German journals, an international effort to reform the nomenclature of organic chemistry emerged in France. It began with a distinctively Parisian opportunity for internationalism. By the summer of 1888, the Tour Eiffel was rising above the Champ de Mars, and plans were well underway for the 1889 Universal Exposition. At once a showcase of arts and technology from around the world and a commemoration of the centenary of the French Revolution, the Exposition positioned Paris as the cultural center of a rapidly progressing industrial world.[93]

At the Société Chimique, Charles Friedel was serving his fourth term as president. At a Council meeting in July, Friedel unveiled a proposal that he'd developed for the Society to take advantage of the exposition to host a Congress of Chemistry, specially inviting foreign chemists to participate.[94] Friedel proposed that the Congress address two topics: "the unification of procedures of commercial analysis" and "the unification of chemical nomenclature and notation." The council, populated with Friedel's scientific allies, approved his plan. Friedel, Grimaux, and Maurice Hanriot, another Wurtz student, were among the committee charged with organizing the Congress.

Soon thereafter, other chemists in Friedel's Alsatian network began to address matters of nomenclature. Each academic year, Friedel invited several chemists to deliver special lectures in his laboratory, a tradition begun by Wurtz in the interest of educating students (and

[93] Horne, *A Social Laboratory for Modern France*, 54–63.

[94] Minutes of Council, 31 Jul 1888, Procès-verbaux de la Société Chimique de Paris, Cahier 2, 77–79, Archives de la Société Chimique de France, Paris. My thanks to Danielle Fauque for her assistance in consulting this material.

their professors) in topics that lay outside the formal curriculum established by the ministry of public instruction.[95] During the academic year 1888-89, Friedel added new tradition: each year, he would select several of these lectures that he deemed of special important for publication in an annual volume. The first volume consisted of seven lectures. In five of these, a young chemist reviewed recent research in a family of synthetic organic compounds, presenting an overview of the findings of the overwhelmingly German collection of chemists who had carried out this research.[96]

Each of these lecturers liberally illustrated his presentation with structural formulas, peppered it with French language names of the substances that he discussed, and mentioned the significance of nomenclature for future French research in each area. Louis Bouveault, a student of Hanriot, delivered a lecture on closed-chain compounds containing nitrogen atoms – a group that included the compounds whose great interest and disordered nomenclature Widman had just recently reviewed in a German periodical. Bouveault hoped that his lecture would make this literature more accessible to his French auditors. "I would be pleased," he said, "if the nomenclature that I have indicated for these numerous series of substances, in rendering less tedious the reading of the innumerable German publications that address them, simplifies the work of bibliography for those among you who would wish to advance the chemical science in this direction."[97]

In their lectures, Friedel's students Auguste Béhal and Adrien Fauconnier made similar efforts to put nomenclature in order. Before discussing research on compounds containing more than one functional group, Béhal first described his notation. "We must here set forth the

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[95] Charles Friedel, ed., *Conférences faites au laboratoire de M. Friedel, 1888-1889* (Paris: G. Carré, 1889), v–vii.

[96] The other two consisted of a quantitative treatment of the solubility of salts, a central problem in the nascent field of physical chemistry, and research in the alkaloids of the cinchona tree, a collection of organic substances including quinine whose constitution remained highly uncertain.

[97] Louis Bouveault, "Sur les composés à chaîne fermée dont le noyau contient plusieurs atomes d'azote," in Friedel, *Conférences faites au laboratoire de M. Friedel*, 25–26.

conventions that enable us to designate of the various atoms of carbon in a molecule," he said, and detailed how he used Greek letters to accomplish this.[98] Fauconnier paused in the midst of his exposition of hydrocarbon rings to remark, "Before beginning the description of these various compounds, we should indicate the nomenclature adopted for the entire series."[99] In the lectures that Friedel solicited and printed in 1888-1889, his students and those of his Alsatian allies not only helped develop a French lexicon for new synthetic compounds, but promoted explicitness in nomenclature practice that might make these names more legible than their German sources.

Friedel's provincial colleagues gained a sudden interest in nomenclature as well. Albin Haller, another Alsace native and a key node in Wurtz's network, penned an essay on the nomenclature of nitrogen-containing compounds as an appendix to the 1889 edition of *Agenda du Chimiste*, a laboratory handbook issued annually for the use of chemists and engineers.[100] In his preface to this essay, in which he reviewed the disparate nomenclature of about three dozen new classes of synthetic compounds, Haller repeated the increasingly standard complaints about the confusion in chemical naming. But he framed his critique in a somewhat different manner. Where Widman complained of the superfluity of names for synthetic compounds, Haller saw the "paucity of words able to translate clearly their origin" as the ultimate source of a confused state of affairs that had made it "impossible, without continuous

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[98] August Béhal, "Relation entre deux groupements fonctionnels dans la même molécule," in Friedel, *Conférences faites au laboratoire de M. Friedel*, 64–65.

[99] Adrien Fauconnier, "Sur les anneaux polyméthyléniques," in Friedel, *Conférences faites au laboratoire de M. Friedel*, 110.

[100] Albin Haller (1849-1925) was a native of Alsace and a key node of Wurtz's Alsatian network, especially when it came to promoting the application of organic chemistry to industry and applied chemistry education. Haller succeeded Friedel in the Sorbonne chair of organic chemistry in 1899. Carneiro and Pigeard, "Chimistes alsaciens à Paris"; Claude Viel, "Albin Haller," in Lestel, *Itinéraires de chimistes*, 245–50.

Conceived and edited by George Salet, another Wurtz student and longtime *maître du conferences* under Wurtz and Friedel at the Sorbonne, the *Agenda du chimiste* adhered to Wurtz's atomist principles and notation. It found broad use among both academic and industrial audiences. Ana Carneiro, "The Research School of Chemistry of Adolphe Wurtz, Paris, 1853-1884" (PhD Dissertation, University of Kent at Canterbury, 1992), 254–255.

and uninterrupted effort, to make sense at first glance of the nature of the new research described in our periodicals." Haller could offer only a glossary of current names and structural formulas in this essay. He looked forward, however, to a day when he and his colleagues might "embrace in one collective view the collection of all organic substances of which nitrogen is a constituent part."[101] Like Friedel and Grimaux, Haller presented clarity of language – in this case, clarity of nomenclature – as a means to a more comprehensive and consistent understanding of chemistry. In this essay, Haller brought both the existing lexicon of names based on the structure of compounds and the case for comprehensive nomenclature reform to the French chemical public. The essay did not mention the discussion of nomenclature at the upcoming Congress, but it established its importance nonetheless.

The International Congress of Chemistry took place from July 29th through August 1st, 1889. Marcellin Berthelot presided, having succeeded Friedel as President of the Société Chimque. The Congress was comprised of four sections, one of which met during each of its four days, and plenary sessions each evening to vote on the resolutions discussed at the sectional meeting earlier in the day.[102]

Its title notwithstanding, the International Congress was mostly a French affair.[103] The three hundred chemists in attendance included only "a certain number of distinguished foreigners," reported a British correspondent. That certain number, unnamed but implicitly small, was in part the consequence of an illness that had laid up Congress secretary Hanriot before he could send out most of the invitations to the Congress. Fauconnier stepped up to

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[101] Albin Haller, "Nomenclature et origine de certains groupes de composés azotés," in *Agenda du chimiste* (Paris: Librairie Hachette, 1889), 483.

[102] "Congrès international de chimie in Paris," *Chemiker-Zeitung* 13, no. 83–84 (1889): 1371–1372, 1391–1393.

[103] Anne Rasumssen has suggested that late nineteenth century international scientific congresses fall into one of two categories: de facto national meetings to which a few foreigners were invited, and congresses that came about expressly to deal with international problems on an international basis; Anne Rasmussen, "Jalons pour une histoire des congrès internationaux au XIXe siècle: Régulation scientifique et propagande intellectuelle," *Relations internationales* 62 (1990): 123–24.

replace him, but further processing of the invitations by the Ministry of Public Instruction brought additional delays. The unknown signature of the student Fauconnier instead of a figure like Friedel or Berthelot may have rendered the belated invitations still less appealing.[104] Most of the foreign chemists who did attend made the trip from the edge of Europe or beyond – Kiev, Constantinople, Rio de Janeiro, Bucharest, and Madrid. The organizers may have sent their invitations early, owing to the greater time it would take for them to prepare for and make the journey to Paris. Positioned on the margins of the intra-European circuit of scientific travel, they may also have been more enthusiastic about the opportunity that the Congress provided than were their more centrally-located colleagues.

On its first three days of meetings, the Congress discussed proposals for standardizing procedures for the analysis of food, agricultural products, and pharmaceuticals. Here, the Congress focused on problems more national than international. Their resolutions – to codify methods for the analysis of wine, to allow chemists to exercise a greater degree of judgment in legal testimony regarding agricultural products, and to establish commissions to develop methods of analysis for drugs – were directed less toward the international community of chemists than toward the French government.

Friedel presided over the Congress' fourth and final section, which addressed nomenclature reform. This final session, reported the correspondent for *Nature*, "was the one of by far the greatest general interest," as testified by the participation of eight foreign chemists, all named Vice-Presidents.[105] Two of these foreigners, Carl Graebe and Emilio Noelting, professors at the universities of Geneva and Mulhouse (the second city of Alsace), respectively, presented proposals and participated actively in the discussions of the section.

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[104] "The International Chemical Congress," *Nature* 40 (August 15, 1889): 369.

[105] Ibid.

The section's main contributors, however, were Friedel's students and allies. Friedel himself proposed adopting the standard German manner of naming for ketones into French usage. Grimaux proposed generalizing the English names for such compounds as benzene and naphthalene in order to reserving the suffix *–ol* for alcohols, and deriving the name of each aldehyde from that of its corresponding alcohol. Bouveault and Béhal presented proposals to regularize the nomenclature of the classes of compounds that they had discussed in their lectures to Friedel's laboratory. Friedel's student and nephew Alphonse Combes presented a report on the numbering of the carbon atoms of benzene in naming its derivatives. Léon Maquenne, another of Friedel's 1888-89 lecturers, and Grimaux debated how best to carry out a proposal that Bouveault offered on behalf of Hanriot. The day's discussions may have been of general interest to the foreign chemists whose presence made the Congress international, but they were dominated by Friedel's set.

At the end of the day, a group of at least sixty-eight chemists gathered to vote on the nomenclature proposals that the section had discussed.[106] Berthelot, who had presided over the three previous plenary sessions, was absent, and Friedel remained in the chair. The gathering approved the relatively uncontroversial proposals of Grimaux and Friedel, as well as those of Graebe and Noelting. It did not approve the proposals of Bouveault, Béhal, or Combes, but it also did not reject them. Rather, the Congress voted to form an International Commission, chaired by Friedel, to unify organic chemical nomenclature. The proposals of Bouveault, Béhal, and Combes, which were directly oriented toward addressing the areas of nomenclature that had become most troubling to chemists over the preceding decade, were to be the starting point of the commission's work.

[106] This number is derived from the account of the congress in *Nature*, which mentioned a 42 to 26 vote in favor of postponing decision on Béhal's proposals; Ibid., 371.

The seven resolutions that the Congress did approve before passing on the torch to the International Commission did not have much to do with the compounds whose nomenclature had become a source of concern over the preceding decade. As at Karlsruhe, there was no suggestion of how these resolutions might be enforced.[107] Nevertheless, they did constitute an accomplishment that Friedel could point out. In his closing address, Friedel defended these resolutions as a concrete achievement and trumpeted them a reason for optimism about the progress still to come:

> M. Friedel made a short speech, in which he thanked especially the foreign members for their attendance and help. Some people might, he said, think that the Section had accomplished but little; that, however, was not his opinion: their aim had been to help workers in their work, and nothing could be more conducive to this aim than the use of the same language among the chemists of all countries. He felt sure that their efforts would prove fruitful in the future, and he hoped that by next year the International Committee would be able to report such serious progress as to justify the summoning of another Congress.[108]

Friedel's enthusiasm rubbed off. After departing Paris, Graebe wrote to his friend and collaborator Heinrich Caro, "I've taken part in the Congress of Chemists and involved myself rather eagerly in the debates over nomenclature. I intend to report on them in Heidelberg and thus perhaps contribute to initiating an international understanding."[109]

In fact, the eager involvement of Graebe and Noelting suggests a reason why Friedel might not have been dismayed by the snafu with the Congress invitations. The Congress' approval of the rather simple proposals that he and Grimaux put forward had allowed him to claim that the Congress had at least taken a few steps toward "the use of the same language among the chemists of all countries." His articulation of the value of the Congress'

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[107] For example, the author of a German summary of the Congress broke one of the very rules on which he was reporting by using the name "benzol" for the hydrocarbon that, by the Congress' decision, was to be named "benzene." "Congrès international de chimie in Paris," 1393.

[108] "The International Chemical Congress," 371.

[109] Graebe to Caro, 11 Aug 1889, quoted in Elisabeth Vaupel, "Carl Graebe (1841-1927): Leben, Werk und Wirken im Spiegel seines brieflichen Nachlasses" (PhD dissertation, LMU München, 1987), 330.

achievements took advantage of this simplicity; it would have been more difficult to present more abstruse proposals such as those that Béhal, Bouveault, and Combes put forward as something that would self-evidently "help workers in their work." Had more of the German chemists then occupied in proliferating nomenclature schemes and reform proposals been present, the Congress could easily have devolved into highly technical debates over narrow collections of complex compounds. Whether by intention or serendipity, the attendance at the Congress at once allowed Friedel to praise its internationalism and to control its agenda.

The makeup of the international commission was similarly well engineered *(Fig. 7)*. To form this commission, the Congress named the seven foreign chemists who had been taken part in the Section discussions, plus the Parisians Friedel, Grimaux, Béhal, Bouveault, Combes, Fauconnier, and Armand Gautier (yet another Wurtz student).[110] Six senior chemists from nations not represented at the Congress were nominated in absentia, as were Beilstein and Baeyer, the reigning authorities on organic nomenclature. Friedel's anti-atomist rivals Berthelot and Emile Jungfleisch were added as well, though neither had expressed interest in matters of nomenclature, and Berthelot had not even attended the discussions of nomenclature at the Congress for which he served as President.

All of these names served a purpose. The seven non-French Congress participants named to the Commission and six chemists named in absentia lent the Commission a broad international representation of thirteen countries in addition to France. Of these thirteen, only those working in German settings and those who had trained with Wurtz or Friedel ended up participating in the Commission's work, but each lengthened the list of nations attached to

[110] An eighth foreigner who had been present for the nomenclature section but had not participated in its discussions, the Brazilian de Luz, was left off the rolls of the International Commission.

Friedel's reform effort.[111] As the reigning authorities on chemical nomenclature and lexicography, the inclusion of Baeyer and Beilstein leant the Commission legitimacy among German chemists that it might otherwise have lacked. On the Parisian front, the inclusion of Berthelot and Jungfleisch made it difficult to dismiss the Commission as a factional project of the Friedel-Wurtz group.

MM.		MM.	
Berthelot,	France.	Von Bæyer,	Allemagne.
Friedel,		Nœlting,	
A. Gautier,		Lieben,	Autriche.
Grimaux,		Franchimont,	Hollande.
Schutzenberger,		Paterno,	Italie.
Jungfleisch,		Armstrong,	Angleterre.
Fauconnier,		Istrati,	Roumanie.
Béhal,		Calderon,	Espagne.
Combes,		Clève,	Suède.
Bouveault,		Bonkowski-Bey,	Turquie.
Græbe,	Suisse.	Ira-Remsen,	Etats-Unis d'Amérique.
Alexeyeff,	Russie.	Mourgues,	Chili.
Beilstein,			

Figure 7. Members of the International Commission.[112]

Yet that was exactly what it was. A week after the Congress adjourned, likely before some of the members of the International Commission even knew of its existence, Friedel called its first meeting. At this meeting, the Commission voted to appoint its Parisian members – undoubtedly a majority of those present – to a Subcommission charged with drafting recommendations for its colleagues abroad to review. The active portion of this Subcommission was still smaller: Friedel, two fellow Wurtz students and longstanding companions in the atomism debates (Gautier and Grimaux), and four young chemists who had been working on matters of nomenclature for the year leading up to the Congress (Combes,

[111] Of these thirteen, the active participants were Graebe, Noelting, Armstrong, Lieben, Franchimont, and Istrati; Lieben and Franchimont had studied with Wurtz, and Istrati studied with Friedel.

[112] "Rapport de la Sous-Commission," *Association Française pour l'Avancement des Sciences: Compte rendu* 21 (1892): 393. The foreigners nominated in absentia included the American Ira Remsen, the Briton Henry Armstrong, and Friedel's former labmate, the Austrian Adolf Lieben.

Béhal, Bouveault, and Fauconnier).[113] The three classes of substances that the Congress placed on the Commission's agenda – nitrogen-containing ring compounds, compounds with multiple functional groups, and polysubstituted benzene derivatives – were those for which Bouveault, Béhal, and Combes had presented proposals at the 1889 Congress, proposals that they had been working on, with Friedel's encouragement, for much of the previous year.

This head start proved valuable in maintaining the French chemists' leadership of the nomenclature reform effort. The rest of the Commission was charged with promoting the discussion of nomenclature in their nations' chemical societies. Baeyer, who needed little encouragement, took the occasion of his report on the formation of the commission to share his latest nomenclature proposals with the executive committee of the Deutsche Chemische Gesellschaft in March of 1890, to universal applause. Baeyer also proposed that Beilstein be added to the Commission, apparently not aware that he'd been named to it from the start, and that the St. Petersburg chemist also be appointed its president.[114] The suggestion might have been enough to sink the nascent reform effort. Beilstein was in no position to take on such a task, isolated hundreds of miles away from the other Commission members and struggling as it was to keep pace with the chemical literature in compiling the third edition of his Handbook.

By that time, though, the studies of Friedel and his Subcommission were already well underway. Not only had Friedel engineered a yearlong head start for his cohort, but the centralization of French science that had kept the atomism debate smoldering also made it possible for the Parisian subcommission to collaborate rapidly and intensively. No further mention was made of putting Beilstein in charge. The division of labor was set: the Parisians

[113] Alphonse Combes, "Le congrès international de nomenclature chimique," *Revue générale des sciences pures et appliquées* 3, no. 8 (April 30, 1892): 258 n1.

[114] "Sitzung vom 10. März, 1890," *Berichte der Deutschen Chemischen Gesellschaft* 23 (1890): 563–566.

took on the task of crafting nomenclature proposals, and their colleagues abroad fostered local support for nomenclature reform, as Friedel seems to have wanted.

Conclusion

Friedel's nomenclature reform efforts came to a head two years later, in the Geneva Nomenclature Congress. The next chapter will turn to the Subcommission's preparations for the meeting and the events of the International Congress itself. First the Subcommission and then the Congress redefined the meaning and scope of organic chemical nomenclature, with far-reaching implications for the organization of chemical knowledge, its deployment in chemical research, and the social order of chemistry.

Geneva Congress Secretary Amé Pictet began his account of the proceedings by describing the circumstances that had brought it about. "The prodigious development of organic chemistry since the middle of our century" had introduced "a regrettable confusion into the nomenclature of carbon compounds." In order to name the "innumerable substances that the new procedures of synthesis have created," Pictet wrote, "new conventions had to be made, gradually and as needed." When these conventions "found themselves in contradiction with each other," the resulting confusion "threatened to exercise an untoward influence on the development of chemistry," in light of which "the reform and unification of the nomenclature of organic compounds was necessary."[115]

Historian Brigitte Schroeder-Gudehus has argued that concerns such as these were a primary motivation for the proliferation of international congresses across many fields of natural science in the late nineteenth century. Writing against an assumption that such congresses simply expressed the inherent political and intellectual internationalism of science,

[115] Amé Pictet, "Le congrès international de Genève pour la réforme de la nomenclature chimique," *Les archives des sciences physiques et naturelles* 27 (May 1892): 485–486.

Schroeder-Gudehus suggests that the *difficulty* of organizing science across national and linguistic boundaries was behind the "hunger for scientific congresses" during this period. In her view, the "common enthusiasm for the rational organization of research activity" was the pragmatic foundation of these congresses.[116] To be sure, the scientific internationalism realized at such meetings took on numerous forms of scientific, social, political significance. As Schroeder-Gudehus and other historians of scientific internationalism have noted, international congresses were sites where scientific disciplines, factions, institutions, individuals could express and compete for power.[117] They were a means by which a nation could lay claim to international leadership in a scientific field (a particularly common aim of savants in Third Republic France).[118] They provided a means of framing scientific collaboration in terms of lofty ideals of international political accord.[119] But all of these moves were possible, Schroeder-Gudehus suggests, only because of the broadly-recognized pragmatic importance of organizational efforts such as the reform and standardization of nomenclature.

The history of the reform movement that led to the Geneva Nomenclature Congress supports many aspects of this account: the importance that the standardization of nomenclature took on during the late nineteenth century, the central role of the need for better organization of chemical naming practices in driving the creation of international chemical Commissions and Congresses, the intellectual, economic, and political goals that Friedel sought to advance by shepherding such a reform effort.

However, this history also shows that perceptions that science needed to be organized were themselves the *product* of particular developments, and took on different degrees of

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[116] Brigitte Schroeder-Gudehus, "Les congrès scientifiques et la politique de coopération internationale des académies des sciences," *Relations internationales* 62 (1990): 138.

[117] Ibid., 135.

[118] Fox, *The Savant and the State,* 276–277.

[119] Rasmussen, "Jalons pour une histoire," 125.

urgency and significance in different intellectual and institutional settings. Chemists had considered the names of organic chemical compounds to be in a state of disorder for decades without seeing the need to do anything about it. The particular configuration of German chemical science and industry enabled the emergence of nomenclature displaying a different sort of disorder, a confusion that was both especially worrisome and especially difficult to address effectively within this context. In contrast, from Friedel's particular position in French chemistry, in light of his experiences over the preceding decades in Parisian intellectual politics, the reform and unification of nomenclature on an international scale presented an opportunity to facilitate a particular sort of chemistry. It was not simply that Friedel thought that he could advance the factional interests of structural organic chemists in Paris by seizing control of an international nomenclature reform effort, or that he could promote the national interests of French science and industry by carrying out nomenclature reform under the French flag. Rather, the *very desirability and possibility of pursuing an international nomenclature reform at all* was shaped by his particular circumstances. The pragmatic importance of organizing and standardizing science on an international scale does not just provide an arena for various sorts of social, intellectual, and political contestation. It is also the product of situated cultures of science.

Chapter 2

"Just as the Structural Formula Does": Chemical Names, Diagrams, and Substances at the 1892 Geneva Nomenclature Congress

On the evening of Easter Monday, April 18th, 1892, thirty-four chemists assembled at the Hôtel de la Métropole, at the southwestern tip of Lake Geneva where it empties into the Rhone, looking out over the Jet d'Eau and across the lake to the mountains to the north. The group included presidents of national chemical societies, editors of prestigious journals, and three of the first five recipients of the future Nobel Prize in chemistry (first awarded in 1901), assembled for an event that a local newspaper promised would "mark an important date in the history of chemistry."[1] They had gathered to discuss an urgent problem that threatened to derail the extraordinary progress of chemical science and industry in the late nineteenth century: confusing nomenclature. Over the following four days – cold, but pleasant, the Congress secretary noted in his diary – in morning and afternoon sessions of about three hours each, the delegates of the Geneva Congress traversed the field of organic chemical substances and mapped it out in names.[2]

As the preceding chapter has shown, during the 1870s and 80s, the long-recognized irregularity in the naming of organic chemicals took on new significance within the intellectual, economic, and institutional ecology of the German-dominated science of organic chemistry. By inventing divergent conventions of rational nomenclature – names that reflected the inner structure, or constitution, attributed to a compound, often as expressed in a structural formula diagram – German chemists transformed disorderly nomenclature from a hopeless but minor

[1] Amé Pictet, Agenda, entry of 19 Apr 1892; "Chronique locale. Congrès de chimie," *Journal de Genève*, April 17, 1892, <http://www.letempsarchives.ch/>.

[2] Pictet, Agenda, entries of 20–21 Apr 1892.

annoyance into a serious but solvable problem. However, it was a French chemist, Charles Friedel, who took action. From a perspective shaped by the distinctive context of Parisian chemistry, the Alsatian savant saw nomenclature reform as an opportunity to advance French science and industry as a whole and the position of his own scholarly faction within it. In 1888, Friedel quietly put his younger colleagues to work on matters of nomenclature and set the machinery of associations that he'd helped to build into motion. By the end of 1889, Friedel had placed himself and a subcommission made up of his students and scientific allies at the head of an international effort to reform the nomenclature of organic chemistry.

The significance of the Friedel's reform project and its ultimate product, the sixty-two rules codified at the Geneva Congress, lay not in a set of chemical names, but in a conception of chemical nomenclature.[3] The effort proceeded in two stages, each of which marked a crucial transformation in the meaning of chemical names. First, over the course of dozens of meetings during 1890 and 1891, Friedel's Parisian cohort developed a comprehensive set of proposals to unify organic chemical nomenclature. Within these proposals, Friedel and his scientific allies reconceived nomenclature not simply as a collection of names but as a continuous field that spanned all organic substances. By making nomenclature express the many experimentally-attested structural relationships among organic compounds, the members of this subcommission sought to recast French chemical pedagogy in the terms of structure theory and to import the collective achievements of German synthetic chemistry, all at once.

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[3] On the Geneva Congress, see Maurice P. Crosland, *Historical Studies in the Language of Chemistry* (New York: Dover, 1978), 347–54; Pieter Eduard Verkade, *A History of the Nomenclature of Organic Chemistry*, trans. S. G. Davies (Boston: Reidel, 1985), 1–48, 276–298; Bernadette Bensaude-Vincent, "Languages in Chemistry," in *The Cambridge History of Science,* vol. 5, ed. Mary Jo Nye (Cambridge: Cambridge University Press, 2003), 174–190; James G. Traynham, "Organic Nomenclature: The Geneva Conference 1892 and the Following Fifty Years," in M. Volkan Kısakürek, ed., *Organic Chemistry: Its Language and Its State of the Art* (New York: VCHA, 1993), 1–8; James G. Traynham, "The Familiar and the Systematic: A Century of Contention in Organic Chemical Nomenclature," in *Essays on the History of Organic Chemistry*, ed. James G. Traynham (Baton Rouge: Louisiana State University Press, 1987), 114–126; Victor Grignard, *Traité de chimie organique* vol. 1 (Paris: Masson, 1935), 1073–1108.

Friedel brought these proposals onto an international stage in 1892, in Geneva, at the International Congress for the Reform of Chemical Nomenclature. The Geneva Congress was guided by a different vision of how, and where, chemical names might be put to work. German chemist Adolf von Baeyer convinced his fellow delegates that their nomenclature should comprise a set of rules for generating unique, "official" names corresponding precisely to unique structural formulas, names tailored for use in chemical journal indexes and reference works. In carrying out this plan, the Geneva Congress articulated a principle that a systematic chemical name should express the structure of a chemical compound, via a precise transformation of the compound's structural formula, a diagram, into words. The relationship between name, diagram, and substance has distinguished chemical nomenclature ever since.

This pivotal transformation in chemical naming did not happen in a vacuum. The forms of sociability among Friedel's subcommission and, especially, among the delegates to the Geneva Congress framed the discussions that produced the Geneva rules.[4] Decisions made at a scientific congress are the product of relationships and circumstances, before, during and after the congress itself. The making of the Geneva nomenclature reveals something about the micro-political dynamics of international science. It also shows how personal values and interpersonal relationships shaped a canonically impersonal mode of representation.

"The greatest authorities in all the nations of Europe"

The thirty-four delegates of the Geneva Congress arrived by different routes. Some traveled by Paris-Lyon-Mediterranean railroad, taking advantage of the half-price fare that

[4] Other historians have situated the Geneva Congress in the culture of French-led efforts to establish international scientific standards during the late nineteenth century; Brigitte Schroeder-Gudehus, ed., "Les Congrès Scientifiques Internationaux," *Relations internationales* 62 (1990): 111–211; Robert Fox, *The Savant and the State: Science and Cultural Politics in Nineteenth-Century France*, (Baltimore: Johns Hopkins University Press, 2012), 276–277.

Friedel had arranged.[5] Some made longer journeys from Palermo and Bucharest. Adolf von Baeyer and his friend and former student Emil Fischer took the scenic route, spending a week relaxing and "practicing a little French" in Montreux, on the opposite side of Lake Geneva, before continuing on to the Congress.[6] Many, such as Baeyer and Fischer, got there on the strength of their reputations, others, by helping to lay the logistic and intellectual groundwork for the week's events. Some, including their host, had expected that a rather different assembly would meet long before.[7] But the presence of this particular group of chemists in this particular city augured well for the success of the Congress on which they were about to embark.

The eminent chemist Carl Graebe, professor of organic chemistry at the University of Geneva, was an ideal host for Friedel's Congress. Graebe was not only a highly respected organic chemist but a thoroughly international figure: he worked in Geneva, visited Paris, and remained steadfastly German in his habits and friendships.[8] Graebe harbored a longstanding interest in matters of nomenclature, had participated eagerly in the 1889 meeting, and offered feedback on the preliminary work of Friedel's subcommission.[9] He commanded the respect of his fellow German organic chemists, and maintained close personal ties with many of them.

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[5] Comité local to Congress invitees, 5 Apr 1892, BGE, Ms. Fr. 3423, 160. The British delegates particularly appreciated this discount; Henry Armstrong, "The International Conference on Chemical Nomenclature," *Nature* 46, no. 1177 (May 19, 1892): 56.

[6] Fischer to Baeyer, 22 Mar 1892, Emil Fischer papers, Box 36. The British delegate Henry Armstrong noted that some delegates had difficulty expressing or understanding the intricacies of nomenclature questions in French; Armstrong, "International Conference on Chemical Nomenclature," 57.

[7] Graebe wrote to Liebermann in January 1890 that the Parisians would soon circulate their proposals, and that the (original) International Commission would likely assemble over the coming summer. Graebe to Liebermann, 2 Jan 1890, quoted in Elisabeth Vaupel, "Carl Graebe (1841-1927): Leben, Werk und Wirken im Spiegel seines brieflichen Nachlasses" (PhD dissertation, LMU München, 1987), 282.

[8] Pictet, *Souvenirs et travaux d'un chimiste* (Neuchâtel: Editions de la Baconnière, 1941), 43–45, 52; Vaupel, "Carl Graebe." Graebe remained much attached to the German language and culture during his long tenure in the predominantly French-influenced Geneva, founding a German club, drinking Bavarian beer, and celebrating the Kaiser's birthday, though he didn't seem to show any favoritism toward students of German descent; Ibid., 384–85. He once wrote to his friend Heinrich Caro that his idea of the good life was "a German laboratory in romantic terrain" Graebe to Caro, 3 Feb 1890, Deutsches Museum, Munich, HS. 02059. Graebe attained the former, if not the latter, when he was called to a professorship in his native Frankfurt in 1906.

[9] For example, in 1868, Graebe proposed extending A.W. Hofmann's nomenclature for open-chain hydrocarbons to aromatic ring hydrocarbons; Carl Graebe, "Ueber die s.g. Additionsproducte der aromatischen Verbindungen,"

The city of Geneva was an equally fortunate setting for the congress. Nestled between Italy, Germany, France, and Austria, Geneva was accessible to chemists from all over Europe. Locations in France or Germany might have been equally accessible, but the choice of a site in either of these nations would likely have invoked national prejudices and resentments. The prospect of the active involvement of either the French or German chemical society in planning the Congress might have been a still greater threat; the attendant formalities could have swollen the list of obligatory invitees and swallowed up much of the time available for discussing nomenclature. Yet Geneva was no chemical wilderness. The city was home to a brood of socially well-connected young chemists happy to assist in organizing the meeting, two of whom, Amé Pictet and Philippe-Auguste Guye, had studied with Friedel in Paris.

French, German, and Italian were all spoken in the city, easing the burden of travel for visitors conversant in any of these languages. But the primary language of the city was French, helping to ensure that the Congress would be held predominantly in that language – a crucial consideration, if the reformed nomenclature was to be of use to Friedel in transforming the language of French chemical pedagogy. Indeed, Geneva was already the site of the kind of chemistry that Friedel (and his mentor Wurtz before him) promoted: German chemistry in the French language.[10] As the Geneva minister of education remarked at the outset of the meeting, the city's position had made it a popular site for international assemblies.[11] Geographically,

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Annalen der Chemie 146, no. 1 (1868): 66–73. A week and a half after the 1889 Congress, he wrote to his colleague Heinrich Caro, "I've taken part in the Congress of Chemists and involved myself rather eagerly in the debates over nomenclature. I intend to report on them in Heidelberg and thus perhaps contribute to initiating an international understanding." Graebe to Caro, 11 Aug 1889, quoted in Vaupel, "Carl Graebe," 330. Graebe was among the foreign chemists whose contributions the subcommission noted in its report; "Rapport de la Sous-Commission," *Association Française pour l'Avancement des Sciences: Compte rendu* 21 (1892): 394.

[10] Alan J. Rocke, *Nationalizing Science: Adolphe Wurtz and the Battle for French Chemistry* (Cambridge, MA: MIT Press, 2001).

[11] "Chronique Locale. Congrès de chimie," *Journal de Genève*, April 21, 1892, <http://www.letempsarchives.ch/>, 3.

linguistically and politically, this was particularly true for Friedel, in his quest to secure both effective rules of nomenclature and an international imprimatur to stand behind them.

In order to achieve either goal, Friedel needed the support of the right collection of chemists. He had been content to leave invitations to the 1889 Congress of Chemistry in Paris in the hands of an ill colleague and an unknown student. In 1892, Friedel took charge of invitations himself, putting the weight of his name and reputation behind the request.[12] As with the nomenclature commission formed at the end of session on chemical naming at the 1889 congress, Friedel sought to represent a broad collection of nations among the invitees. He sent seventy-one invitations to chemists working in fourteen European nations and America.[13] In doing so, he made use of the international connections he had forged in his mentor's laboratory and his own. Ten invitees from seven countries outside France had studied with Friedel or his teacher Wurtz, as had at least ten of the fifteen French chemists on Friedel's list.

In addition to his students and former labmates, Friedel sought out the participation of chemists with the professional stature to lend authority to the work of the Congress. Friedel sent invitations to the aging founders of structural organic chemistry August Wilhelm Hofmann and Edward Frankland, to his fellow current leaders of the field, such as Baeyer, and to younger rising stars such as Emil Fischer, William Ramsay, and Arthur Hantzsch. The ages and nationalities of the invitees thus mirrored the contemporaneous social geography of chemistry, apart from the overrepresentation of young chemists from Paris and Geneva who took responsibility for much of the planning.

Famous chemists like Baeyer might lend a reformed nomenclature authority, but they would not necessarily have much direct say over whether and how it would be applied. Editors

[12] Charles Friedel, invitation to the Geneva Congress, 15 Mar 1892, BGE, Ms. Fr. 3438, 338.

[13] The total includes thirty-four chemists who attended the Congress and thirty-seven others who could not attend, but sent messages of support for the nomenclature reform effort; "Congrès de nomenclature chimique," *Bulletin de la Société Chimique de Paris* 7 (1892): xxiv. It is possible that other chemists were invited and failed to respond.

of chemical journals, on the other hand, had the power to promulgate and enforce the Congress's decisions as norms of chemical communication. Friedel invited the editors of leading French, German, British, and American chemical periodicals, as well as the most influential chemical editor of all, Friedrich Beilstein, who was then preparing the third edition of his *Handbuch* for publication.[14] Not all of these editors attended; Friedel could not have held out much hope that Ira Remsen would make the trip from Baltimore, and he did not. By inviting them, however, Friedel ensured that the decisions of the Congress would not catch the editors unaware. Editors might well be more inclined to enforce the decisions of a Congress to which they had been invited than they would be for rules handed down by a group that had not consulted them.

About half of the invitees attended. A few of those who did not, such as Hofmann and Frankland, may have stayed home for reasons of age and health. Others were likely deterred by distance. None of the three Russian invitees – Beilstein, Dmitri Mendeleev, and Nikolai Menshutkin – made the trip from St. Petersburg, nor did the Iberians who were invited. Of the five Berliners on Friedel's list, only Ferdinand Tiemann, editor of the *Berichte*, attended. On the other hand, the Romanian chemist Constantin Istrati, who was passionate about nomenclature and devoted to Friedel, his doctoral advisor, traveled from Bucharest to Geneva.[15]

Research interests also divided those who attended from those who did not. Wilhelm Ostwald and François-Marie Raoult, two of the founders of the emerging field of physical chemistry, declined their invitations. The structure and naming of organic compounds bore little relevance to their investigations of such phenomena as dissociation and reaction rates.

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[14] These included Maurice Hanriot of the *Bulletin de la Société Chimique de Paris*, Ferdinand Tiemann of the *Berichte der Deutschen Chemischen Gesellschaft*, Jakob Volhard and A.W. Hofmann of the *Annalen der Chemie*, Ira Remsen of the *American Chemical Journal*, and William Crookes of the *Chemical News*.

[15] Verkade discusses Istrati's efforts in nomenclature, including composing a gargantuan textbook on the topic, at length; Verkade, *A History*, 320–371. Chapter 4 will touch upon Istrati's peculiar textbook.

Indeed, Ostwald inveighed against the synthetic mania of his organic colleagues, and would later formulate the anti-atomist philosophy of energetics.[16] Four British chemists whose research focused on inorganic and physical topics also declined their invitations.[17] Friedel did not invite the physical chemists Jacobus van't Hoff and Svante Arrhenius, nor did he include his anti-atomist Parisian rivals Berthelot and Jungfleisch. If present, they might have disputed the very premise of the Geneva Congress: bringing the names of organic compounds into clear correspondence with their constitution, understood as a network of atoms and bonds. Instead, some were not invited; for others, their disinterest in the project apparently outweighed any feeling of opposition to it.

Notwithstanding the declined invitations and omissions, with such chemists as Henry Armstrong, Baeyer, Stanislao Cannizzaro, Fischer, Graebe, Adolf Lieben, Ramsay, and Friedel himself in attendance, the Congress secretary Pictet could justly characterize the reformers who gathered at Geneva as "the greatest authorities in all the nations of Europe."[18] (*Figs. 1-3*) Their particular area of authority was structural organic chemistry. Friedel's invitees, winnowed by self-selection, were virtually all specialists in organic chemistry who had grown up professionally using and elaborating structure theory and structural formulas.[19] In contrast to the attendees of the Karlsruhe Congress three decades before, the Geneva delegates *began*

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[16] On Ostwald's criticism of contemporary organic chemistry, see John W. Servos, *Physical Chemistry from Ostwald to Pauling: The Making of a Science in America* (Princeton: Princeton University Press, 1990), 3–4, 64.

[17] Crookes, James Dewar, Henry Roscoe, and Edward Thorpe.

[18] Amé Pictet, "Le congrès international de Genève pour la réforme de la nomenclature chimique," *Les archives des sciences physiques et naturelles* 27 (May 1892): 486.

[19] The sole exception was the Italian chemist Alfonso Cossa, who played a negligible role in the Congress. On Cossa, see George B. Kauffman and Ester Molayem, "Alfonso Cossa (1833–1902), a Self-Taught Italian Chemist," *Ambix* 37, no. 1 (1990): 20–34. Philippe-Auguste Guye, who missed much of the Congress because of compulsory military exercises, was a physical chemist; however, he had trained with Friedel, and the physical structure of molecules, not kinetics, was his primary interest. Amé Pictet, "Nomenclature organique," in *Autobiographie* (manuscript of 1935-37), vol. 2, Fondation des archives de la famille Pictet, DJG 07/2009. Though Achille Le Bel was best known for his hypothesis of the tetrahedral shape of the carbon atom, during the 1880s Le Bel's research focused on the chemistry of petroleum and its component hydrocarbons; Claude Millot, "Achille Le Bel," in Laurence Lestel, ed., *Itinéraires de Chimistes 1857-2007* (Paris: EDP Sciences, 2007), 321–26.

their Congress with a shared working understanding of the constitution of compounds and the formulas used to represent it.[20] They included influential editors who could put the decisions of the Congress into effect and require, rather than merely suggest, that others do so. Their very first decision – naming Friedel chair for the entirety of the Congress, instead of the diplomatic but inefficient practice of rotating the presidency among savants of different nations – signaled the delegates' commitment to action.[21] The Congress was equipped for and committed to accomplishing something over its four days in Geneva.

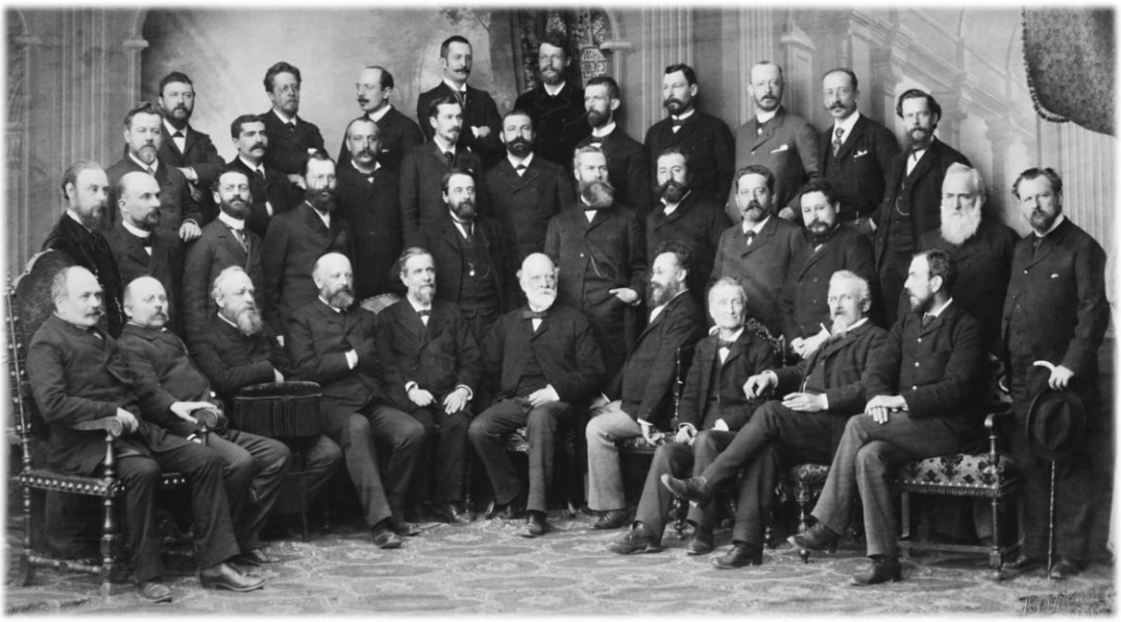

Figure 1. The delegates to the Geneva Congress.[22]

[20] Several of the eldest Geneva delegates – Baeyer, Friedel, Cannizzaro, Gladstone, and Lieben – had attended the Karlsruhe Congress. Two of its organizers, Kekulé and Wurtz, were mentors to Baeyer and Friedel, respectively. Cannizzaro had played a starring role at this meeting, delivering a speech that persuaded many attendees (including a young Dmitri Mendeleev) of the fruitfulness of the system of element weights most consistent with the atomic theory. On the Karlsruhe Congress, see the discussion of this meeting in Chapter 1 and the works cited in note 19 therein.

[21] Amé Pictet, Minutes of the Geneva Congress (manuscript), 169, BGE, Ms. Fr. 3423; Armstrong, "International Conference on Chemical Nomenclature," 56.

[22] *Front row:* Philippe Barbier, Emanuele Paternò, **Carl Graebe, Adolf von Baeyer**, **Stanislao Cannizzaro, Charles Friedel, Adolf Lieben,** John Hall Gladstone, Alfonso Cossa, **William Ramsay**. *Second row:* **Albin Haller**, Paul Cazeneuve, **Emil Fischer, Maurice Hanriot, Achille Le Bel,** Ernst von Meyer, **Constantin Istrati, Ferdinand Tiemann, Louis Bouveault,** Denis Monnier, Olivier. *Third row:* **Henry Armstrong,** Rudolf Nietzki, Léon Maquenne, Maurice Delacre, **Auguste Béhal**. *Back row:* Léon-Albert Arnaud, **Arthur Rudolf Hantzsch**, **Alphonse Combes, Amé Pictet,** Zdenko Hans Skraup, Philippe-Auguste Guye, Frédéric Reverdin, Emilio Noelting, Michel Fileti, Antoine Franchimont. (Identities per Crosland, *Historical Studies*, 351.) Names of **chemists discussed in this dissertation**

Allemagne.

MM.
A. von Baeyer (Munich).
E. Fischer (Würzburg).
E. von Meyer (Leipzig).
E. Noelting (Mulhous).
F. Tiemann (Berlin).

Angleterre.

H.-E. Armstrong (Londres).
J.-H. Gladstone (»).
W. Ramsay (»).

Autriche.

A. Lieben (Vienne).
Z. Skraup (Graz).

Belgique.

M. Delacre (Gand).

France.

A. Arnaud (Paris).
Ph. Barbier (Lyon).
A. Béhal (Paris).
L. Bouveault (Paris).
P. Cazeneuve (Lyon).

MM.
A. Combes (Paris).
C. Friedel (»).
A. Haller (Nancy).
M. Hanriot (Paris).
A. Le Bel (»).
L. Maquenne (Paris).

Hollande.

A.-P.-N. Franchimont (Leyde).

Italie.

S. Cannizzaro (Rome).
A. Cossa (Turin).
M. Fileti (»).
E. Paterno (Palerme).

Roumanie.

C. Istrati (Bucarest).

Suisse.

C. Graebe (Genève).
P.-A. Guye (»).
A. Hantzsch (Zurich).
D. Monnier (Genève).
R. Nietzki (Bâle).
A. Pictet (Genève).

Figure 2. List of delegates to the Geneva Congress, after the roster printed in the Congress proceedings.[23]

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are in **bold**. Reverdin, a member of the local organizing committee, was not included in official lists of Congress delegates. It is not clear what role the man whom Crosland identifies as "Olivier" played in the Congress. Photo: Deutsches Museum, Munich, Bildstelle, BN 32547, copyright Photo Deutsches Museum. By kind permission of the Deutsches Museum.

[23] Pictet, "Congrès international de Genève," 487.

Membres de la Commission qui se sont excusés de ne pouvoir assister au Congrès :

MM. Athanasesco (Bucarest), Balbiano (Rome), Bauer (Vienne), Beilstein (Saint-Pétersbourg), Bonkowski-Bey (Constantinople), Calderon (Madrid), Ciamician (Bologne), Cleve (Upsala), Crookes (Londres), Dewar (Cambridge), van Dorp (Amsterdam), Ferreira de Silva (Porto), Frankland (Londres), A. Gautier (Paris), Goldschmiedt (Prague), E. Grimaux (Paris), Henry (Louvain), A.-W. v. Hofmann (Berlin), Hoogewerff (Delft), Ladenburg (Breslau), Liebermann (Berlin), Lippmann (Vienne), Lobry de Bruyn (Amsterdam), Lourenço (Lisbonne), Menschutkin (Saint-Pétersbourg), Mendelejeff (Saint-Pétersbourg), V. Meyer (Heidelberg), Ostwald (Leipzig), Piccard (Bâle), Pinner (Berlin), Raoult (Grenoble), Remsen (Baltimore), Roscoe (Manchester), Schutzenberger (Paris), Thorpe (Londres), Volhard (Halle), Witt (Berlin).

Figure 3. Chemists who declined invitations to participate in the Geneva Congress.[24]

Making nomenclature official

On Tuesday morning, the chemists who had arrived in Geneva the evening before proceeded up the hill of the Vieille Ville to the Salle du Grand Conseil for the Congress' opening session.[25] After a local official welcomed the delegates to Geneva, Friedel began the proceedings by introducing the fifty-three page report that he and his fellow subcommission members had prepared as a basis for the discussions of the Congress. The product of two years of labor, forty-five meetings, and the feedback of several non-French colleagues, it described an ambitious reinterpretation of the mandate conferred upon the subcommission at its creation.[26] Instead of seeking to unify the nomenclature of particular families of compounds, Friedel and his band had conceived an entirely new understanding of nomenclature that would unite the

[24] "Congrès de nomenclature chimique," xxiv.

[25] "Réunions-convocations-concerts," *Journal de Genève*, April 19, 1892, http://www.letempsarchives.ch/.

[26] Baeyer, Beilstein, Graebe, Istrati, and Lieben were among those who had offered comments on a preliminary draft; "Rapport de la Sous-Commission," 394.

names of all organic chemical substances under a system governed by a few core principles. Before the morning's session was through, however, Friedel's plan would be supplemented and in part supplanted by Adolf von Baeyer's vision of how and where such a system of names should work.

The International Commission formed in Paris in 1889 had originally been charged with unifying the nomenclature of each of three groups of compounds: compounds with rings containing nitrogen, compounds with multiple functions, and benzene derivatives with more than one substituent.[27] In so doing, reported Friedel to the assembled chemists, he and his subcommission had been "led to examine" two further matters:

> d) The rules to follow in naming each chemical function by a consistent suffix or a prefix inserted into the name;
>
> e) The denominations to apply to radicals and in general to functional groups that present themselves most often in molecules. [28]

As Friedel presented it, the subcommission had decided that they could not unify the nomenclature of complex substances without first setting out rules for naming their simpler component parts. This meant, however, that the proposed reform would address not only the substances at the leading edge of synthesis that had prompted the reform effort, but also simple organic chemical substances with well-established names. The French chemists were explicit about the scope of their endeavor; building upon the 1889 Congress, "the propositions adopted, at the end of numerous meetings, by the Subcommission… allow, we believe, the clear, succinct, and unambiguous naming of all known organic chemical substances." Friedel and his subcommission had been tasked with unifying the outer reaches of chemical vocabulary, but they proposed a universal grammar for forming acceptable chemical names.

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[27] "Congrès international de chimie," 1393.

[28] "Rapport de la Sous-Commission," 393–395.

The subcommission had adopted seven principles to guide its pursuit of this goal:

1. Change current nomenclature as little as possible, except to complete and standardize it in order to avoid confusion and superfluous synonyms.
2. As much as possible, found the laws of the reformed nomenclature on the general principle of *substitutions*, except where it's necessary to express a direct addition.
3. In all of the names of derivatives of the same family – of the same hydrocarbon, the same alcohol, etc. – include a shared root that indicates their relationship and their parent.
4. Express the groups which characterize the functions of these molecules by prefixes and suffixes added to this root.
5. Conserve established names such as camphor, xylene, naphthalene, and terebine as roots, without indicating the structure of these familiar compounds or radicals.
6. As much as possible, adopt or construct names that are spoken and written according to the chemical formula, separating out each radical and indicating their positions following a fixed order.
7. Conserve common names such as alcohol, camphor, chloral, quinine, indigo, tyrosine, and urea, since they have passed into regular usage, but without prejudice against the corresponding new names.[29]

This was a blueprint for nomenclature reform that took into account both the benefits of rational nomenclature and the drawbacks of replacing established terms with neologisms. The second and sixth of these principles tethered the reform to structural formulas and the tenets of structure theory. Names were to be based on structural formulas, interpreted according to the principle of substitution, which classified compounds containing functional groups like the chloride –Cl or the alcohol –OH as derivatives of simpler compounds containing hydrogen atoms in place of this "substituent." The third and fourth principles articulated the linguistic form in which the reformed names would express chemical relationships.

[29] Ibid., 393–394.

Substitution products of the same parent compound would share a common root, and compounds possessing the same chemical function would share a common prefix or suffix. Over the preceding decades, chemists had usually applied the most familiar conventions of rational nomenclature in these ways; the principles were not novel, but they had not previously been stated generally, formally, and explicitly.

With the first, fifth, and seventh principles, Friedel and his colleagues sought to rein in their reformist impulses with a commitment to lexical conservatism. They wanted, after all, to create a nomenclature that chemists could and would use, and their decades of conflict with the anti-atomists in Paris had taught them how suspicious chemists and academic institutions could be of changes to nomenclature and notation. The subcommission's principles described a reform that was sweeping but inclusive. Friedel and his colleagues sought to supplement the portions of the existing nomenclature that were working well for chemists with procedures for making sure all organic substances could be named consistently and effectively.

Whether they multiplied synonyms or aimed to eliminate them, previous proposals addressing organic chemical nomenclature had always addressed the nomenclature of some particular set of compounds. So, too, did the questions that Friedel's subcommission had been charged to answer. *Nomenclature* was always succeeded by *of*; nobody spoke of organic chemical nomenclature in general. Friedel and his subcommission changed this. The scheme that Friedel presented to his fellow delegates at the outset of the Geneva Congress redefined nomenclature as a broad, continuous field that addressed itself at once to all organic substances. Order was to be established within this field in three ways: through a piece-by-piece correspondence between name and structural formula, through establishing such correspondence in a consistent fashion across many different classes of substances, and through incorporating well-worn names that had proven their use.

Having laid out the principles on which the subcommission had based its efforts, Friedel opened the floor for discussion of the particular recommendations that made up the bulk of the report, beginning with procedures for naming the hydrocarbons. Baeyer, however, was not yet ready to move on. He requested that, prior to entering into a detailed discussion of any particular family of compounds, the Congress answer a few basic questions that had not yet been settled to his satisfaction. In what settings was nomenclature reform needed most urgently? What principles did the Congress need to prioritize in order to ensure that the new names were effective in these settings? How far was the reform to extend?

Friedel and his fellow subcommission members had sought to accommodate the various demands placed on names by different chemists and in different settings by building flexibility into their proposed rules of nomenclature. They proposed allowing chemists to select among various nomenclature procedures that might apply to a compound.[30] In response to Baeyer's challenge, Friedel and his colleagues remarked that this freedom of choice was especially useful in pedagogy, noting "how advantageous it is for instruction to be able alternately to cast light on one or another of the functions of a given compound, by successively employing several names to designate it." They cited the example of aniline, a coal-tar distillate of paramount importance in the synthetic dye industry. Distinct aspects of the chemical behavior of this compound were associated with two structural units, an amine functional group and a benzene core. An instructor could refer to the substance either as *phenylamine* or *aminobenzene* to emphasize these different properties, and could use similar names to illustrate the compound's relationships to other substances with similar chemical behavior.[31]

[30] Ibid., 397–398.

[31] Pictet, "Congrès international de Genève," 489.

Baeyer, however, was primarily concerned to establish a reformed nomenclature for the purposes of chemical lexicography.[32] He worried that rules of the sort that Friedel proposed would actually worsen the proliferation of synonyms.[33] A year before the Geneva Congress, Baeyer had sent Friedel's subcommission a proposal for determining a unique, official name by which each compound could be designated in indexes and dictionaries. Bouveault had fleshed out another method for determining such official names.[34] Ultimately, however, the subcommission dismissed the proposals, deciding that either method would produce intolerably long and complicated names, and that set alongside existing nomenclature practices, such "unique" names would just be that many more synonyms.[35]

At Geneva, Baeyer clarified his position: the demands of indexing were so pressing that the Congress should focus *exclusively* on establishing for each compound "an official name, the translation of its constitution, which would permit it to be retrieved easily in tables and dictionaries."[36] Baeyer proposed that the length and euphony of these official names be relegated to secondary considerations. In journal articles, lectures, classrooms, and other settings where such concerns were significant, he suggested, chemists would remain free to use whatever names they wished.

After a long discussion with Lieben, Bouveault, and Friedel, Baeyer won the assent of the President and the other delegates to his plan. The Congress agreed unanimously to adopt Baeyer's version of systematic nomenclature as its first resolution: "Alongside the usual procedures of nomenclature, an official name will be established for each organic compound,

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[32] Amé Pictet, Minutes of the Geneva Congress (manuscript), 170.

[33] Pictet, "Congrès international de Genève," 490.

[34] Louis Bouveault, "Mémoire annexe," *Association Française pour l'Avancement des Sciences: Compte rendu* 21 (1892): 445–455. It is not clear whether this portion of the report was presented at the Congress; at any rate, Bouveault's proposal, a forbidding minefield of concatenated Greek letters, seems not to have been discussed.

[35] "Rapport de la Sous-Commission," 394.

[36] Pictet, Minutes of the Geneva Congress (manuscript), 170.

permitting it to be found under a unique heading in indexes and dictionaries."[37] With that, they broke for lunch.

The decision amounted to a qualification – or rather, a division – of the field of nomenclature which the subcommission had defined as the object of their proposals. The notion of a field encompassing all organic substances and all of their names remained, cast as "the usual procedures of nomenclature." Following Baeyer's lead, the Congress carved out a separate field of "official names," whose application was restricted to reference works.[38] The subcommission's vision of order, however, no longer applied to either field. Within the compass of indexes, Friedel's commitments to lexical conservatism and to flexibility appeared to have little to offer; in such settings, the Congress determined that the uniqueness of a name and its correspondence to a structural formula were paramount.[39] Elsewhere, on the other hand, the Congress would not attempt any reform at all.

Pictet summarized:

> We will continue to say alcohol, chloroform, sugar, and the chemist who, either in his articles or in the classroom, would use the expressions "ethanol," "trichloromethane," and "hexanal-pentol," which are henceforth the official names of these three substances, would fall into the same ridiculous pedantism as the botanist who in everyday language employed the term "brassica oleracea" to designate "un chou" [a cabbage].[40]

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[37] The journal editors among the delegates were concerned about who would bear the burden of applying the new nomenclature. The Congress accordingly added to this resolution a provision proposed by Hanriot: "The Commission expresses the view that authors should form the habit of mentioning in their articles the official name, in parentheses, next to their chosen name." Ibid.; Pictet, "Congrès international de Genève," 490–491.

[38] With its second resolution, the Congress further restricted the Geneva nomenclature to compounds whose structure had been determined (and could thus be unambiguously converted into a name); Pictet, "Congrès international de Genève," 492.

[39] Pictet accordingly edited the subcommission's list of guiding principles for his official report, eliminating those that mentioned conserving existing names. Compare "Rapport de la Sous-Commission," 393–394 and Pictet, "Congrès international de Genève," 491.

[40] Pictet, Minutes of the Geneva Congress (manuscript), 168.

The structure of chemical function and the function of chemical structure

The group reconvened that afternoon at the Hotel Métropole, where the remaining six sessions of the Congress were to be held. Having established their aim – a set of rules for constructing a unique, official name based on the constitution of each organic compound – they had to determine how to make such a system work.[41] This was a question of the relationship between name, formula diagram, and substance. Once again, the Geneva delegates arrived at a different answer than the subcommission had. In doing so, they articulated not only a new kind of chemical name but also a distinctive way of working with structural formulas.

The session's first topic was the saturated hydrocarbons: compounds made up of carbon atoms linked in open chains, with all of their remaining units of valence taken up by hydrogen atoms. They were commonly named in several different ways, both trivial and rational, but these names were well-established and caused little confusion. Beyond affirming the usefulness of Hofmann's familiar system of rational names, the subcommission had paid these simplest and most chemically inert of the organic compounds little mind.

The Geneva delegates had to be more particular. They quickly agreed that Hofmann's well-known names – *ethane*, *pentane*, and the like – should be uniquely assigned to the "normal" isomers whose carbon atoms were arranged in a linear chain. There were equally familiar conventions for naming branched hydrocarbons – the use of trivial prefixes like *iso-*, or selecting one carbon atom as "methane" and adding prefixes for all of the carbon chains attached to it – but none of these seemed likely to yield a general method of forming unique names.

Lieben thought that the French chemists' approach to a different set of compounds might be of use. For the special case of open-chain compounds containing multiple functional

[41] Pictet, Agenda, entry of 19 Apr 1892.

groups, the subcommission had formulated a more rule-bound alternative to its flexible approach. Béhal, who had been studying the nomenclature of these compounds since 1888 and wrote this portion of the subcommission's report, proposed taking the longest linear chain of carbon atoms in the compound's carbon "skeleton" as a starting point, and numbering the carbon atoms of this chain from one end to the other. The name of the hydrocarbon corresponding to this chain would serve as the root of the name of the compound. Each functional group would be indicated by a prefix or suffix, along with the number of the skeleton carbon atom to which it was attached.[42]

Applying Béhal's procedure, Lieben proposed locating the longest carbon chain in the structural formula of a branched hydrocarbon, taking the name of the hydrocarbon corresponding to this "principal chain" as a root, and then naming the remaining "side chains" as if they were functional substituents.[43] Baeyer suggested adopting Béhal's method of numbering carbon atoms for the branched hydrocarbons, as well.[44] The other delegates agreed to both propositions. Led by Baeyer and Lieben, the Congress applied the subcommission's method for naming a group of structurally complex organic compounds exhibiting a variety of chemical behaviors to the group that was structurally simplest and most chemically inert.

Further, while the saturated hydrocarbons had merited only a few lines of discussion in the subcommission's report, Baeyer sought to make them the basis of the entire Geneva system. He proposed retaining the identity and numbering of the principal chain for all functional derivatives of hydrocarbons – effectively, for all other organic compounds. Béhal disagreed. For compounds containing a functional group, Béhal and his fellow subcommission members advocated selecting a principal chain containing the carbon atom to which the

[42] "Rapport de la Sous-Commission," 414–416. Béhal also specified various criteria for determining the end of the chain where the numbering should start.

[43] Pictet, "Congrès international de Genève," 493–494.

[44] Pictet, Minutes of the Geneva Congress (manuscript), 171.

functional group was directly attached, and numbering the chain from the side closest to this group. Such a procedure, they contended, would make for a more straightforward naming process, shorter names, and – most importantly – names that more clearly expressed the compound's chemical function. Baeyer countered that unlike such "functional nomenclature," his procedure would ensure consistent naming and numbering among different functional derivatives with the same carbon skeleton.[45]

After some debate, Baeyer's proposal won out. As a result, no matter what the substance, the Geneva rules latched on to it through the "ensemble of atoms of carbon directly bonded to one another, forming an invariable skeleton, which is found in all the compounds derived by substitution of the hydrocarbon that contains it."[46] The Geneva nomenclature generated consistent, unique names through the application of a consistent, even algorithmic procedure of naming: take a compound's structural formula, reduce it to a carbon skeleton, identify the longest chain in that skeleton, and so on through fourteen rules of hydrocarbon nomenclature before moving on to functional groups. The Geneva nomenclature was systematic in process as well as product.

For example, take pinacone, a product of the combination of two molecules of acetone that Friedel had discussed in his 1869 doctoral thesis. In addition to this name, Friedel assigned it the name *tetramethyl ethylglycol*. He selected the root "glycol" in order to emphasize the compound's chemical function, a set of properties and characteristic chemical reactions that Friedel had established through painstaking experiment to fit the pattern of other glycols. Friedel justified the structural formulas that he assigned to pinacone and the other glycols, and the interpretive value of structural formulas in general, on the basis of their effectiveness in

[45] Auguste Béhal, "La nomenclature chimique au congrès international de Genève," *Moniteur scientifique* 39, no. 606 (June 1892): 412–413.

[46] Alphonse Combes, "Le congrès international de nomenclature chimique," *Revue générale des sciences pures et appliquées* 3, no. 8 (April 30, 1892): 258.

expressing these analogies in behavior.[47] Friedel's rational name classified the compound according to its chemical function, and served as a building block in his argument that structural formulas could do the same. In the same manner, the subcommission advocated names that directly expressed a particular analogy in chemical behavior.

In contrast, a chemical editor applying the Geneva rules to name pinacone would begin with its structural formula *(Fig. 4)*. Setting aside the two alcohol functional groups, he would identify the longest chain within the carbon skeleton and select the name of the saturated hydrocarbon corresponding to this four-carbon chain, *butane*, as a root *(Fig. 4, I)*. Treating the two remaining carbon atoms as side chains, he would use the single-carbon substituent prefix *methyl-*, adding *di-* to account for both of them, forming *dimethylbutane (Fig. 4, II)*. After numbering the principal chain, he would arrive at the full name of the hydrocarbon skeleton, *2,3-dimethylbutane (Fig. 4, III)*. With the skeleton taken care of, he would then turn to the –OH groups. Adding *di-*, for the two groups, to the functional suffix *–ol*, and using the numbering determined by the hydrocarbon skeleton, he would complete the official name for pinacone, *2,3-dimethyl-2,3-butanediol (Fig. 4, IV)*. At no point in the process was empirical observation of the compound to be taken into account. Instead, the relationship between the Geneva name of an organic compound and its functional behavior and relationships was mediated by the Geneva rules and the structural formulas on which they operated.

[47] Charles Friedel, "Recherches sur les acétones et sur les aldéhydes," *Annales de la chimie et la physique* 16 (1869): 322, 390–397.

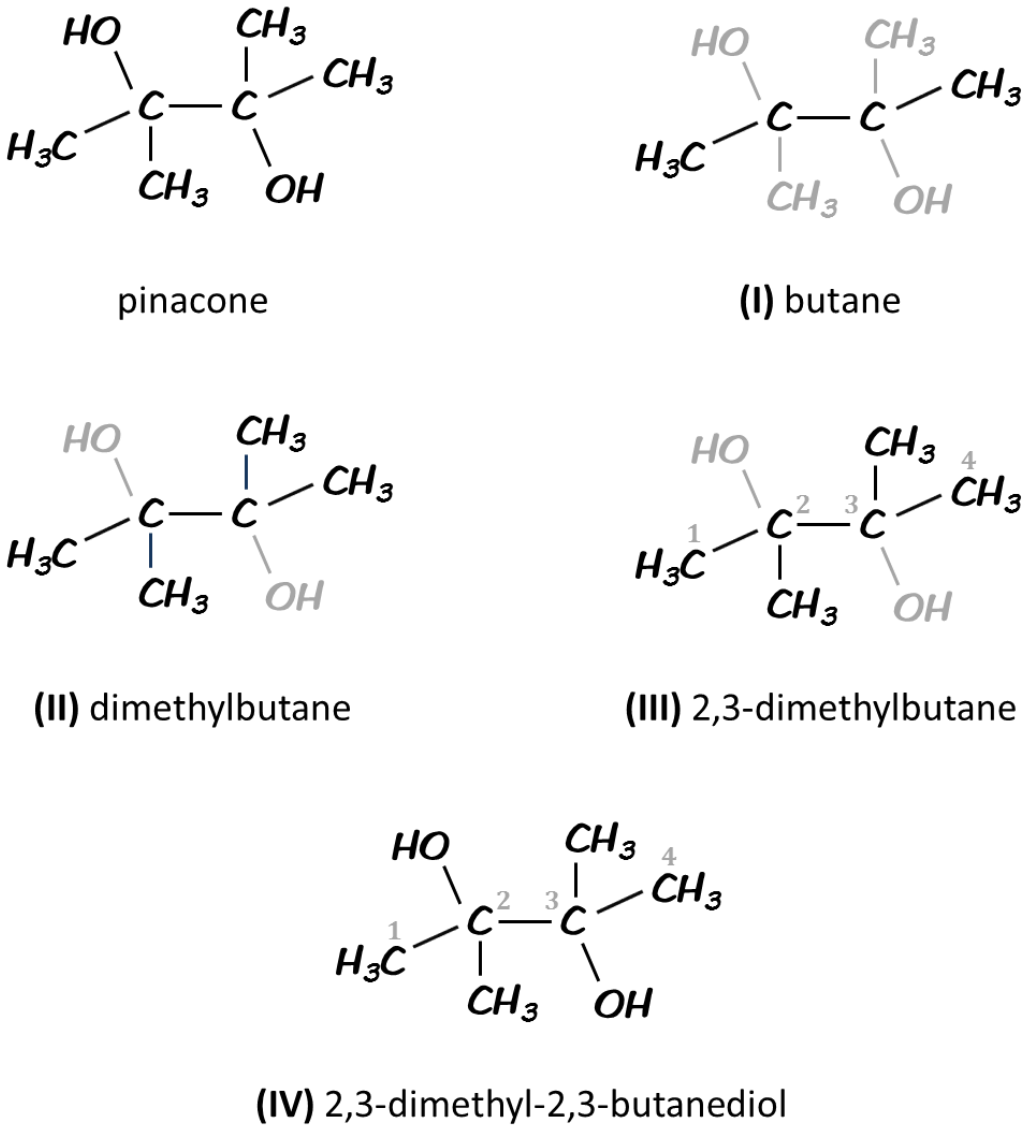

Figure 4. Naming pinacone according to the Geneva rules.

The difference between the subcommission's and the Congress's views on the relationship of formula, name, and substance was sharper still in the case of unsaturated hydrocarbons, compounds in which one or more pairs of adjacent carbon atoms were linked by two or three bonds rather than "saturated" with bonds to hydrogen atoms. As Pictet reported it, Baeyer's proposal for naming unsaturated hydrocarbons was an incontestable correction of a surprisingly fundamental oversight. The subcommission had recommended adopting

Hofmann's schema, which indicated compounds with two, four, six, and eight hydrogen atoms fewer than their saturated analogs with the suffixes *–ene, –ine, –one*, and *–une*.[48] Baeyer, however, noted that this procedure failed to specify the location of the double and triple bonds within the carbon chain. The approach thus named compounds according to their composition, not their structure, violating one of the subcommission's own guiding principles. Instead of the general absence of two or four hydrogen atoms, Baeyer recommended that the suffixes *–ene* and *–ine* represent the specific presence of a double and triple bond, respectively.[49] The other delegates, including the subcommission members, accepted this proposal straightaway.[50]

The subcommission's failure to adopt rules specifying the location of double and triple bonds was puzzling. It wasn't for lack of awareness of the issue; Béhal's dissertation, which Friedel supervised, addressed the thorny experimental problem of pinning down the position of such bonds.[51] Further, the subcommission followed its endorsement of Hofmann's nomenclature with an addendum suggesting the use of names expressing the constitution of unsaturated compounds in certain cases to avoid confusion with other names ending in *–ine* or *–one*.[52] Why did the subcommission recommend a procedure that did not live up to its goal of rational nomenclature, when an alternative that did was sitting three paragraphs below in the same report?

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[48] "Rapport de la Sous-Commission," 400–01.

[49] Pictet, Minutes of the Geneva Congress (manuscript), 171–72; Pictet, "Congrès international de Genève," 497–498. Since the two suffixes are indistinguishable when pronounced in English, the suffix "ine" was later exchanged for "yne."

[50] Verkade singled out Baeyer's method as "a very important positive result of the Conference," on the basis of which "the whole of the modern rules for the nomenclature of acyclic hydrocarbons could be established." Verkade, *A History*, 23.

[51] Charles Friedel, "Rapports sur les thèses de doctorat d'état, 1881-1889," 12 Apr 1888, Pierrefitte-sur-Seine, Archives Nationales, AJ/16/5534, 800–02.

[52] The names of most alkaloids, plant-derived, biologically active compounds such as morphine, quinine, and nicotine, ended in *–ine*. The names of ketones, homologues of acetone, often ended in *–one*; the Geneva Congress applied this ending to all ketones.

The introduction to the subcommission's discussion of hydrocarbons suggests an answer:

> We believe that we need to give to each of the classes of hydrocarbons, saturated, bivalent, quadrivalent, etc., suffixes which mark their valence, this having been measured by the number of atoms of chlorine or bromine or double the number of molecules of hydracids that each hydrocarbon is able to accumulate in its molecule, *without substitution and without the molecule losing its ability to return to its original state.*[53]

For the subcommission, the valence of an unsaturated hydrocarbon was grounded not in a feature of a structural formula, but in experiment. The procedure described above offered a straightforward method for determining this valence, whereas it could be very difficult to determine the specific location of a double- or triple-bond in a carbon chain, as Béhal and Friedel knew firsthand. If a compound's constitution could be established in the former manner but not the latter, it could not be represented by a unique structural formula. The subcommission's proposal nevertheless provided a mechanism for assigning it a name expressing those features of its constitution that had been determined.

One further reason for this approach was epistemic caution. Friedel and his collaborators also wished to avoid creating names that expressed a degree of structural detail not warranted by experiment. They wrote:

> Instead of employing interminable, obscure names, often founded on dubious hypotheses and incomplete and uncertain proofs, names having the pretension of expressing definitively the entire constitution of the compound, it would be better, in general, to limit ourselves to expressing clearly by the name of the substance its relationships with the other substances of the natural family to which it is known to belong and its characteristic and experimental functional properties.[54]

[53] "Rapport de la Sous-Commission," 400. Emphasis in original.

[54] "Rapport de la Sous-Commission," 398.

To be sure, a good many of the names presented as examples in the subcommission's report did express "the entire constitution" of the compounds to which they referred. But the French chemists advocated a system of nomenclature that would not reach beyond the bounds of experimentally attested properties and relationships in order to do so.[55]

"What is important above all," Pictet wrote in his summary of the decisions of the Geneva Congress, "is that the official name be the faithful translation of the constitution of the compound, and that it represent the constitution to the mind just as the structural formula does."[56] Over their two years of lexical labor preparing for the Congress, Friedel and his fellow subcommission members had also sought to develop chemical names that worked "just as the structural formula does." The distinct relationships that the subcommission and the Congress hoped to forge between name, diagram, and substance reflected the differing goals of each group.

Friedel and his allies aimed to reinforce the teaching of structure theory in France in a manner that could resist the dogged criticism of atomic skeptics in Paris. To that end, they crafted proposals for a nomenclature that expressed experimentally-determined chemical behavior through the interpretive lens of structure theory, just as the structural formulas did. According to the subcommission's proposal, systematic names and formula diagrams would each represent organic compounds in the same manner, grounded ultimately in observations of chemical function.

Baeyer's goal, in contrast, was to bring the advantages of structural formulas to bear on a problem to which the diagrams themselves couldn't be applied: determining a unique

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[55] In fact, Béhal found fault with Baeyer's proposal on the grounds that apparent similarities in chemical constitution, as expressed in structural formulas, could obscure differences in behavior. For example, Baeyer's scheme assigned the suffix *–diene* to compounds containing either two adjacent or two non-adjacent double bonds. However, the former, commonly known as allenes, displayed markedly different chemical behavior than the latter; Béhal, "Nomenclature chimique au congrès international," 413.

[56] Pictet, "Congrès international de Genève," 491.

heading for each compound in an alphabetically-ordered index. Guided by Bayer and Lieben, the Geneva delegates built a set of rules for systematically disassembling a structural formula and naming and numbering its pieces in a determined order, such that the process could be reversed equally systematically to regenerate the diagram from the official name. The proximate referent of a name formed according to the Geneva rules was not the compound at all, but the structural formula. Geneva names stood in for formula diagrams where the latter could not be used and summoned these structural formulas, when needed, to represent the compounds themselves.

A "solid and durable foundation"

By midmorning on Wednesday, the second day of the Congress, the core framework of the Geneva nomenclature was complete. The decision to establish unique names, the determination to derive all other names from those of the hydrocarbons, and the fourteen rules for naming hydrocarbons left little room for further substantive change to the method that the delegates were codifying. These resolutions diverged from the subcommission's report, but Friedel and his cohort did not express dissatisfaction – at least not on the record. After all, if they had lost the battle for the specific form of the new nomenclature, they were winning the war of convincing the assembled chemists to agree to *some* collection of nomenclature rules. As British delegate Henry Armstrong wrote of the Congress's opening sessions, "gradually, the doubts which many entertained as to the possibility of devising a practical rational scheme of nomenclature were dispersed, and ere many hours had elapsed the sympathies of all present were enlisted on behalf of the work."[57]

Still, much of "the work" remained to be done. Straight-chain hydrocarbons were a small and rather chemically uninteresting corner of the world of organic compounds. The

[57] Armstrong, "International Conference on Chemical Nomenclature," 57.

delegates dedicated the remainder of their time in Geneva to extending the nomenclature to as many compounds as possible, while trying to avoid creating inconsistencies.

What counted as an inconsistency in nomenclature was itself a matter of debate. The question arose in the delegates' discussion of how to name the organic acids. The chemical unit associated with the acid function was –COOH. In its report, the subcommission had recommended considering this carbon atom as part of the carbon skeleton. Bouveault dissented, recommending that this carbon instead be considered part of the acid functional group. Bouveault was concerned about the legibility of individual names and the applicability of the rules to classes of compounds that the Congress had not yet considered. He argued that his approach yielded clear names for substances for which the subcommission's would generate especially awkward ones, and, unlike the latter, could also be applied consistently to the class of aromatic compounds that the Congress had yet to address.[58] Supporters of the subcommission's approach favored preserving the integrity of the rules that they had already codified and consistency among the names that these rules established. They contended that their favored approach would ensure that the names of acids had the same root as those of their closest chemical relatives, and that Bouveault's approach would require introducing qualifications into the hydrocarbon rules.

Lunchtime approached and the delegates remained deadlocked, so Friedel put the matter to a vote. Twenty-four chemists, including Baeyer and Friedel, opted to count the acid group's carbon as part of the skeleton, while nine sided with Bouveault.[59] By majority vote and under the pressure of a ticking clock, the assembled chemists accepted awkward names and

[58] Pictet, "Congrès international de Genève," 502–503.

[59] Pictet, Minutes of the Geneva Congress (manuscript), 174.

future complications in exchange for simplicity and consistency in the application of whatever rules the Congress managed to codify.

As Wednesday drew to a close, the delegates had dispatched only a few functional groups, and Baeyer, Lieben, Fischer, and others were set to depart the following day. Cannizzaro proposed nominating another commission to study the many matters that seemed sure to remain unresolved.[60] But leaving Geneva without at least an approximately complete system of nomenclature would squander the opportunity to establish the new names in the forthcoming edition of Beilstein's *Handbuch* and other settings in which they could be disseminated and tested.

When Friedel brought up the acid anhydrides as the final order of business for Wednesday, Graebe proposed that the "current mode" of designating these substances be retained.[61] This set the pattern for the Congress' final two days, in which the delegates switched gears from intensive debate to churning out resolution after resolution, either rubberstamping subcommission recommendations or conserving "the current nomenclature."[62] A dispute between Friedel and Baeyer over what actually constituted one such "current practice" might have given them pause.[63] However, any such qualms seem to have been outweighed by the imperative of bringing as many compounds as possible within the compass of the Geneva rules. In their final substantive meeting on Friday morning, after half of the delegates had departed, the remaining attendees took up the numbering of benzene derivatives. They approved Combes' long proposal in its entirety, persuaded, Pictet wrote, by the "simplicity and precision of the notation that he proposed." Exhaustion, the lack of time for

[60] Ibid., 175.

[61] Ibid.

[62] Pictet, "Congrès international de Genève," 501–512.

[63] The disagreement pertained to the usual meaning of the prefixes "sulfo" and "thio"; Béhal, "Nomenclature chimique au congrès international," 414.

further debate, and a determination to broaden the officially-sanctioned nomenclature as far as possible likely also played a part.[64]

For several chemical families, including compounds with more than one variety of functional group, the delegates were unable to come to a consensus. Nor could they elect to sanction current practice, for these compounds had no well-established names. Notwithstanding its opening resolution, the Congress acknowledged that it could not responsibly ignore everyday usage when any rule it ratified would be the only available standard for such usage. In reserving the naming of compounds for further study, the Congress called for an "attempt to reconcile the exigencies of spoken nomenclature with those of a terminology applicable to dictionaries."[65] Choosing to create a nomenclature expressly for dictionaries had freed the delegates from worrying too much about infelicitous names that its rules might create – they were just for indexes, not for everyday discourse. For this reason, however, the Geneva delegates came up short where everyday names had not yet been established – precisely those sets of compounds whose nomenclature the International Commission had originally been tasked with unifying.

This irony did not much bother the Congress attendees. They had come to Geneva not to criticize or argue about nomenclature reform, but to carry it out. They carefully limited the scope of their project to "official" nomenclature and prioritized rules that could serve as a starting point for naming any compound whose structure was relatively simple. They set aside substances whose complexity made them difficult to fit into this project, including most of those that Friedel's International Commission had been created to address.[66] According to

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[64] Pictet, "Congrès international de Genève," 512–519. The delegates elected to set aside some difficult questions regarding the position of benzene in the Geneva rules' order of lexical operations; Ibid., 513–514.

[65] Ibid., 509–511, 519–520.

[66] The Congress also ignored structural features that challenged its methodical approach to nomenclature. Baeyer and Hantzsch had both worked extensively on the study of the spatial configuration of compounds that Geneva

Pictet, they felt that, despite the limited scope of their achievements, "the results achieved so far are nonetheless of considerable importance, and the International Congress of Geneva, in establishing the principles of the official nomenclature, will have laid a solid and durable foundation for the reform that it had intended to accomplish."[67] The delegates departed Geneva satisfied that their rules of nomenclature were complete enough to be applied and robust enough to build upon.

Sociability à la nomenclature

The achievements of the Geneva Congress were made possible in large part by its intimacy and collegiality: "the great advantage to be derived from the personal intercourse which such meetings promote," as Armstrong put it.[68] Much of this interaction took place outside of the Congress' twice-daily official sessions. After dining together on Tuesday evening, the chemists proceeded to the theater, where the Geneva minister of education invited them to join him in his lounge and box. The evening's show, the ubiquitous fairy comedy *Le Pied du Mouton*, allowed the delegates to focus their attention on conversation and champagne.[69]

The next evening, the delegates assembled for the official Congress banquet, joined by several of their wives who had accompanied them to Geneva and a number of local

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invitee Victor Meyer had recently christened "stereochemistry." In 1888, Baeyer had coined the terms "cis" and "trans" to describe the isomerism that characterized double-bonds and substituted ring bonds; Adolf Baeyer, "Über die Constitution des Benzols," *Annalen der Chemie* 245, no. 1–2 (1888): 137. Yet as far as the Geneva rules were concerned, cis-trans isomerism didn't count as a difference in molecular structure. The Geneva rules assigned maleic and fumaric acid, the canonical pair of cis-trans isomers, the same name, *butenedioic acid*. On stereochemistry, see Peter J. Ramberg, *Chemical Structure, Spatial Arrangement: The Early History of Stereochemistry, 1874-1914*, (Burlington, VT: Ashgate, 2003).

[67] Pictet, "Congrès international de Genève," 520.

[68] Armstrong, "International Conference on Chemical Nomenclature," 57.

[69] Pictet, Agenda, entry of 19 Apr 1892. *Le Pied du Mouton* (literally, "The Sheep's Foot") was a French comedy written in 1806, featuring slapstick, fairies, and spectacular effects. It was popular throughout the century, especially with middle- and lower-class audiences, and was adapted as a film in 1907; Jack Zipes, *The Enchanted Screen: The Unknown History of Fairy-Tale Films* (New York: Routledge, 2011), 37–38.

dignitaries.[70] As the guests dined on a main course of "sirloin à la nomenclature," Graebe toasted Friedel for his leadership in the nomenclature reform effort. Friedel, in turn, toasted Graebe, Geneva, and Switzerland for their amiable welcome.[71] Baeyer toasted the successive roles played by France and now Germany in driving the discipline of chemistry forward, and drank to the next nation – whether France once again, Russia, Italy, or England – that would undoubtedly soon claim the laurels.[72] The evening concluded with a nightcap at a nearby haunt, the *Taverne du Crocodile*.[73]

The topic of the Congress also contributed to this collegiality. Organic chemical research focused on ever more complex and specific compounds and phenomena, and nomenclature constituted a rare area of shared concern and expertise. The distinctive social and intellectual dimensions of the gathering made the thirty-four chemists in attendance feel, as Emil Fischer later put it, "like one big family."[74] Such ties fostered accord in the Congress's discussions of nomenclature; they also encouraged the delegates to put their professional authority behind the resulting Geneva rules, uniting the assembly as "a mission…which will explain the enterprise to chemists generally."[75] In comparison to the series of congresses of applied chemistry inaugurated in 1894 – "noisy and confusing" affairs, in Fischer's opinion, "which I

[70] At the turn of the twentieth century, the wives of scientists often accompanied their husbands on professional travel. On the road, as at home, both spouses participated in constituting a gendered lifestyle that shaped the production of scientific knowledge; Staffan Bergwik, "An Assemblage of Science and Home: The Gendered Lifestyle of Svante Arrhenius and Early Twentieth-Century Physical Chemistry," *Isis* 105, no. 2 (2014): 265–291.

[71] "Chronique locale. Congrès de chimie," *Journal de Genève*, April 22, 1892, http://www.letempsarchives.ch/.

[72] Armstrong, "International Conference on Chemical Nomenclature," 56–57.

[73] "Chronique Locale," April 22, 1892. For the younger and less well-known Geneva delegates, this professional bonding was itself a significant product of the Congress. Istrati later took advantage of his relationship with Fischer to solicit invitations for students to work in Fischer's lab in Berlin; Istrati to Fischer, letters of 1896-1906, Emil Fischer papers, Box 21. Pictet recalled, "I was treated particularly kindly, I must say, by Emil Fischer; I became close with Bouveault and Skraup. Everyone received me most generously and I found most agreeable this permanent contact with these distinguished colleagues." Pictet, "Nomenclature Organique," in *Autobiographie* (manuscript).

[74] Emil Fischer, *Aus meinem Leben.* (Berlin: Springer, 1922), 135.

[75] Armstrong, "International Conference on Chemical Nomenclature," 57.

have avoided if at all possible" – the Geneva participants looked back fondly on the intimacy and purpose of the Nomenclature Congress.[76]

The reception of the Geneva nomenclature

The delegates' fond memories of the Geneva Congress were mirrored in their enthusiastic accounts of the Congress and its nomenclature. Indeed, for some members of this "mission," their faith in their cause swept aside concerns of consistency and accuracy. In the first few weeks after the Congress, Combes and Hanriot reported on the Congress in Paris scientific newspapers, and Armstrong published an account in *Nature*.[77] Notwithstanding the Congress' very first resolution, all three speculated on how the Geneva nomenclature might be applied in various settings beyond the indexes and dictionaries for which it was intended. The Romanian delegate Istrati went a step farther, immediately setting to work revising a textbook that he had recently published to include Geneva names for all of the substances that he addressed.[78] Presumably relying on personal notes, Hanriot enumerated thirty-one rules established by the Congress; Armstrong appended a list of forty-six to his account.

In mid-May, the Congress secretary Pictet published an account of the Congress and an annotated list of sixty-two rules. Vetted and approved by Graebe and Friedel and circulated to

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[76] Fischer, *Aus meinem Leben*, 135–136. Pictet reflected, "I never had the same experience again, except perhaps thirty years later, at the Solvay conferences." Pictet, "Nomenclature Organique," in *Autobiographie* (manuscript). Christoph Meinel has cited the Geneva Congress as an example of the shift over the course of the nineteenth century from a form of internationalism linking individual chemists as a cosmopolitan republic of letters to an internationalism linking national groups. While this dissertation provides further evidence for the general trend that Meinel diagnoses, I argue, in contrast, that the Geneva Congress and chemists' subsequent international efforts inspired by it marked an effort to reverse this trend. Christoph Meinel, "Nationalismus und Internationalismus in der Chemie des 19. Jahrhunderts," in *Perspektiven der Pharmaziegeschichte: Festschrift für Rudolf Schmitz zum 65. Geburtstag*, ed. Rudolf Schmitz and Peter Dilg (Graz: Akademische Druck- u. Verlagsanstalt, 1983), 225–242.

[77] Combes, "Le congrès international de nomenclature chimique"; Maurice Hanriot, "Le congrès international de nomenclature chimique," *Revue Scientifique* 49, no. 20 (May 14, 1892): 609–13; Armstrong, "The International Conference on Chemical Nomenclature."

[78] C. I. Istrati, *Curs elementar de chimie* (Bucharest: Lito-tipografia C. Göbl, 1893). Friedel arranged for the publication of a French edition and wrote a preface for the translation; Constantin I. Istrati, *Cours élémentaire de chimie*, trans. Alphonse Adam (Paris: G. Carré, 1895).

the Congress delegates, Pictet's text of the resolutions became standard.[79] English, Italian, and Romanian translations soon appeared; German and Russian versions followed in 1893.[80]

Nonetheless, much remained to be done, and not just by the subcommission members who busied themselves drafting new rules to extend the Congress's system to compounds not addressed at Geneva.[81] As Pictet reminded the readers of his account, "Only the application of the nomenclature procedures that have been adopted will determine their utility and the modifications that will need to be made."[82] Some delegates, like Istrati, took the application of the new nomenclature upon themselves. But for the kind of systematic use that would both test and disseminate the Geneva rules, the delegates looked most of all to their Russian colleague Beilstein.[83]

The St. Petersburg chemist was not entirely pleased with the timing of the Congress or his level of involvement in its work. By the time the results of the Geneva Congress reached him, Beilstein had already sent the first three hundred pages of the third edition of his *Handbuch* to the printers. In a letter to Emil Fischer, he complained:

> The new nomenclature is creating no end of worry for me. It is truly a pity that I have been continually held back from it. I've pressed on and on and yet I've never gotten anything until it was too late. Thus, for example, I've introduced unnecessary new names for the alcohols. Publishers, printers, and the public were waiting, and I had to proceed as best I could. My readers are used to rapid supplements and I did not want to suffer a loss of my good name before the whole world.[84]

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[79] Pictet to Friedel (draft), n.d. [May 1892], BGE, Ms. Fr. 3460, Folder 1891-1900; Friedel to Pictet, 17 May 1892, BGE, Ms. Fr. 3438.

[80] Verkade, *A History*, 8, 51; "Resolutions Adopted by the International Commission for the Reform of Chemical Nomenclature Assembled at Geneva from the 19th to the 22nd April, 1892," *Chemical News* 65 (1892): 277–280.

[81] Verkade, *A History*, 18–48.

[82] Pictet, "Congrès international de Genève," 520.

[83] Armstrong, generally quite positive about the Congress, complained that certain delegates had tried to speed things along in order to get as much new nomenclature as possible into Beilstein's third edition of the *Handbuch*. Armstrong felt that the rules were not yet ready to be applied in such a high-profile setting. Armstrong, "The International Conference on Chemical Nomenclature," 58.

[84] Beilstein to Fischer, 4/16 Oct 1892, Emil Fischer papers, Box 9.

Grudgingly, however, Beilstein decided to adopt the Geneva names. He left no doubt about who had convinced him to do so, informing Fischer, "I have come to an agreement with the French, and they will now see in the next installment how I follow and apply the new names. ... I comfort myself that the same names will appear in the supplement to Wurtz' *Dictionnaire* and in my book."[85]

Some delegates found that their colleagues back home were not as easily converted to the nomenclature reform effort as those at Geneva had been. When Armstrong introduced the new system to the Chemical Society of London in June, his compatriots were skeptical. Alexander Crum Brown wondered what would happen to the official name of a substance if its constitution were determined to be different than originally thought. Another chemist observed that the Geneva names were nothing more than "a statement of opinion as to the constitution." Percy Frankland, son of Edward, opined that empirical formulas would in any case provide a better means of organizing a register. Armstrong was not swayed; he maintained that "a system of nomenclature based on formulae had become a necessity of the times" and that it was therefore important that his fellow Britons "do our best to establish as good a system as possible, adapted for use by English-speaking chemists." Little came of his admonition.[86]

In short, those who had attended the Congress thought that the Geneva nomenclature was quite promising; those who had not, did not. It was a testament to Friedel's judicious invitations and the draw of the Congress that this was enough to gain the rules of nomenclature wide circulation, if not general use. Even some of the skeptics understood the stakes of the new definitions of nomenclature that Friedel and Baeyer had wrought. Most perceptive of all was

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[85] Ibid.

[86] Quoted in Henry Armstrong, "Contributions to an International System of Nomenclature. The Nomenclature of Cycloids," *Proceedings of the Chemical Society* 8, no. 114 (1892): 130–131.

Crum Brown, the man who had invented structural formulas in the first place. "We must keep in mind," he observed, "that such systematic names as had been suggested were really names of formulas rather than names of substances."[87] Many of the specifics of the Geneva rules would fall into disuse, but this newly defined relationship among systematic names, structural formulas, and substances would last.

Conclusion

The Geneva Congress did more than introduce a new set of names for organic chemical compounds, or even a new way of naming them. Over four days in April of 1892, Europe's "greatest authorities" on organic chemistry crafted a new meaning of chemical nomenclature, and a new relationship between chemical names, chemical formula diagrams, and chemical substances.

In laying the groundwork for Geneva, Friedel and his band of students and associates redefined nomenclature not simply as the collected vocabulary of chemists but as a continuous field that could be made subject to a flexible grammar grounded in the constitution of chemical compounds, just as structural formulas were. Guided by lexicographic rather than pedagogical concerns, Adolf Baeyer convinced the delegates of the Geneva Congress to redefine nomenclature once more, dividing it into an everyday sphere to be left to its own devices and a realm of official nomenclature, where each name was to be a precise and unique transcription of a structural formula diagram.

Neither Friedel nor Baeyer intended this form of nomenclature to replace well-established trivial names. Friedel's subcommission wished to sanction the use of familiar trivial names within a flexible nomenclature, while Baeyer advocated leaving existing naming practices alone everywhere but in reference texts. But, as Crum Brown recognized, they

[87] Ibid., 130.

nonetheless defined *systematic* nomenclature in terms of a correspondence between name and structural formula, a proximate relation into which the empirical behavior of the substance did not enter. The definition proved enduring.[88]

This entailed not only a different kind of chemical name but a different manner of reading a structural formula. The rules of the Geneva nomenclature did not operate on the chemical properties and relationships that the formula expressed, but on regularities inherent to the diagrams: patterns of connection among atomic symbols that provided purchase for a logical order of operations mapping diagram to name. On the one hand, by enshrining the late-nineteenth-century structural formula as the basis of chemical names, the Geneva Congress helped establish the status of such diagrams as the "iconic vernacular" of chemistry.[89] On the other, the Congress also launched the study of methods for "official" nomenclature as a separate channel of research running parallel to the chemical mainstream.

The last act of the Geneva Congress was to bestow its rules upon those for whose benefit they were intended: chemical editors. The delegates' final resolution invited "the editors of major chemical journals to agree to apply the principles that it has adopted."[90] The editors of the *Berichte der Deutschen Chemischen Gesellschaft* and the *Bulletin de la Société Chimique*, both present at Geneva, did so. However, the editor of the British *Journal of the Chemical Society*, not invited, did not. The reaction of another editor left out of the Congress suggested that the process of creating rules of nomenclature might not be so easily divided from that of putting them into practice: "If it is hoped to rally as many supporters as possible to

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[88] "To be useful for *communication* among chemists, nomenclature for chemical compounds should additionally contain within itself an explicit or implied relationship to the structure of the compound, in order that the reader or listener can deduce the structure (and thus the identity) from the name." Robert Panico et al., *A Guide to IUPAC Nomenclature of Organic Compounds: Recommendations 1993* (Boston: Blackwell Scientific Publications, 1993), xiii. Emphasis in original.

[89] Roald Hoffmann and Pierre Laszlo, "Representation in Chemistry," in *Roald Hoffmann on the Philosophy, Art, and Science of Chemistry*, ed. Jeffrey Kovac and Michael Weisberg (New York: Oxford University Press, 2012), 176.

[90] Pictet, "Congrès international de Genève," 520.

the proposed conventions, it is necessary to allow all those interested a say in it."[91] The Geneva delegates tailored rules expressly to generate names that they considered as suitable as possible for organizing a chemical reference work. Yet those who actually had to carry out this editorial labor were not so sure that the Geneva nomenclature would make their indexes any easier to compose or use. As the next chapter will show, some doubted whether names were the correct tools for that job at all.

[91] Frédéric Seiler, "Le congrès international de nomenclature chimique, de Genève," *Schweizerische Wochenschrift für Chemie und Pharmacie* 30 (May 13 and 20, 1892): 191–195, 202–205, quoted in translation in Verkade, *A History*, 52.

Chapter 3

Systematic Nomenclature and the Structure of Chemical Reference Literature, 1892-1920

In the mid-1920s, an American organic chemist described his process for finding information about any given chemical compound as follows:

> I of course always turn first to the Richter Lexikon and the Stelzner Literatur-Register, and I have occasionally been helped by looking through the formula indexes of the *Berichte*, beginning where the Stelzner Register ends. If what I am interested in is a type of compound rather than any single one, I naturally turn to Beilstein, but often I have found Meyer-Jacobson and even Richter's Organische Chemie more satisfactory. Then, of course, I always consult the Decennial Index and succeeding indexes of *Chemical Abstracts*; the formula indexes are invaluable in this respect.[1]

Of course… naturally…of course. Such certainty would seem to mark the realization of the aims of the 1892 Geneva Congress for the Reform of Chemical Nomenclature, whose delegates sought to bring order to chemical reference works by establishing a system of official names for chemical substances. Yet the editorial enterprises that made such certainty possible departed significantly from the rules codified at Geneva. Indeed, the architecture of each of the crucial reference works described by this anonymous chemist was defined in part by the *rejection* of certain principles of the Geneva nomenclature.

In the immediate wake of the Geneva Congress, the reception of the sixty-two rules of nomenclature codified there had been quite positive. Over the two years following the Congress, the French text of the Geneva Nomenclature – a set of principles for generating unique, official names expressing the structure of chemical compounds, for use in indexes and

[1] Evan Jay Crane and Austin M. Patterson, *A Guide to the Literature of Chemistry* (New York: J. Wiley & Sons, 1927), 223.

dictionaries – was translated into English, German, Italian, Russian, and Romanian.[2] By 1894, the editors of two major chemical journals, the *Berichte der Deutschen Chemischen Gesellschaft* and the *Bulletin de la Société Chimique*, were applying Geneva names in their annual indexes. Even more significantly, the St. Petersburg chemical lexicographer Friedrich Beilstein incorporated the names into the latest edition of his *Handbuch*, an organic chemical reference work of extraordinary scope and detail that had become an indispensable resource for chemists throughout Europe and America. In his account of the meeting, Congress secretary Amé Pictet emphasized that such application of the Geneva rules by editors of chemical publications would promote broader familiarity with the rules among chemists and reveal shortcomings and gaps in the nomenclature, so that they could be addressed promptly.[3] This appeared to be coming to pass. The Geneva attendee Léon Maquenne wrote that Beilstein's application of the rules, in particular, was both a "true success" and a "powerful encouragement to carry on the work that the Congress of Geneva was able only to sketch out."[4]

Maquenne did just that, alongside the other members of the close-knit Parisian Subcommission, the group responsible for the substantial groundwork that had gone into the Geneva Congress. Led, as ever, by Charles Friedel, they continued to work out proposals pertaining to the systematic nomenclature of more complex substances, compounds whose disordered nomenclature had inspired the reform movement but for which the Congress had been unable to settle upon acceptable rules. They presented their steady progress at the annual meetings of the *Association Française pour l'Avancement des Sciences*, sometimes joined by a foreign colleague or two from the International Commission that had met at Geneva. In

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[2] Pieter Eduard Verkade, *A History of the Nomenclature of Organic Chemistry*, trans. S. G. Davies (Boston: Reidel, 1985), 8, 51; "Resolutions Adopted by the International Commission," *Chemical News* 65 (1892): 277–280.

[3] Amé Pictet, "Le congrès international de Genève pour la réforme de la nomenclature chimique," *Les archives des sciences physiques et naturelles* 27 (May 1892): 488, 520.

[4] Léon Maquenne, "La nomenclature chimique au congrès de l'Association Française," *Revue générale des sciences pures et appliquées* 4 (1893): 557.

1897, the Subcommission presented the culmination of its work: a significant revision of the entire Geneva nomenclature, "the result of long study and of trial and error over the course of a great number of meetings," especially on the part of Friedel's nephew and protégé Alphonse Combes. The Subcommission members hoped that the International Commission would reconvene in the near future, adopt these rules that "resolve the problem of nomenclature in a complete and logical fashion for the fatty series," and present them to a broader chemical public.[5]

And then this work ground to a halt. A few months before the 1897 meeting, Combes died suddenly at the age of thirty-eight. A year and a half later, Friedel fell ill and died as well. Deprived of both its leader and its most active member, the International Commission never again met, and the revised 1897 rules withered on the vine. Another international commission addressing organic chemical nomenclature was assembled in 1913, but its work was interrupted by World War I. A sustained international effort in the area would resume only in 1920.[6] Reflecting on this long period of dormancy, the longtime chairman of the latter

[5] "Discussion sur le rapport de la Sous-Commission Française pour la Réforme de la Nomenclature Chimique," *Association Française pour l'Avancement des Sciences: Compte rendu* 26 (1897): 240. For an extended discussion of the so-called Saint-Etienne Nomenclature (named for the site of the 1897 AFAS meeting) and a comparison to the Geneva rules, see Verkade, *A History*, 1–48.

[6] This chapter addresses the period from 1892 to 1920, that is, from the Geneva Congress through the publication of the first collective index to *Chemical Abstracts* and the initial volumes of the fourth edition of Beilstein's *Handbuch*. This is a peculiar chronology for the history of chemistry, in which 1914 and the "Chemists' War" usually figure as a pivotal moment. The war did play a role for editors compiling chemical reference works, but it did so in the background, not on center stage. These enterprises originated well before 1914, and editorial efforts continued steadily across the outbreak, the ebbs and flows, and the conclusion of the war. This is less a testament to some enduring spirit of scientific cosmopolitanism than to the great importance that national chemical societies invested in these works. Nevertheless, these continuities in the work of chemical compilation may serve as a reminder that however disruptive and productive it may have been, World War I was one among many factors shaping the transformations in the chemical sciences at the dawn of the twentieth century.

A more significant effect of the war for the history of systematic chemical nomenclature was the postwar formation of the International Union of Pure and Applied Chemistry and its nomenclature commissions. Chapters 5 and 6 address the Union and these commissions.

commission speculated that "the international work on the nomenclature of organic chemistry had been begun at least twenty years too early."[7]

Yet international commission members were not the only chemists who claimed authority over matters of nomenclature. At the same time as the Geneva delegates were drawing a boundary between everyday chemical names and the "official" nomenclature of indexes and dictionaries, a new cohort of chemists specializing in the compilation of such publications was emerging. Unlike the editors invited to the Geneva Congress, most of these chemists were not members of the professoriate taking on additional responsibilities on a low-paying or volunteer basis. Some were individual entrepreneurs, and an increasing number were employees engaged by national chemical societies seeking more professional management of their ever-growing literary enterprises.[8]

These chemist-editors did not see their role in nomenclature reform as merely a matter of popularizing and testing out rules designed by others, as the Geneva Congress had assumed they would. Embroiled in the business of compiling and publishing chemical literature – a process in which nomenclature loomed large – they were not willing or able to wait for an international commission to design nomenclature rules for their use. Nor were they looking for

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[7] Verkade, *A History*, 51–52. As Chapter 1 has shown, diverse circumstances contributed to the emergence of this "international work" during the late 1880s. Verkade's comment nevertheless suggests the intriguing possibility that the concept of scientific "prematurity" (classically formulated by Gunther Stent), and the attendant historiographic debate regarding the value of the concept, might apply just as much to matters of naming and classification as discovery. See Gunther Stent, "Prematurity and Uniqueness in Scientific Discovery," *Scientific American* 227, no. 6 (December 1972): 84–93; Ernest B. Hook, ed., *Prematurity in Scientific Discovery: On Resistance and Neglect* (Berkeley: University of California Press, 2002).

[8] There is a rich emerging literature on the history of scientific periodicals in the nineteenth century. Highlights include Melinda Baldwin, *Making "Nature": The History of a Scientific Journal* (Chicago: University of Chicago Press, 2015); Alex Csiszar's forthcoming *The Scientific Journal: Authorship and Knowledge in the Nineteenth Century*; Iain P. 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, no. 3 (2014): 397–419.

The history of the scientific journal from the seventeenth century through the present day, focusing in particular on the *Philosophical Translations*, is the subject of a major current research project; see "Publishing the Philosophical Transactions," <https://arts.st-andrews.ac.uk/philosophicaltransactions/> and Aileen Fyfe, Julie McDougall-Waters and Noah Moxham, ed., "350 Years of Scientific Periodicals," special issue of *Notes and Records* 69, no. 3 (2015): 227–352.

an abstract set of nomenclature standards to implement. Rather, they sought nomenclature practices that would make their texts easier to compile and use, and they developed such practices in the process of their editorial labors. Some doubted that a worthwhile system of nomenclature could be conceived in any other way.

This was particularly true for those engaged in compiling abstract journals, annual registers, handbooks, collective indexes, and other reference works. During the 1880s, enterprising chemical editors like Beilstein published new varieties of reference works, texts oriented toward coping with the increased volume of chemical publications and organic chemical substances. Around the turn of the century, editors working for the national chemical societies that took charge of these reference works began to reconceive their compilation not as a series of one-off efforts but as an ongoing process. It was not the problem of *constructing* a reference work, but of *maintaining* it that spurred the new crop of editors to carry out their most intricate and influential nomenclature projects. In the ongoing production of any given text, a system of nomenclature could be an aid or an obstacle, a side concern or a central principle determining how editors and their assistants collected and organized information.[9]

Editors made many of these decisions about index nomenclature in the context of broader efforts by chemical societies to consolidate large swaths of chemical literature under

[9] The effort and significance that chemists invested in these reference works exemplify Geoff Bowker and Susan Leigh Star's observation that "seemingly purely technical issues like how to name things and how to store data in fact constitute much of human interaction and much of what we come to know as natural"; Geoffrey C. Bowker and Susan Leigh Star, *Sorting Things Out: Classification and Its Consequences* (Cambridge, MA: MIT Press, 1999), 326. Across various chronological, geographic, and disciplinary subfields, a number of historians have recently begun to consider large reference compendia not only as historical sources but as objects of historical inquiry in their own right. My study of the practices, technologies, and social order of scientific compilation in this chapter draws upon the example of such works as Ann Blair, *Too Much to Know: Managing Scholarly Information before the Modern Age* (New Haven: Yale University Press, 2010); Eugenia Lean, "Proofreading Science: Editing and Experimentation in Manuals by a 1930s Industrialist," in Jing Tsu and Benjamin A. Ellman, eds., *Science and Technology in Modern China, 1880s-1940s*, (Boston: Brill, 2014), 185–208; James Delbourgo and Staffan Müller-Wille, ed., "Listmania," focus section of *Isis* 103, no. 4 (2012), 710–752; Lorraine Daston, "The Sciences of the Archive," *Osiris* 27 (2012): 156–187; Bruno J. Strasser, "Collecting Nature: Practices, Styles, and Narratives," *Osiris* 27 (2012): 303–340; Bruno J. Strasser, "Collecting, Comparing, and Computing Sequences: The Making of Margaret O. Dayhoff's Atlas of Protein Sequence and Structure, 1954–1965," *Journal of the History of Biology* 43 (2010): 623–660.

their institutional umbrella. A variety of concerns informed these choices. Henry Watts, editor-in-chief of the abstract journal of the Chemical Society of London, introduced one of the earliest sets of rules of chemical nomenclature; his primary aim was cutting costs by making it possible to replace diagrams with names. Paul Jacobson, head of publications at the Deutsche Chemische Gesellschaft (German Chemical Society), saw systematic nomenclature as an obstacle to the compatibility of the various works under his purview, and sought alternative means for indexing and organizing these texts to make it easier for chemists to work across them. The American editors E. J. Crane and Austin Patterson, in contrast, considered systematic nomenclature an indispensable tool for an ongoing compilation process. However, they felt that this tool could be deployed only if editors gave up the Geneva Congress' ambition of forming an absolutely consistent set of rules and made positive use of exceptions instead.

These editors shared one overriding concern: figuring out how to incorporate the ever-growing harvest of chemical substances and information without having to repeat the work of gathering the previous crop. The systems of nomenclature and notation that editors chose, and the manner in which they incorporated such systems into the organization of the text, could have an enormous impact on their ability to build upon past work rather than repeating it, both in the present and in the future. And that could mean the difference between financial solvency or failure for a reference project, shrinking relevance or growing influence for a chemical society, and, ultimately, informational hegemony or dependence for a nation.

Abstract journals and the economics of nomenclature

"Abstracting is a literary form that dates back far in history with no recorded beginning," begins one chemist's history of the topic.[10] The assertion may be a bit over-the-top,

[10] Herman Skolnik, "Historical Development of Abstracting," *Journal of Chemical Information and Computer Sciences* 19, no. 4 (1979): 215.

but it's also indicative of the degree of importance that modern chemists have attached to abstracts in keeping abreast of the progress of their science. These short summaries of significant scholarly publications were a familiar part of chemical literature since at least the 1830s. They were found both in sections within journals that also published original articles and in standalone journals such as Jöns-Jacob Berzelius' annual *Jahresbericht* and the weekly *Chemisches Zentralblatt*.[11]

In the mid-1870s, Henry Watts, longtime editor of the Chemical Society of London's *Journal*, had a problem. As German chemical periodicals published more and more studies in structural organic chemistry, the section of the *Journal* dedicated to abstracts of foreign articles swelled to nearly three-quarters of each number, crowding out the original research that the Society prided itself on supporting. On the advice of a publications reform committee, the Society's Council decided to split the journal into two separate units. The *Journal of the Chemical Society (Abstracts)* would digest the foreign literature for the society's members, while the Society would continue to provide an outlet for original research in *Journal of the Chemical Society (Transactions)*.[12] Watts remained editor of both.

The decision to divide the *Journal* addressed the organizational challenge, but by increasing the total number of pages to be printed, it created a financial one. The calculus was simple: the shorter each abstract, and the fewer the formulas (for which printing costs could be more than three times the cost of plain text), the more abstracts the Society could print.[13] At the beginning of 1879, Watts issued a set of guidelines for the chemists who composed

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[11] Newspaper accounts of scientific meetings and discoveries served a similar function around the turn of the nineteenth century, as did science- and chemistry-focused weeklies that proliferated during the second half of the nineteenth century. See Watts, "'We want no authors'"; Baldwin, *Making "Nature"*; William H. Brock, "The Chemical News, 1859-1932," *Bulletin for the History of Chemistry* 12 (1992): 30–35.

[12] Chemical Society, Minutes of Council, 28 Jun 1877, RSC, AR0142.

[13] Figures on costs for printing plain text vs. formulas are taken from "Agreement between the American Chemical Society and the Eschenbach Printing Company, Easton, PA., for the publication and distribution of Chemical Abstracts," 10 Nov 1913, W.A. Noyes papers, Box 4, folder Journal of the American Chemical Society, 1907–16.

abstracts for the *Journal*.[14] A few of the instructions addressed matters of style and composition – avoid long block quotes, skip over methods sections, read the whole paper before writing the abstract. But the majority addressed matters of nomenclature and notation, in the service of two goals: compressing the abstract into as few lines as possible, and getting rid of any formula diagrams that could possibly be removed.

Watts directed abstractors to use dots instead of dashes to representing bonds, to use synoptic symbols like Ph for C_6H_5 and Ac for $CO.CH_3$., and to use parentheses to indicate the structural position of a subunit grammatically rather than illustrating it graphically with a line break. He laid out a complicated but compact scheme for indicating the substitution pattern of benzene and naphthalene compounds, and mandated using these names in place of hexagonal structural formulas for these compounds (*Fig. 1*). Other instructions focused on nomenclature, such as restricting the use of the suffix *–ol* to hydroxyl derivatives and specifying the use of *ethylene*, *acetylene*, and *allene* for the simple compounds $CH_2=CH_2$, $CH\equiv CH$, and $CH_2=C=CH_2$, rather than alternative synonyms or formulas. While these nomenclature rules appear to be oriented towards standardization, not brevity, it seems most likely that the nomenclature rules, too, were primarily a matter of shortening the abstract. An abstractor who used *pyrrole* instead of *pyrrol* would not feel obliged to draw the structure of a derivative just to illustrate that it didn't contain the hydroxyl group that the –ol suffix might suggest.

Watts created his rules as a means for economizing on space and diagrams in the face of the proliferation of articles to abstract and structural formulas within those articles. But around 1880, as we have seen, chemists were grasping for means of bringing order to their naming practices, and by 1881, Watts was able to boast to Henry Armstrong about the broad

[14] Henry Watts, "Instructions to Abstractors," *Journal of the Chemical Society (Transactions)* 35 (1879): 276–281.

adoption of his nomenclature standards in the US and Australia.[15] A year after that, Watts reprinted his instructions for the use of original authors, "in the hope that they may have some influence in promoting uniformity of Nomenclature and Notation, especially in Papers communicated to this Journal."[16] After the Geneva Congress, Henry Armstrong cited Watts' instructions as evidence of the longstanding soundness of British nomenclature practice, especially in contrast to the lawlessness of "our German colleagues – who have hitherto been, as a rule, almost uniformly neglectful in matters of nomenclature."[17]

Watts' successors found his nomenclature rules so favorable that they saw no reason to replace them. In 1893, the editor C. E. Groves elected to maintain Watts' scheme rather than adopting the rules of the Geneva Congress, to which Groves had not been invited. He did so even when the *Journal*'s practices explicitly diverged from the Geneva rules, such as the use of the terms *ethylene* and *acetylene* for the compounds with Geneva names *ethene* and *ethine*.[18] In 1900, the new editor W. P. Wynne did adopt a policy of taking the longest carbon chain in a hydrocarbon as the root of its name, a fundamental feature of the Geneva nomenclature.[19] But Wynne also added a new rule stipulating the use of Greek letters to specify carbon atoms in such chains, specifically contradicting the Congress' decision to number these carbons. Further,

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[15] Watts to Armstrong, 13 Sept 1881, quoted in James Traynham, "Organic Nomenclature: The Geneva Conference 1892 and the Following Fifty Years," in M. Volkan Kısakürek, ed., *Organic Chemistry: Its Language and Its State of the Art* (New York: VHCA, 1993): 3.

[16] Henry Watts, "Nomenclature and Notation," *Journal of the Chemical Society (Transactions)* 41 (1882): 247.

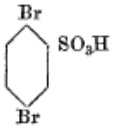
[17] Henry Armstrong, "The International Conference on Chemical Nomenclature," *Nature* 46, no. 1177 (May 19, 1892): 58. There was no direct connection between Watts' instructions and Friedel's reform effort. None of the Geneva delegates seem to have mentioned these guidelines, and Watts' successor was not invited to participate. Nonetheless, seeing an opportunity to stake a historical claim on behalf of Britain and to present a British genealogy that might make the Geneva rules more appealing to his countrymen, Armstrong described certain of Watts' principles as important precedents "legalized and extended by the Congress."

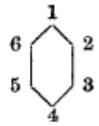
[18] "Instructions to Abstractors," *Journal of the Chemical Society (Abstracts)* 64 (1893): xxxi–xxxvii.

[19] W. P. Wynne, "Instructions to Abstractors," *Journal of the Chemical Society (Abstracts)* 78 (1900): iii–vii. The influence of Armstrong, a mentor and longtime collaborator, may have led Wynne to take a more favorable view of systematic nomenclature than his predecessors had. On Wynne and Armstrong's close personal and professional relationship, see E. H. Rodd, "Obituary Notice: William Palmer Wynne, 1861–1950," *Journal of the Chemical Society* (1951): 1936–1941.

Wynne strengthened the role of established names in determining future nomenclature, adding a rule referring to the most recent collective index of Chemical Society (1883-1892) as the default standard of reference for nomenclature questions.

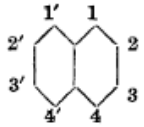
"13. In representing the constitution of benzene derivatives, as a rule, merely indicate the relative positions of the radicles in the symbol of benzene by figures, instead of by means of the hexagon symbol, as in the following example:

Paradibromobenzenesulphonic acid, $C_6H_3Br_2.SO_3H$ [Br : SO_3H : Br = 1 : 2 : 4] instead of 

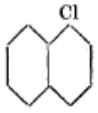
the figures always being used in the order 

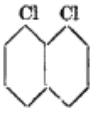
"14. In representing the constitution of derivatives of other 'closed chain' hydrocarbons also, as rule, do not employ graphic formulæ, but merely indicate the position of the radicles introduced in the following manner:—

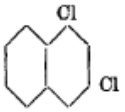
"*In the case of naphthalene*, express the position of the radicles introduced in place of hydrogen relatively to the carbon atoms common to the two 'rings,' and number the positions in the one ring 1, 2, 3, 4, and those in the other 1′, 2′, 3′, 4′ in the order shown by the annexed symbol:



"The dichloronaphthalenes, for example, are spoken of simply as 1 : 2 dichloronaphthalene, or dichloronaphthalene [Cl : Cl = 1 : 2], &c., thus:—

1 : 2 dichloronaphthalene = 

1 : 1′ dichloronaphthalene = 

1 : 3 dichloronaphthalene = 

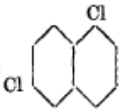
1 : 3′ dichloronaphthalene = 

Figure 1. Watts directed abstractors to use bracketed symbols and locant numbers instead of structural formula diagrams to express the constitution of benzene and other "closed-chain" compounds, to cut down on typesetting and printing costs.[20]

[20] Watts, "Instructions to Abstractors," 278–279. Note the missing "Cl" in the structural formula for 1:2 dichloronaphthalene – unintentional evidence of the value of systematic names, even when they were redundant.

With a few minor additions, these rules remained in place through the 1920s. There was little about the structure of the publication to encourage change. The editors continued to collect abstracts and publish articles, and once a decade, they assembled the accumulated annual indexes of the *Transactions*, *Abstracts*, and *Proceedings* into decennial volumes. A turn-of-the-century proposal to merge the abstracts journal with those of the Society for Chemical Industry, the American Chemical Society, or both came to nothing.[21] With no need to rename or reorganize previously compiled material, Chemical Society editors felt no need to consider minutiae of nomenclature.

An alternative to systematic nomenclature

Beginning in 1900, the Chemical Society's "Instructions to Abstractors" did refer to one outside resource. For naming derivatives of closed-chain compounds, abstractors were to use the numbering of ring atoms given in the 1899 second edition of the *Lexikon der Kohlenstoff-Verbindungen*, compiled by German chemist Max Moritz Richter.[22] Richter and Beilstein had much in common, as did the reference works that they created.[23] Both men began their compilations as a sideline to their day jobs, Beilstein as a professor in St. Petersburg, Richter as an industrial dye chemist and entrepreneur.[24] Both published first editions of these works during the early 1880s. Each compiled three editions of his work on his own, before passing on editorial responsibility for subsequent editions to the Deutsche Chemische Gesellschaft. Both

[21] Dabbin to Noyes, 11 Jul 1905, W.A. Noyes papers, Box 3, Folder General Correspondence, 1905-06; *Proceedings of the American Chemical Society* (1905): 62; Chemical Society, Minutes of Council, 18 Jan 1906, and inserted therein, Clarke to Ramsay, letter of 10 Jan 1906, RSC, AR0146.

[22] Wynne, "Instructions to Abstractors," v.

[23] On Beilstein and the early history of the *Handbuch*, see Chapter 1 of this dissertation and Michael D. Gordin, "Beilstein Unbound: The Pedagogical Unraveling of a Man and His Handbuch," in *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives,* ed. David Kaiser (Cambridge, MA: MIT Press, 2005), 11–39.

[24] Gordin, "Beilstein Unbound"; Max Moritz Richter, *Lexikon der Kohlenstoff-Verbindungen*, 2nd ed., vol. 1 (Hamburg and Leipzig: Leopold Voss, 1900), viii–xii.

Richter's *Lexikon* and Beilstein's *Handbuch* were highly praised and widely used at the turn of the twentieth century.

Beilstein and Richter shared another trait: both editors sought to *minimize* the role of nomenclature in the organization of their texts. In the course of compiling and organizing their works, these editors decided not to try to reshape nomenclature, but to circumvent it – each in a different way. As the Deutsche Chemische Gesellschaft assumed responsibility first for Beilstein's text, then for Richter's, its chief editor Paul Jacobson worked to reshape the organization of these works as a stable framework within which they could grow in perpetuity. With systems of nomenclature and notation as his thread, Jacobson knit together these and other publications of disparate origin into a sweeping and highly coordinated reference enterprise that chemists around the world came to rely upon for information about organic chemical substances.

Beilstein and Richter were not the first chemists to construct comprehensive reference works, but the works that preceded theirs comprised a different genre. In the middle decades of the nineteenth century, a number of chemists embarked upon sweeping, encyclopedic chemical reference projects. These works involved well-respected editors – the pioneering researchers and educators Liebig and Wöhler in Germany, Watts in England, the atomist Wurtz and the anti-atomist Frémy at the helms of competing projects in France – and staffs of younger chemists writing long, discursive articles on chemical topics.[25] In most of these works, articles

[25] The first editions of these works were Justus Liebig, Johann Christian Poggendorff, and Friedrich Wöhler, eds., *Handwörterbuch der reinen und angewandten Chemie*, 10 vols. (Braunschweig: F. Vieweg und Sohn, 1842–1864); Henry Watts, *A Dictionary of Chemistry and the Allied Branches of Other Sciences*, 5 vols. (London: Longmans, Green, 1863–1868); Adolphe Wurtz, ed., *Dictionnaire de chimie pure et appliquée*, 3 vols. (Paris: L. Hachette, 1869–1878); Edmond Frémy, ed., *Encyclopédie chimique*, 10 vols. (Paris: Dunod, 1882–1899). On the competing French reference works, see Danielle Fauque, "La réception de la théorie atomique en France sous le Second Empire et au début de la IIIe République," *Archives internationales d'histoire des sciences* 53 (2003): 64–112.

were commissioned and published in alphabetical order.[26] Volumes appeared one by one, from A to Z; in most cases at least a decade passed between the publication of the first and final volumes. Once the series was complete, the entries at the beginning of the alphabet were out of date. The editor (or, more often, a successor) would then begin the process all over again with supplementary volumes or a new edition.

Nomenclature entered into these works as subject matter. The "Nomenclature" article in Watts' *Dictionary of Chemistry* outlined various conventions of rational nomenclature and commented, as was customary, on the general disorder of the field.[27] Shortly after the Geneva Congress, Friedel used the second supplement to Wurtz's *Dictionnaire* that he was editing as a means of disseminating the Geneva rules and his Subcommission's proposals for extending them. In the "C" volume – a convenient letter – he inserted a twenty-page article by Combes entitled "Chimique (nomenclature)" and an eleven-page entry by Bouveault, "Chaînes Fermées (nomenclature des)."[28] These works might describe nomenclature, but with articles addressing chemical concepts or classes rather than individual substances, nomenclature was not integral to their structure.

A nearer model for the work of Beilstein and Richter was Leopold Gmelin's *Handbuch der Anorganischen Chemie*, published in German beginning in 1817 and supplemented and translated into English by Watts from 1848-1872. Gmelin's work was a collection of data and

[26] Frémy's encyclopedia was an exception; it was arranged analytically according to a schema intended to be pedagogically meaningful. This organization may have made the work more difficult to compile. Frémy's volumes were published sporadically, in small sections, and out of order.

[27] George C. Foster, "Nomenclature," in Watts, *A Dictionary of Chemistry*, vol. 4, 118–34.

[28] Alphonse Combes, "Chimique (nomenclature)," in *Dictionnaire de chimie pure et appliqué*, 2nd supp., ed. Charles Friedel and Camille Chabrié, vol. C (Paris: Librairie Hachette, 1894), 1060–1076; Louis Bouveault, "Chaines fermées (nomenclature des)," in Friedel and Chabrié, *Dictionnaire de chimie*, 2nd supp., vol. C, 1043–1053.

literature references addressing chemical elements, simple compounds, reactions, and laboratory apparatus, edited and republished in an updated edition every decade or so.[29]

In his own *Handbuch*, whose first edition was published between 1880 and 1882, Beilstein sought to bring Gmelin's approach to organic chemistry. The number of organic compounds recorded in the literature through 1880 made compiling information on their preparation, characteristic reactions, and physical properties a daunting task. But the process of dealing with all of that information – and in so doing assembling an exhaustive list of literature references for each compound – raised another significant question: in what order should he place all of those compounds? In keeping with his original plan of writing a textbook, Beilstein chose to arrange the 15,000 compounds addressed in the *Handbuch* by chemical function, in order of increasing complexity of structural features associated with these functions.[30] However, this ordering presented some challenges: decisions about the relative complexity of different structural features were often arbitrary, and the sheer number of combinations of chemical functions that he had to address stretched his table of contents to twenty-three pages and 275 separate headings. Beilstein also included an alphabetical subject index, but there he faced the same challenge as an author that he'd confronted as a reader: it was difficult to know which name to use when many compounds were indexed in other works under multiple different names.[31] A reader could learn a lot about the relationships among various classes of compounds by flipping between the subject index, table of contents, and

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[29] Helen Schofield, "The Evolution of the Secondary Literature in Chemistry," in *Proceedings of the 1998 Conference on the History and Heritage of Science Information Systems*, ed. Mary Ellen Bowden, Trudi Bellardo Hahn, and Robert Virgil Williams, (Medford, NJ: Information Today, 1999), 97–98; Crane and Patterson, *A Guide to the Literature of Chemistry*, 161–167.

[30] Gordin, "Beilstein Unbound," 11, 24. While Gordin depicts Beilstein's organization of the *Handbuch* as a point of divergence from his original pedagogical intentions, its ordering is actually quite similar to that of the textbooks that Beilstein took as his points of reference, especially August Kekulé, *Lehrbuch der organischen Chemie* (Erlangen: F. Enke, 1861).

[31] Friedrich Beilstein, *Handbuch der Organischen Chemie*, 1st ed., vol. 1 (Hamburg and Leipzig: Leopold Voss, 1881), vii.

main text in search of the entry for a particular compound. But that reader often would have happily sacrificed such insights in exchange for direct access to the desired data and citations.

Like Beilstein, Richter sought to address the increasingly time-consuming and unreliable process of searching journal indexes for information on an organic compound. He acknowledged in the introduction to his 1884 *Tabellen der Kohlenstoff-Verbindungen* that Beilstein had beaten him to the mark, and offered his work "as a supplement" to Beilstein, in whose work the benefits of Richter's own "are already mostly united."[32] Richter's aims were more modest than those of his fellow editor; in place of Beilstein's detailed physical and chemical properties and exhaustive literature citations, Richter offered a concise entry for each compound: its name, melting point and boiling point (if documented), any common salts, and a few literature references that Richter considered most significant.

Richter's work was not merely a watered-down version of Beilstein's, however. As a single volume of about 500 pages, it was easier to handle than Beilstein's two-volume, 2,200-page reference. Most of its entries occupied only one or two lines, and its consistent layout made the *Tabellen* far easier to browse than Beilstein's *Handbook*, whose densely-packed, synoptic text often ran more than a page for a single compound (*Figs. 2-3*).

The most user-friendly feature of Richter's work, however, was its mode of organization. Like Beilstein, Richter included an alphabetical subject index at the end of the work, but for its main body, he chose neither the alphabetical ordering of an index nor the sort of intricate chemical classification that Beilstein had designed. Rather, he arranged his index according to a seemingly obsolete lexical tool: the empirical formula, a concise list of the number of atoms of each element contained in one molecule of the compound. (See *Fig. 3* caption for an explanation of this system.)

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[32] Max Moritz Richter, *Tabellen der Kohlenstoff-Verbindungen* (Berlin: R. Oppenheim, 1884), v. For its second and third editions, Richter retitled the work *Lexikon der Kohlenstoff-Verbindungen*.

390 FETTREIHE. — C. MEHRATOMIGE VERBINDUNGEN.

6. Hexylenglykole $C_6H_{14}O_2$.

1. Aus **Normal-Hexylenbromid.** $CH_3.CH(OH).CH(OH).CH_2.CH_2.CH_3$ (?) (WÜRTZ. *A. ch.* [4] 3, 180). — *Darstellung.* Man kocht am Rückflusskühler ein Gemenge von 20 Thln. $C_6H_{12}.Br_2$ (aus Mannit), 11,5 Thln. K_2CO_3 und 140 Thln. H_2O, unter Zusatz einiger Cubikcentimeter verdünnter Kalilösung. Man sättigt mit CO_2, fügt genügend K_2CO_3 hinzu und schüttelt mit Aether aus (HECHT, MUNIER, *B.* 11, 1154). — Siedep.: 207°; spec. Gew. = 0,9669 bei 0°. Mit Wasser mischbar. Giebt mit HJ secundäres Hexyljodid. — Wird vom Chromsäuregemisch zu Essigsäure, CO_2 und Buttersäure oxydirt (HECHT, MUNIER).

Diacetat $C_6H_{12}(C_2H_3O_2)_2$. Siedep.: 215—220°; spec. Gew. = 1,014 bei 0°.

2. **Diallyldihydrat** $(C_3H_5)_2.(H_2O)_2 = CH_3.CH(OH).CH_2.CH_2.CH(OH).CH_3$. *Bildung.* Diallyldihydrojodid $(C_3H_5)_2.(HJ)_2$ (S. 331) reagirt auf Silberacetat unter Bildung von freiem Diallyl, Diallylacetat und Diallyldiacetat. Letzteres wird mit der theoretischen Menge festen Kalis verseift (WÜRTZ, *A. ch.* [4] 3, 162). — Siedep.: 212—215°; spec. Gew. = 0,9638 bei 0° (W.). SOROKIN beobachtete (*Ж.* 10, 399) den Siedepunkt 219—220° (i. D.) bei 745,2 mm; spec. Gew. = 0,9759 bei 0°; = 0,9605 bei 24° (gegen Wasser von 0°). Löslich in Wasser. Verbindet sich mit conc. HCl oder HJ zu $(C_3H_5)_2.(HCl)_2$ oder $(C_3H_5)_2.(HJ)_2$. Bei der Oxydation mit Chromsäuregemisch treten CO_2 und Essigsäure auf (SOROKIN).

Monoacetat $(C_3H_5)_2(H_2O).(C_2H_4O_2)$. *Bildung.* Entsteht, neben dem Diacetat, aus $(C_3H_5)_2.(HJ)_2$ und $AgC_2H_3O_2$ (WÜRTZ). — Siedep.: 210°.

Diacetat $C_6H_{12}(C_2H_3O_2)_2$. Siedep.: 225—230°; spec. Gew. = 1,009 bei 0°.

3. **Pinakon** $(CH_3)_2.C(OH).C(OH)(CH_3)_2$. *Bildung.* Bei der Einwirkung von Natrium auf Aceton (Fittig, *A.* 110, 25; 114, 54; STÄDELER, *A.* 111, 277) oder von Natriumamalgam auf wässriges Aceton (FRIEDEL, *A.* 124, 329). Aus Tetramethyläthylenbromid und Silberacetat und Zerlegen des gebildeten Pinakonacetates mit Baryt (PAWLOW, *Ж.* 10, 290 = *A.* 196, 126). — *Darstellung.* Eine wässrige Lösung von K_2CO_3 von solcher Stärke, dass sie Aceton nicht merklich löst, aber von Natrium noch leicht zersetzt wird, wird mit (600 bis 700 Thln.) Aceton übergossen und Natrium (200—250 Thle.) in erbsengroßen Stücken eingetragen. Man fraktionirt die obere Schicht und erhält Isopropylalkohol und (60—75 Thle.) Pinakon (FRIEDEL, SILVA, *J.* 1873, 340). — Kleine Nadeln. Schmelzp.: 35—38°; Siedep.: 171—172° bei 739 mm. Leicht löslich in kaltem Alkohol, wenig in kaltem CS_2 (LINNEMANN, *A. Spl.* 3, 374). Schwer löslich in kaltem Wasser, leicht in heißem; beim Erkalten scheidet sich das Hydrat ab. Zerfällt beim Kochen mit verd. H_2SO_4 in Pinakolin $C_6H_{12}O$ und Wasser. Ebenso bei der Einwirkung von Chlor. — Wird vom Chromsäuregemisch zu Aceton oxydirt. — Jodwasserstoffgas über Pinakon geleitet, erzeugt Isopropyljodid und wenig Hexan C_6H_{14} (LINNEMANN, *J.* 1871, 422). Beim Erhitzen von Pinakon mit concentrirter Jodwasserstoffsäure auf 160° entstehen Pinakolin und ein Jodür $C_6H_{11}J$. Steigert man die Temperatur auf 270°, so wird Hexan gebildet (BOUCHARDAT, *Z.* 1871, 699). — $POCl_3$ liefert das krystallisirte Chlorid $C_6H_{12}Cl_2$ (Schmelzp.: 160°), während PCl_5 die Chloride $C_6H_{11}Cl$ und $C_6H_{10}Cl_2$ erzeugt (FRIEDEL, SILVA, *B.* 6, 35). — Pinakon durch ein schwach glühendes Rohr geleitet, zerfällt in Aceton und Isopropylalkohol. $C_6H_{14}O_2 = C_3H_6O + C_3H_8O$ (THÖRNER, ZINCKE, *B.* 13, 645). — Es gelingt nicht zusammengesetzte Aether des Pinakons darzustellen. Chloracetyl erzeugt einen chlorhaltigen Körper. Mit Essigsäureanhydrid scheinen bei 200° Kohlenwasserstoffe zu entstehen (TH., Z.).

Pinakonhydrat $C_6H_{14}O_2.6H_2O$. Pinakon vereinigt sich leicht mit Wasser. — Vierseitige Tafeln. Schmelzp.: 46,5° (LINNEMANN). Wenig löslich in kaltem Wasser, leicht in Alkohol. Sublimirt schon bei gewöhnlicher Temperatur. Verflüchtigt sich unzersetzt mit Wasserdämpfen. Zerfällt bei der Destillation in Wasser und Pinakon. Verliert das Krystallwasser über Schwefelsäure.

7. Heptylenglykol $C_7H_{16}O_2$.

Ein **Chlorhydrin** $C_7H_{15}ClO$ entsteht aus Octylen und einer 2—3procentigen Lösung von unterchloriger Säure (CLERMONT, *Z.* 1870, 411). — Siedep.: 206—208°; spec. Gew. = 1,014 bei 0°; = 1,001 bei 14°.

8. Octylenglykole $C_8H_{18}O_2$.

1. Aus **Octylenbromid** (mit Octylen aus Fuselöl) (WÜRTZ, *A.* 128, 231). — Das Diacetat $C_8H_{16}(C_2H_3O_2)_2$ siedet bei 240—245°.

2. Aus **Octylenbromid** (mit Octylen aus Methylhexylcarbinol?) (CLERMONT, *A. Spl.* 3, 254). — Siedep.: 235—240°; spec. Gew. = 0,932 bei 0°; = 0,920 bei 29°.

Figure 2. A page of Beilstein's *Handbuch,* first edition.

$C_6H_{13}Cl$ 13) isom. Hexylchlorid. Sd. 118° und Sd. 124° (*B.* 6, 36; 7, 953).
14) isom. Hexylchlorid (sec.?). Sd. 123,5° (*M.* 2, 313).

$C_6H_{13}Br$ 1) (norm.) Hexylbromid. Sd. 155,5° (*A.* 187, 137).
2) (norm.) Methylpropylätholbromid. Sd. 142—145° (*M.* 4, 34).
3) (sec.) Hexylbromid. Sd. 143—145° (*A.* 188, 251).

$C_6H_{13}J$ 1) (norm.) Hexyljodid. Sd. 179,5° (*A.* 163, 196; 187, 138; *J.* 1863, 526).
2) (sec.) Hexyljodid. Sd. 167° (*J.* 1861, 731; 1862, 480; 1870, 449; *B.* 9, 1086; 11, 1421; *A.* 132, 307; 135, 130; 165, 147; 196, 351).
3) (sec.) Methylpseudobutylcarbinoljodid. Sd. 140—144° (*J.* 1873, 339).
4) (sec.) Aethylpropylcarbinoljodid. Sd. 164—166° (*Bl.* 25, 9.)
5) (tert.) Dimethylpropylcarbinoljodid. Sd. 142° (*A.* 195, 254); Sd. 139—140° (*A.* 209, 84).
6) (tert.) Dimethylisopropylcarbinoljodid (*A.* 196, 125; *J. r.* 10, 288).
7) isom. Hexyljodid. Sd. 154—160° (unc.) (*M.* 4, 44).
8) isom. Hexyljodid. Sd. 100° bei 70 mm (*A.* 178, 18)
9) isom. Hexyljodid. Sd. 150° (aus Fuselöl) (*A.* 128, 228).

$C_6H_{14}O$ 1) (prim.) norm. Hexylalkohol. Sd. 157,2° (cor). Na (*A.* 88, 325; 133, 180; 161, 272; 163, 193; 185, 43; 187, 126; *B.* 16, 743).
2) (prim.) Pentylcarbinol. Sd. 152—153° (*A.* 195, 102; *B.* 6, 147).
3) (prim.) Methylpropyläthol. Sd. 146,6° (146,9°) (*M.* 4, 32, 40).
4) (sec.) Methylbutylbarbinol. Sd. 136° (*A.* 135, 139; 161, 272; 165, 151; 177, 307; 178, 22; *J.* 1863, 519; *M.* 2, 320).
5) (sec.) Aethylpropylcarbinol. Sd. 135° (cor.) (*Bl.* 25,7; *B.* 8, 1019).
6) (sec.) Pinakolinalkohol (Methylpseudobutylcarbinol). Sm. 4° (*J.* 1873, 339).
7) (tert.) Methyldiäthylcarbinol. Sd. 121—122,5° (*Z.* 1865, 615; *J. pr.* [2] 26, 111).
8) (tert.) Dimethylpropylcarcinol. Sd. 122,5—123,5° (117—118°) (*Z.* 1865, 617; *A.* 195, 254; 209, 84; *J, r.* 10, 250; *J. pr.* [2] 26, 111).
9) (tert.) Dimethylisopropylcarbinol. Sd. 117° (*Z.* 1871, 275; *A.* 196, 123; *J. r.* 10, 286; 13, 560; *B.* 14, 2065, 2066; *J. r.* 1882, 99; *J. pr.* [2] 26, 111).
10) Propyläther. Sd. 85—86° (*A.* 151, 304; 161, 37; 214, 163).
11) Isopropyläther. Sd. 60—62° (*A.* 126, 306; 214, 164).
12) norm. Aethylbutyläther. Sd. 91,7° (*A.* 158, 167).
13) Aethyldiisobutyläther. Sd. 78—80° (*A.* 93, 118).
14) Methylisoamyläther. Sd. 92 (*A.* 81, 80).
15) Trimethylcarbinoläthyläther. Sd. 68—69° (*C. r.* 93, 69).

$C_6H_{14}O_2$ 1) norm. Hexylenglykol. Sd. 207° (*B.* 11, 1154; *A. ch.* [4] 3, 180; *B.* 16, 398).
2) Pinakon (Tetramethyläthylenglykol). Sm. 35—38°; Sd. 171—172° (*A.* 110, 25; 111, 278; 114, 54; 124, 329; 196, 126; *J. r.* 10, 290; *J.* 1871, 422; 1873, 340; *A. Spl.* 3, 374; *Z.* 1871, 699; *B.* 6, 35; 13, 645; 16, 399).
3) Pinakonhydrat = ($C_6H_{14}O_2$, $6H_2O$). Sm. 46,5° (*A. Spl.* 3, 377; *A.* 196, 127).
4) Diallyldihydrat. Sd. 212—215° (219—220°). 2HCl, 2HJ (*A. ch.* [4] 3, 162; *J. r.* 10, 399; *J. pr.* [2] 23, 18).
5) Aethylidendiäthyläther (Acetal). Sd. 104° (*A. ch.* [3] 56, 139; *A.* 64, 322; 100, 116; 126, 62; 203, 25; *B.* 16, 512).
6) Aethylenglykoldiäthyläther. Sd. 123,5° (*A. ch.* [3] 55, 431).

$C_6H_{14}O_3$ 1) Glykolacetal. Sd. 167° (*B.* 5, 150).
2) Trimethylglycerinäther. Sd. 148° (*J.* 1864, 495).
3) prim. sec. Hexylglycerin. Sd. 181° bei 10 mm (*J. r.* 1881, 353).
4) Methyläthylglycerin. Sd. 170—176° bei 53 mm (*M.* 4, 41).
5) Verbindung (Alkohol) (*B.* 13, 1843).

$C_6H_{14}O_4$ Triäthylenglykol. Sd. 290° (*A. ch.* [3] 67, 279; 69, 333).

$C_6H_{14}O_5$ Diglycerin. Sd. 220—230° bei 10 mm (*A. ch.* [3] 67, 300).

$C_6H_{14}O_6$ 1) Mannit. $4 + 3CaO$ (*A. ch.* [3] 46, 173; *J.* 1856, 635; *B.* 14, 1760; 15, 797). $2 + SrO$, $2 + BaO$, Pb_2 (*Ber. J.* 25, 557) (*A.* 131, 50; *A. ch.* [3] 57, 213).
2) Dulcit. Sm. 188,5° (*A.* 24, 241; 76, 358; 80, 345; 117, 143; 123, 372; *B.* 12, 1274; 13, 2306; *J.* 1862, 480; *A. ch.* [4] 27, 68); $BaO + 7H_2O$, 3PbO, 3CuO.

Figure 3. A page of Richter's *Tabellen*, first edition.

Figure 3, ct'd. Richter first arranged empirical formulas of organic compounds by the number of carbons atoms that they contained. He then arranged each of these sections by the number of other elements in the compound. Within each of these subsections, he arranged the entries by increasing number of atoms of each of these other elements, beginning with hydrogen, then oxygen, and so on in a set order roughly corresponding to frequency of occurrence. All isomers sharing that formula were then listed by name.

For example, in the page shown above is from the C_6 section (six carbon atoms) and the subsection of compounds containing two other elements. We see the transition from H_{13} to H_{14} (hydrogen always comes first, after carbon). Other elements are listed in the order O-N-Cl-Br-J (that is, I)-S-P-[other, in alphabetical order]; the three-element C_6H_{13} formula list concludes with entries containing Cl, Br, and J, and the C_6H_{14} list begins with O.[33]

The choice went very much against the tide. As we saw in Chapter 1, organic chemists of the late nineteenth century were interested in the determining the constitution of compounds and in designing new ones to synthesize, often with the help of structural formulas. Yet Richter compiled his reference work on the basis of empirical formulas, which indicated only the composition of organic molecules, entirely ignoring the pattern of connections that was the basis of their behavior and relationships.

In an 1896 article entitled "A Contribution to Nomenclature," Richter conducted the readers of the *Berichte* through a line of reasoning that culminated in the adoption of such an approach. The bulk of this article detailed a proposed method (yet another one!) for generating systematic names of ring compounds. Richter's proposal could most charitably be described as peculiar (*Fig. 4*). He advocated eliminating all familiar trivial names, even for a compound as ubiquitous as benzene, in favor of basing the names of ring compounds on the same roots used for open-chain hydrocarbons. He proposed introducing two entirely new kinds of symbols seemingly plucked from a printer's worst nightmares: circled numbers indicating the size and

[33] Ibid., 1–5. Richter printed an explanation of his system in German, French, English, and Italian at the beginning of the third edition of this work; Max Moritz Richter, *Lexikon der Kohlenstoff-Verbindungen*, 3rd ed. (Hamburg and Leipzig: Leopold Voss, 1910), 1–13.

relative position of the rings in a compound, and a tiny circle hovering directly above a locant number – which was sometimes already in superscript – to indicate that the number referred to a position within a ring. Furthermore, he included only two tangential citations to other nomenclature proposals over the course of the nineteen pages in which he laid out his own, barely engaging with the alternative nomenclature practices that he sought to supersede.[34]

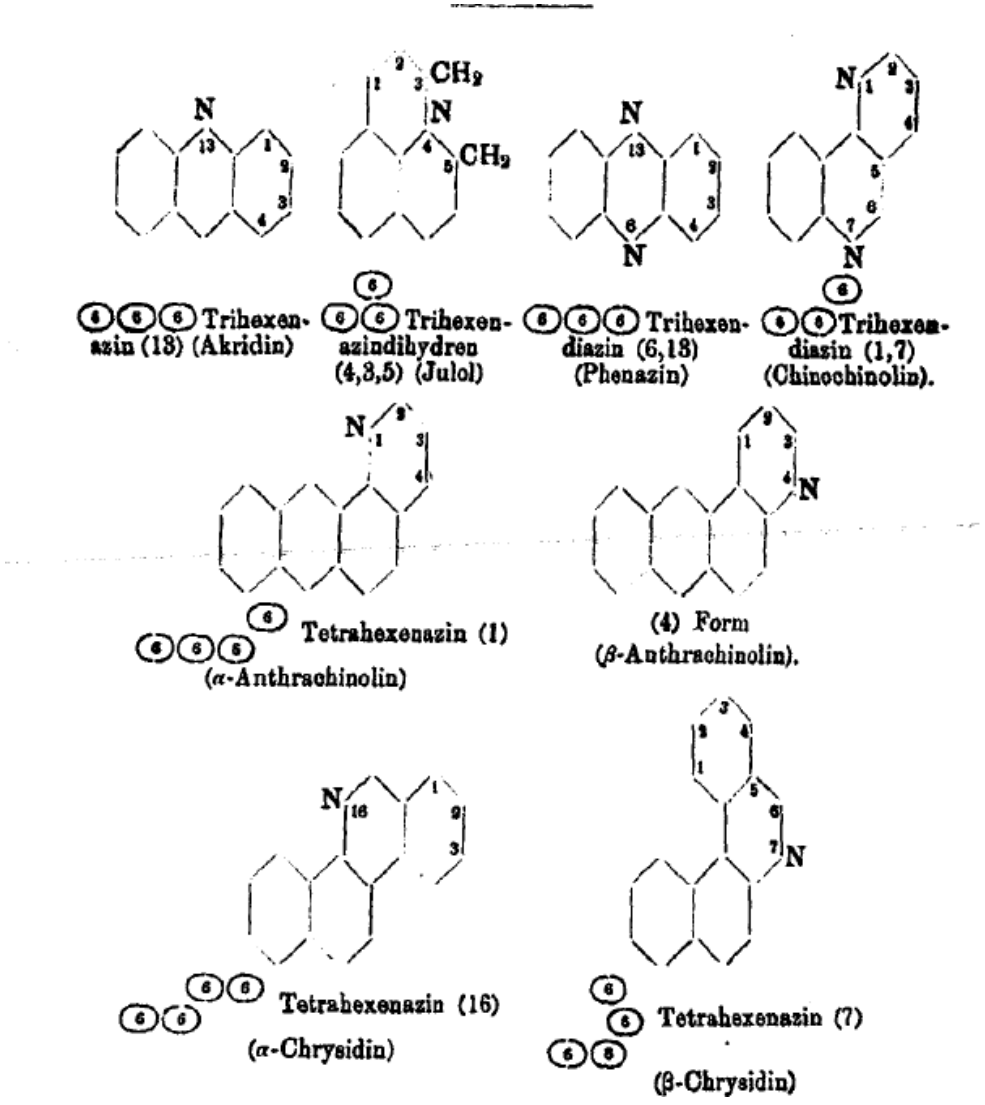

Figure 4. Systematic names that Richter provided as examples of the application of his proposals.[35]

[34] Max Moritz Richter, "Ein Beitrag zur Nomenclatur," *Berichte der Deutschen Chemischen Gesellschaft* 29, no. 1 (1896): 586–608.

[35] Ibid., 600.

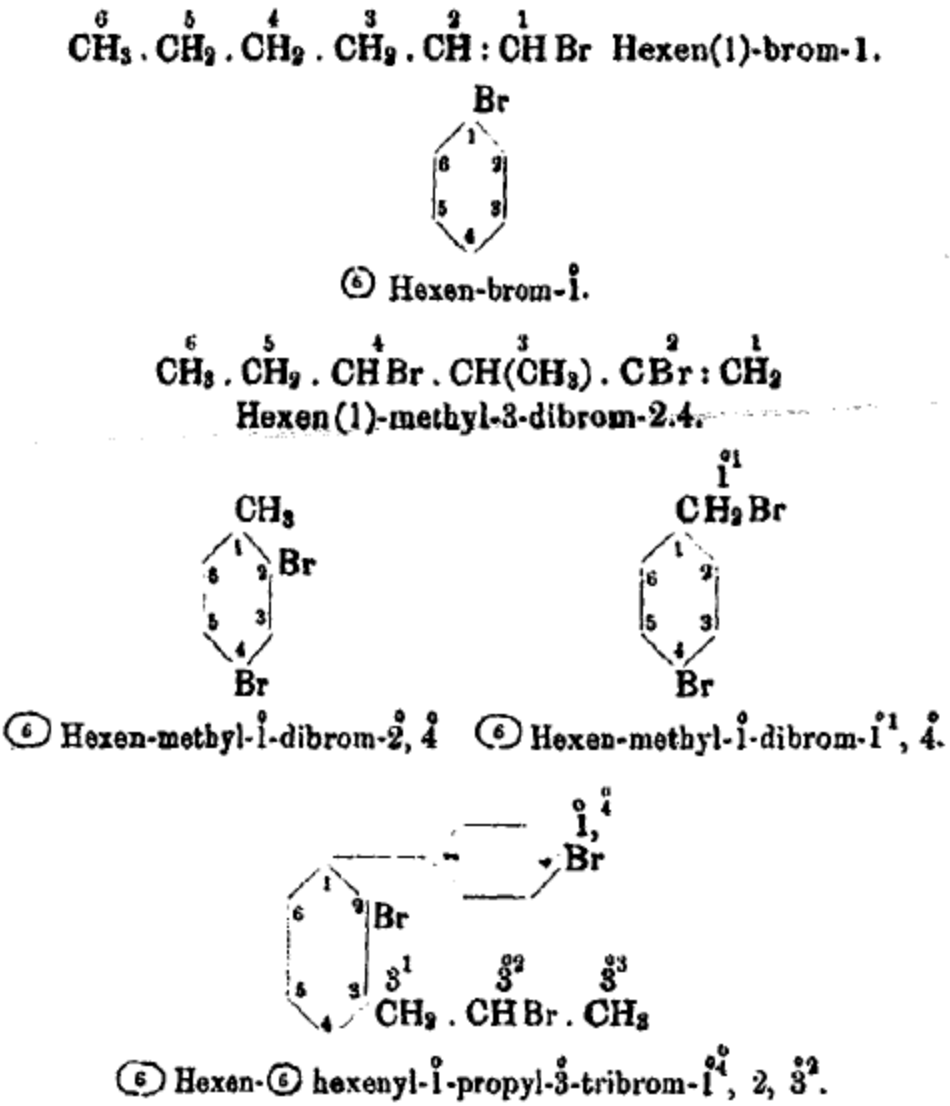

Figure 4 (ct'd). Systematic names that Richter provided as examples of the application of his proposals.[36]

However, the introduction and conclusion of Richter's article suggested that he had not published these proposals merely to advance the cause of nomenclature reform. He opened by observing that the oft-noted confusion in organic chemical nomenclature was particularly troublesome for those "whose work is primarily literary, so that in the preparation of references or in work on large collected volumes they are at a loss and have to resort to

[36] Ibid., 602.

arbitrariness." As one of these literary workers, and at the encouragement of Beilstein and other colleagues, Richter had consented to presenting his own nomenclature proposal. He had sent it to the *Berichte* rather than to members of the (not yet defunct) International Commission because he felt that journal publications were a better foundation for "a strong and stable edifice of nomenclature [*Nomenclaturgebäude*]" than relatively brief international meetings for which participants might or might not have adequately prepared.[37]

After laying out his proposal, Richter juxtaposed the respective names that his procedure and *Berichte* editor Ferdinand Tiemann's application of the Geneva rules generated for a complex ring compound (*Fig. 5*).

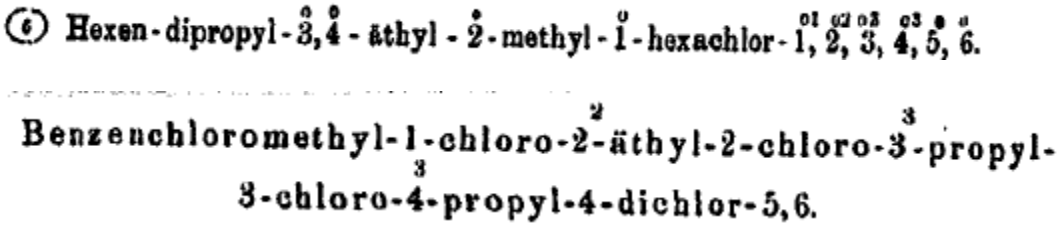

Figure 5. Above: Richter's name. Below: Geneva name (per Richter), according to Tiemann's German translation of the rules.[38]

Richter claimed, rather dubiously, that the superiority of his method was "clear as day."[39] His next point, however, was more compelling: "I had wished to be able to give my honored readers more euphonious and shorter names in my reform proposals; yet these unfortunately cannot be attained if I place rigor and unambiguousness as the highest aim, and I must console myself with the fact that nature as well as culture leaves us to reckon with the imperfect."[40] In

[37] Ibid., 586–88.

[38] Ibid., 603. For Tiemann's German-language adaptation of the Geneva rules, see Ferdinand Tiemann, "Ueber die Beschlüsse des internationalen, in Genf vom 19. bis 22. April 1892 versammelten Congresses sur Regelung der chemischen Nomenclatur," *Berichte der Deutschen Chemischen Gesellschaft* 26 (1893): 1595–1631.

[39] Richter, "Ein Beitrag zur Nomenclatur," 603.

[40] Ibid., 606.

Richter's judgment, *neither* sort of name was a suitable basis for ordering a chemical reference work. What was a literary worker to do?

His answer, he concluded emphatically, was the *empirical formula*: a humble device, but a valuable and unambiguous one. His formula index at one stroke narrowed down the number of names through which the user had to search from the 60,000 organic compounds then known to the isomers that shared any given empirical formula – twenty or so, at most. A much less rigorous approach to nomenclature would suffice to distinguish the members of such a short list.[41]

Richter's curious article reveals three important points about how editors of reference works thought about systematic nomenclature in the wake of the Geneva Congress. First, just as important as *how* Richter thought that one should build a nomenclature and organize a reference work was *who* he believed was qualified for such a task. "I have not come to my conclusions and my proposals by way of deduction or speculation," he wrote, with a subtle dig at chemists who moonlighted in lexicography. Rather, he had arrived at his conclusions through "long years of work, which I have undertaken in the interest of facilitating our high science."[42] If the Geneva rules were truly a nomenclature for indexes, then they shouldn't have been created *for* editors *before* they set to work, but *by* editors *in the process of* compiling their reference texts.

Second, whether Richter's nomenclature of ring compounds amounted to a quixotic but earnest proposal or an implausibly elaborate straw man, he did call attention to an important internal contradiction in the Geneva nomenclature and the aims of its proponents. Although the Congress had subordinated the length and euphony of names to their uniqueness and

[41] Specifically, Richter proposed an informal hybrid procedure based on the principle of substitution and incorporating elements of the Geneva nomenclature, Widman's nomenclature of heterocyclics, and trivial names of other ring compounds. Richter, *Lexikon der Kohlenstoff-Verbindungen*, 2nd ed., vol. 1, 10–14.

[42] Richter, "Ein Beitrag zur Nomenclatur," 608.

correspondence to chemical structure for the purpose of register-making, such a decision was more easily pronounced than enforced. In order to function as names at all, Richter's critique suggested, chemical names would have to be shorter and more elegant than any systematic names were likely to be. If the price of assembling a system of nomenclature suitable for indexes was a collection of names that were generally illegible, then perhaps another approach to index-making was in order.

Finally, while Congress delegates weighed the pros and cons of rules of nomenclature in themselves, editors were centrally concerned with the time and labor involved in the shift from their existing practices to a new set of names or organizational scheme. After completing the third edition of the *Handbuch* in 1899, Beilstein wrote to a colleague that the system by which he organized the first edition "became useless a long time ago," but that "to rewrite all of the material...would have taken a year, and I never would have been able to finish the job."[43] Richter thought more highly of his empirical formula scheme, but he would have found it similarly difficult to replace. He wrote this article while immersed in compiling a new edition of a work arranged according to a method of ordering compounds that he'd adopted a decade before the Geneva Congress, and as it was, the resolutions of the Geneva nomenclature had "necessitated extensive alternations" in his preliminary revisions and significantly delayed the publication of his second edition.[44] No wonder he found the prospect of switching away from a formula index to an alphabetical index of systematic names uncongenial.

The reference enterprise of the Deutsche Chemische Gesellschaft

Richter published his article during a period of change at the Deutsche Chemische Gesellschaft (DChG). The celebration of the twenty-fifth anniversary of Kekulé's proposal of the

[43] Beilstein to Paul Jacobson, n.d., cited in Paul Jacobson, "Beilsteins Handbuch der Organischen Chemie, ein Spiegel ihrer Entwicklung," *Naturwissenschaften* 7, no. 14 (April 1, 1919): 224.

[44] Richter, *Lexikon der Kohlenstoff-Verbindungen*, 2nd ed., vol. 1, viii.

hexagonal structure of benzene in 1891 and the death of the Society founder A.W. Hofmann a couple of weeks after the Geneva Congress in 1892 marked a turning point in the history of the institution.[45] Adolf von Baeyer, Emil Fischer, and other leaders of the Society set their sights on consolidation of the chemical literature as a means of carving out an expanded role for the Society in rationalizing and promoting German chemistry. Over the course of 1895 and 1896, Fischer and Baeyer arranged for the Society to assume responsibility for the abstract journal *Chemisches Zentralblatt* and for Beilstein's *Handbuch* as well as the *Berichte*. This would unify under the editorial direction of the Society the world's most influential chemical journal, abstract journal, and reference work.[46]

To manage this process of consolidation, Beilstein recommended that Baeyer and Fischer call upon Paul Jacobson.[47] At the time, Jacobson was employed as extraordinary professor of organic chemistry at Heidelberg, where his mentor, Victor Meyer, held the chair.[48] Jacobson had trained with Robert Bunsen in Heidelberg and Carl Liebermann in Berlin, completing his PhD in 1882.[49] After a short term in a Berlin industrial firm, he took a position as assistant to Meyer in Göttingen. In 1889, he followed Meyer to Heidelberg, where Meyer succeeded Bunsen. When Baeyer and Fischer offered him a full-time position in Berlin as General Secretary and Editor-in-Chief for the DChG in mid-1896, Jacobson was granted a

[45] On the "Benzolfest," see Alan J. Rocke, *Image and Reality: Kekulé, Kopp, and the Scientific Imagination* (Chicago: University of Chicago Press, 2010), 293–323.

[46] The *Annalen der Chemie* remained an important outlet, but the *Annalen* tended to publish long articles summarizing significant research rather than short articles publicizing (and establishing priority for) novel findings. On the increasing significance of priority of publication and the concomitant emergence of such short articles as a prominent form of scientific writing, see Csiszar, *The Scientific Journal*.

[47] Beilstein took pride in having brought Jacobson into his position in the DChG. Beilstein to Fischer, 2/15 Oct 1906, Emil Fischer papers, Box 9.

[48] It had been at Meyer's urging that Richter undertook the revision of his *Tabellen* in 1889. Richter, *Lexikon der Kohlenstoff-Verbindungen*, 2nd ed., vol. 1, viii.

[49] "Paul Jacobson," in Dagmar Drüll, *Heidelberger Gelehrtenlexikon 1803-1932*. 2nd ed., in preparation. My thanks to Dr. Drüll-Zimmermann for sharing her work in progress.

temporary leave of absence to test the waters of the new job.[50] A year later, with little prospect for advancement at Heidelberg with the middle-aged Meyer firmly ensconced in the university's chair in chemistry, Jacobson elected to leave the faculty for good in May of 1897.[51]

Jacobson positioned indexes as the centerpiece of the "plan for the centralization of chemical literature" that he had been hired to carry out.[52] He and his assistant Robert Stelzner turned first to the index of the *Berichte*, over which they exercised direct control. In 1898, his first full year of sole editorship, Jacobson marshalled the support of the DChG Council and its representatives to the near-dormant International Nomenclature Commission – a group including Geneva delegates Fischer, Tiemann, and Baeyer, as well as Beilstein, the most noteworthy user of Geneva names – to *eliminate* the use of systematic organic nomenclature as a means of organizing the index to the *Berichte*. In place of the subject index, Jacobson and Stelzner installed a formula index modeled on Richter's.[53] Tiemann had introduced the Geneva nomenclature to German readers and put it to use in the *Berichte* for years. Beilstein had put his authority behind Geneva names by including them in the third edition of his work. Baeyer had been the one who insisted that the Geneva nomenclature had to be tailored for use in indexes in the first place. But Jacobson convinced them all to disavow the very premise of a systematic nomenclature of indexes.[54]

In a preface to their inaugural annual formula index to the *Berichte*, Jacobson and Stelzner justified their decision. They argued, first, that the Geneva system hadn't worked. Six

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[50] Jacobson to Faculty, 24 Jun 1896, Universitätsarchiv Heidelberg, Heidelberg, Germany, PA 1765.

[51] L. Kriegsberger to the Academic Senate, 14 Mai 1897, Universitätsarchiv Heidelberg, PA 1765. Had Jacobson delayed the decision two more months, his career might have turned out very differently; Meyer took his own life over the summer. B. Horowitz, "Victor Meyer - His Life and Work," *Journal of the Franklin Institute* 182 (1916): 375.

[52] Jacobson to Fischer, 27 May 1895, Emil Fischer papers, Box 21, Folder "Jacobson, Paul Heinrich."

[53] The formula index was for organic compounds of determinate composition only. Inorganic compounds and compounds of unknown composition were listed in a separate alphabetical subject index.

[54] "Auszug aus dem Protocoll der Vorstands-Sitzung vom 29. Juni 1898," *Berichte der Deutschen Chemischen Gesellschaft* 31, no. 2 (1898): 1822–1823.

years after the Congress, the rules still failed to cover much of the terrain of organic chemistry. For those compounds that the Geneva rules did address, they argued, the formation of the new official names had worsened the problem of synonymy rather than alleviating it.[55] But for the purposes of their argument, Jacobson and Stelzner set aside the Geneva nomenclature itself in order to attack the principles behind it. Echoing the logic of Richter's "nomenclature proposal," they contended that even a system of official index names that fulfilled all of the aspirations of the Geneva nomenclature was doomed to disappoint the hopes of its creators.

Jacobson and Stelzner based this judgment on three properties of such a "rational register-nomenclature." First, they contended that such a nomenclature could never serve the purpose of everyday chemical naming ("text-nomenclature," as they called it). They pointed out that any useful new substance would inevitably receive a trivial name, for the sake of convenience. As for established trivial names, the editors asked readers to reflect on their own lexical attachments, asking whether they would surrender their freedom to use the beloved terms *toluene*, *xylene*, *aniline*, *anthracene*, or *naphthalene*. They invoked semiotic considerations, contrasting the algorithmic reasoning process required to convert Bouveault's proposed systematic term *pentazadiene* into a structural formula to the immediacy of the association between the idea of the formula and the familiar term *pyrrole*.[56] They conjured political analogies, suggesting that a rational register-nomenclature treated "all compounds as equals, as the citizens of a socialist State would be treated," while everyday nomenclature, like the German social order, "raises out from the great masses certain compounds to which fate or

[55] Friedel and his subcommission had expressed the same concern prior to the Geneva Congress. "Rapport de la Sous-Commission," *Association Française pour l'Avancement des Sciences: Compte rendu* 21 (1892): 394.

[56] All names rendered in contemporaneous English orthography.

merit has assigned a 'role.'"[57] For these reasons and more, Jacobson and Stelzner contended, a nomenclature of this kind would never spread beyond reference works.

Second, the editors argued that the division between register-nomenclature and everyday chemical names would lead to a parallel social division within the discipline of chemistry. The study of systematic nomenclature rules would crowd out other portions of chemists' training, and chemists would still have to learn the everyday names, too. Even worse, worried Jacobson and Stelzner, the complexity of the official nomenclature could lead to a division of labor between "register-educated scholars" who carried out literary work and "register-ignorant technicians" who carried out operations at the bench. "Such a state of affairs," they wrote, "would be highly undesirable and would contradict the traditions of our science" – that is, the union of head and hand in chemical research.[58]

Finally, they worried that the epistemological foundation of the rational register-nomenclature was dangerously flawed. The problem with establishing an official name for each compound based on its constitutional formula, they wrote, was that constitutional formulas were revised all the time. Jacobson and Stelzner were not the first to note this problem – Alexander Crum Brown, president of the Chemical Society of London in 1892, had raised the same concern in response to Henry Armstrong's report on the Geneva Congress.[59] Like Crum Brown, the *Berichte* editors worried that the adoption of systematic nomenclature would bring about a quandary in the case of compounds determined to have a different structure than had previously been accepted: either continue using a name that violated the stated rules of nomenclature and misrepresented the structure of the compound in question, or introduce a

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[57] Paul Jacobson and Robert Stelzner, "Zur Frage der Benennung und Registrirung der organischen Verbindungen," *Berichte der Deutschen Chemischen Gesellschaft* 31 (1898): 3371–3374.

[58] Ibid., 3374–3375.

[59] Henry Armstrong, "Contributions to an International System of Nomenclature. The Nomenclature of Cycloids," *Proceedings of the Chemical Society* 8 (1892): 130.

corrected name and generate just the sort of synonyms and uncertainty that the system was created to avoid. The recently-discovered phenomenon of tautomerism, in which the structure of a compound changed under different physical and chemical conditions, added a further complication, and others would surely follow. Even if solutions to such challenges could be conceived, Jacobson and Stelzner doubted whether the apparatus of international scientific cooperation was sufficiently nimble to settle upon revised methods of rational nomenclature at the pace of change in chemical theory.[60]

These fundamental problems with rational register-nomenclature, Jacobson and Stelzner concluded, had driven them to seek an alternative approach to organizing the index of the *Berichte*. They found Richter's formula index an ideal option. It eliminated the gap between register-nomenclature and text-nomenclature and the trouble of forming official register names. It would unify indexing systems rather than add to their number, since Richter was already using it in his *Lexikon*. Even for those not yet familiar with Richter's work, the formula index was easy to use; Jacobson and Stelzner claimed that it took less than a half-hour to get used to the new register.[61] And perhaps most importantly, it provided "a means to address the present indexing calamity immediately" rather than extending the interminable wait for internationally-sanctioned official names beyond the narrow scope of the Geneva rules.[62]

Unlike Richter, Jacobson and Stelzner did not claim any special authority over nomenclature. On the contrary, they wished to leave systematic nomenclature to others:

> Even within the formula register system, the improvement and standardization of nomenclature and numbering is of the utmost importance. We believe

[60] This was a longstanding challenge for scholars engaged in compilation. Linnaeus, for example, grappled with the challenge of maintaining sufficient flexibility in both his classification schema and the physical texts that he structured in accordance with it, developing an index-card-like filing system through trial and error; Isabelle Charmantier and Staffan Müller-Wille, "Carl Linnaeus's Botanical Paper Slips (1767–1773)," *Intellectual History Review* 24, no. 2 (2014): 215–238.

[61] Jacobson and Stelzner, "Zur Frage der Benennung," 3380–3383.

[62] Ibid., 3387.

> moreover, that this work, in which outstanding representatives of our science have taken part and hopefully will continue to take part, will be able to achieve much greater success with the acceptance of the formula-register system. For now the troublesome obligation to ensure a unique register location by means of names would fall away, and current efforts would be able to focus on the goal of the greatest possible simplicity and clarity.[63]

Jacobson and Stelzner did not want to put nomenclature reform into the hands of the editors, but to relieve nomenclature reformers of responsibility for making indexes. That was the job of editors like them.

As he was extending Richter's formula index system to the Berichte, Jacobson was also arranging for Richter to cross-reference his own reference work with Beilstein's. Richter agreed to include in the second edition of his *Lexikon* a reference to a volume and page number in the third edition of Beilstein's *Handbuch* after every entry covered in the latter work – some 57,000 compounds. Richter explained in his introduction that this made his *Lexikon* not only a valuable resource on its own and in conjunction with the index to the *Berichte* but also "a collective index *par excellence* to 'Beilstein.'"[64]

Thousands more entries in Richter's *Lexikon* did not contain these references, because the compounds had first appeared in the literature more recently than Beilstein had completed the corresponding section of his *Handbuch*. Immediately upon accepting his position with the Society, Jacobson set to work preparing a supplement to each of these four volumes covering new compounds and more recent literature references. With the aid of several assistants and the labor pool of abstractors absorbed along with the *Chemisches Zentralblatt* into the DChG

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[63] Ibid., 3388.

[64] Richter, *Lexikon der Kohlenstoff-Verbindungen*, 2nd. ed., vol. 1, 6.

publications apparatus, Jacobson prepared supplements to each of the four volumes of the third edition, published between 1900 and 1906.[65]

Beilstein thought that Jacobson did an exceptional job with this supplement, and wrote Fischer to tell him so. Generally rather modest about his *Handbuch* when he had been the one compiling it, Beilstein was intensely proud of Jacobson's achievement: "We have now a standard work before us that has no equal, and all other scientific disciplines can envy us for it." But he also expressed fears about what was to come. Beilstein worried about how the Society could possibly raise the money to fund the enormous amount of work that it would take to prepare a new edition, even more enormous than the nine volumes of the one just completed. And he remained as concerned as ever about the unsatisfactory organization of the text.[66]

Fischer and Jacobson solved the former problem with aggressive fundraising, starting a foundation to support the preparation of the new edition and accumulating over 250,000 marks ($60,000) from chemical industry sponsors before the first volume was printed in 1918.[67] As for the text's organization, assistant editors Bernhard Prager, Dora Stern, and Paul Schmidt developed an intricate scheme of chemical classification that they felt could be carried on indefinitely. The "Beilstein system," as it came to be known, classified organic substances into high-level divisions, classes, subclasses, and series, organized in a more rigorous and

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[65] The first supplementary volume, published in 1900, covered the literature through 1 Jul 1899. The fourth, published in 1906, covered the literature through 1 Jul 1903.

[66] Beilstein to Fischer, 2/15 Oct 1906, Emil Fischer papers, Box 9.

[67] Totals quoted in William A. Noyes, "Chemical Publications," *Journal of the American Chemical Society* 42, no. 11 (1920): 2106. The list of supporters included the major German chemical firms, as well as several individual notables such as Emil Fischer and Carl Duisberg. The Swiss pharmaceutical firm Hofmann-La Roche was the only non-German patron. This method of raising capital from industry paralleled the public-private partnership that Fischer had arranged to fund the Kaiser Wilhelm Institute for Chemistry, founded in 1911; Jeffrey Allan Johnson, *The Kaiser's Chemists: Science and Modernization in Imperial Germany* (Chapel Hill: University of North Carolina Press, 1990).

Beilstein himself proposed the rather quixotic plan of nominating the DChG, or perhaps the *Handbuch* itself, for a Nobel Prize, and using the prize money as an endowment for the *Handbuch*; Beilstein to Fischer, 2/15 Oct 1906.

detailed manner than the previous arrangement. For convenience, these low-level sections were assigned a permanent, consecutive numbering.[68] The Beilstein system was famously complex: it took the first forty-six pages of the first volume of the fourth edition to explain how it worked, and it spawned a large collection of keys and guidebooks.[69] Unlike the Geneva nomenclature, however, it was strictly a means of classifying and ordering substances, not a means of generating a name to be used in identifying them. Jacobson hewed to the same principle in the application of the complex Beilstein system as in that of Richter's simple empirical formula index: don't ask the same tool of organization to provide both an index location and a name.

This is not to say that Jacobson and his colleagues ignored nomenclature. Both the fourth edition of Beilstein's *Handbuch* and Stelzner's 1913 *Literatur-Register*, the DChG-edited continuation of Richter's *Lexikon*, contained detailed nomenclature guidelines.[70] But these guidelines were not immanent in the fabric of the reference work, as the Geneva delegates had imagined that their nomenclature would be. For the compilers and users of DChG reference works, the gears of nomenclature ground into action only after a compound had already been located precisely by a different sort of symbolic representation – an empirical formula, or a structural formula-derived, numerically-labeled position within the Beilstein system. Jacobson

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[68] For a contemporaneous English-language overview, see Donald W. MacArdle, "Classifying Organic Compounds by the Prager-Jacobson System," *Chemical and Metallurgical Engineering* 22, no. 6 (1920): 256–258.

[69] Bernhard Prager et al., eds., *Beilsteins Handbuch der Organischen Chemie*, vol. 1, 4th ed. (Berlin: J. Springer, 1918), 1–46. Not counting such brief overviews as MacArdle, "Classifying Organic Compounds," the first such guidebooks were Bernhard Prager, Dora Stern, and Konrad Ilberg, *System der organischen Verbindungen: Ein Leitfaden für die Benutzung von Beilsteins Handbuch der Organischen Chemie* (Berlin: J. Springer, 1929) and Ernest Hamlin Huntress, *A Brief Introduction to the Use of Beilstein's Handbuch der Organischen Chemie* (New York: J. Wiley & Sons, 1930). New guidebooks and revised editions continued to appear steadily into the twenty-first century, shifting focus during the 1980s and 90s to the use of the online database that supplanted the *Handbuch*; Stephen R. Heller, *The Beilstein System: Strategies for Effective Searching* (Washington, DC: American Chemical Society, 1998); Alexander J. Lawson, "The Beilstein Database," in *Handbook of Chemoinformatics*, ed. Johann Gasteiger (Weinheim: Wiley-VCH, 2003), 608–628.

[70] Jacobson had entered into the same agreement with Richter as he had with Beilstein. Richter edited the third edition of his *Lexikon*, published in 1910, with the understanding that the DChG would assume responsibility for the work thereafter; Fischer to Richter, 18 Jul 1910, Emil Fischer papers, Film Reel 5.

and his fellow editors pushed nomenclature to the side in favor of methods of organization that they felt would provide a lasting architecture for their texts and ensure the permanence of the considerable work already invested in their perpetual process of revision.

In 1908, the reference enterprises of the DChG were formally divided from the *Berichte*, with Jacobson as head of the new "Division for Chemical Compilations [*Abteilung für chemische Sammelliteratur*]."[71] Under Jacobson's continued leadership, his staff spent 1908-1912 carrying out the first step of producing a fourth edition of Beilstein's work, reorganizing the material from the third edition and its supplements under Prager, Stern, and Schmidt's new system. From 1913 through 1916, they collected literature published between 1899 and 1910, nearly doubling the number of compounds to be listed in the *Handbuch* to approximately 140,000; the first volume of this fourth edition was published in 1918.[72] Stelzner continued to publish the *Literatur-Register* as a formula index listing more recent citations from the major chemical journals, bridging most of the ever-increasing gap between the literature covered by *Beilstein* and the latest publications. In 1926, the DChG reorganized the *Literatur-Register* as an annual formula index to the *Zentralblatt*, tying the Society's three major reference publications into a unified system. But by that time, German chemistry had a very different geopolitical standing, and the DChG's prodigious enterprise in chemical information had a rival across the Atlantic.

Nomenclature in America, 1880s-1900

The internationalism of the Geneva Congress extended only very tenuously to America. Only one American, Johns Hopkins chemist Ira Remsen, was included among Friedel's Geneva invitees, and he did not attend.[73] Americans gave the Geneva Congress little more attention

[71] Robert Stelzner, ed., *Literatur-Register der Organischen Chemie*, vol. 1 (Braunschweig: Vieweg, 1913), xv.

[72] Jacobson, "Beilsteins Handbuch der Organischen Chemie," 222.

[73] "Rapport de la Sous-Commission," 393. Remsen received his PhD from Göttingen in 1870, after studying with Volhard in Munich and Fittig in Göttingen. Given the prevalence of close associates of Friedel among the Geneva

than its organizers had given them. A couple of journals, including Remsen's, reprinted the account of the Congress that Armstrong had published in *Nature*, and an MIT instructor studying with Geneva attendee Ernst von Meyer returned from Leipzig with a translation of Pictet's proceedings to insert into the Institute's quarterly, but that was the extent of the discussion of the Geneva rules in American periodicals.[74]

American chemists of the late 1880s had nonetheless been concerned about the state of organic chemical nomenclature. In 1885, Albert Leeds, analytical chemist and vice-president of the decade-old American Chemical Society (ACS), assembled a committee of five chemists "to consider a system of nomenclature and notation, to be used in the publications of the Society."[75] A year later, the committee published its recommendations in a three-page report. In deference to "the importance of uniform practice in respect to nomenclature and notation among chemists speaking the same language," the committee recommended adopting the

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invitees, it is a bit surprising that Friedel's dear friend and longtime collaborator James Mason Crafts was not selected as an American representative. However, Crafts does not seem to have participated in the 1889 Congress. Further, by 1889, Crafts had been in Paris for fifteen years, casting his status as suitable representative of American chemistry in doubt. His return to America in 1891 to take up a professorship of organic chemistry at MIT may also have forestalled an invitation to Geneva. Alternatively, Remsen's status as founder and editor of the *American Chemical Journal*, the leading chemical periodical in the US, may have earned him the nod. On Remsen, see William A. Noyes and James F. Norris, "Ira Remsen, 1846-1927," in *Biographical Memoirs of the National Academy of Sciences*, vol. 14 (Washington, DC: National Academy of Sciences, 1931), 206–257. On Crafts, see Charles R. Cross, "James Mason Crafts, 1839-1917," in *Biographical Memoirs of the National Academy of Sciences*, vol. 9 (Washington, DC: National Academy of Sciences, 1918), 158–177.

[74] Henry E. Armstrong, "Reviews and Reports: The International Conference on Chemical Nomenclature," *American Chemical Journal* 15, no. 1 (1893): 50–59. Henry E. Armstrong, "The International Conference on Chemical Nomenclature," *The Journal of Analytical and Applied Chemistry* 6 (1892): 390–400. P. S. Burns, "Reforms in the Nomenclature of Organic Chemistry," *Technology Quarterly and Proceedings of the Society of Arts* 5 (1892): 229–249. (Pictet was not credited.) On Burns' study with Meyer, see P. S. Burns, "Beiträge zur Kenntniss der polymeren Nitrile. VII. Abhandlung. Chemisches verhalten einiger dimolekularen Nitrile," *Journal für praktische Chemie* 47, no. 1 (1893): 105–134; Anja Werner, *The Transatlantic World of Higher Education: Americans at German Universities, 1776-1914* (New York: Berghahn Books, 2013), 146.

[75] C. E. Munsell, "Proceedings of the American Chemical Society," *Journal of the American Chemical Society* 7, no. 1 (1885): 39. Through 1892, the ACS was primarily a local society based in New York City. It took off as a national organization after reorganizing to allow other regional societies to join as local sections and new local sections to be formed. The Society rotated the site of its national meeting among these local sections. See Arnold Thackray et al., *Chemistry in America, 1876-1976: Historical Indicators* (Boston: Reidel, 1985); Charles Albert Browne and Mary Elvira Weeks, *A History of the American Chemical Society, Seventy-Five Eventful Years,* (Washington, DC: American Chemical Society, 1952).

nomenclature system already codified by the Chemical Society of London, and appended the nomenclature rules from Watts' "Instructions to Abstractors" to their report.[76]

While the American committee hewed to Watts' recommendations in specifics of nomenclature, they did depart from his recommendations on certain matters of principle. First, the Americans were less willing to follow Watts in sacrificing clarity, as they saw it, in the interest of economy. For example, the American committee reversed some of Watts' notation recommendations, advocating the use of dashes instead of dots to indicate bonds and the full formula for radicals rather than abbreviations like Ph and Ac. Second, the committee emphasized that their principles were intended "merely for general guidance," and that any author who felt hampered by these guidelines "should be allowed the widest liberty that is consistent with clearness of expression." They saw nomenclature reform and standardization as a way to make contemporary chemistry more accessible to students, people engaged in chemical industry and commerce, scientists in related fields, and others "who are less acquainted with its technical details." And while the committee members were not prepared at that time to go beyond Watts' limited guidelines, like Friedel and his cohort, they saw a much more ambitious reform of chemical nomenclature as imperative and quite feasible. "It may well be asked," they wrote, "whether the whole subject of nomenclature should not be considered anew by chemists, with a view to the introduction of radical changes at an early day."[77]

A year later, the Chemistry Section of the American Academy for the Advancement of Science formed a Committee on Orthography and Pronunciation of Chemical Terms.[78] The four-person group, whose membership did not overlap with the ACS committee, was chaired by

[76] A. A. Brenneman et al., "Report of the Committee on Nomenclature and Notation," *Journal of the American Chemical Society* 8, no. 6 (1886): 116–118.

[77] Ibid., 117–118.

[78] Thomas H. Norton, "The Spelling and Pronunciation of Chemical Terms," *Science* 20, no. 510 (1892): 272–274.

Thomas Norton, professor of chemistry at the University of Cincinnati.[79] Unlike Friedel's subcommission, Norton's committee sought to incorporate a national perspective into its study. From 1887 through 1891, the committee repeatedly solicited the opinions not only of Chemistry Section members but also of philologists and "American Chemists in general."[80] The committee drew its mandate from the responses of the latter, which testified to "the earnest desire existing among our fellow chemists to have fixed rules of pronunciation replace the existing irregularities, divergencies and uncertainty."[81]

This was a problem of a somewhat different order than the matters of nomenclature that Friedel's International Commission was tackling across the Atlantic. The majority of the committee's final report consisted of a guide to the pronunciation of chemical elements and both organic and inorganic chemical names, detailing such matters as where to place accents and how to pronounce the vowels in common prefixes and suffixes.[82] As with the ACS committee, the few propositions that addressed matters of written nomenclature hewed to Watts' recommendations, dictating, for example, that the use of the suffix *–ol* be restricted to alcohols and phenols, and a terminal *e* added to the names of compounds such as *pyrrol(e)* and *indol(e)*.

The character of the committee's work was best captured in its extensive discussion of what to do about substances ending in *–in(e)*. Watts had instructed authors to use the termination *–ine* for basic substances such as *aniline* and the alkaloids (*caffeine*, *nicotine*, *quinine*, etc.) and to reserve *–in* (the ending of the German versions of the aforementioned

[79] The other members were James Lewis Howe and H. Carrington Bolton, both prominent chemical bibliographers, as well as analytical chemist Edward Hart.

[80] Thomas H. Norton et al., "Report of the Committee on Spelling and Pronunciation of Chemical Terms," *Journal of the American Chemical Society* 14, no. 3 (March 1, 1892): 63.

[81] Thomas H. Norton, "Report of the Committee on Spelling and Pronunciation of Chemical Terms," *Proceedings of the American Association for the Advancement of Science* 39 (1890): 150.

[82] This report was likely the only article ever published in the *Journal of the American Chemical Society* to contain a guide to the phonetics of English vowels in the footer of each page.

names, pronounced "een" in that language) for neutral compounds such as *glycerin*. Norton's committee, however, found this guideline unwise, deeming "a difference in spelling, with little or no corresponding difference in pronunciation on the part of most chemists,— a useless and undesirable complication," particularly since it demanded "a very extensive knowledge of the constitution of a great number of compounds." Instead, the committee advocated dropping the final *e* in all cases, reckoning that the letter was "of no important significance."[83] This judgment was premised on a logocentric conception of chemical language. Norton and his colleagues argued that the lack of clear phonetic function of the terminal *e* in spoken language disqualified it from taking on chemical significance in written chemical names.[84]

As for how to pronounce this –*in* stripped of its -*e*, the committee discarded "een" as too Continental to be accepted in English usage (notwithstanding its vogue in chemical industry marketing, as in the detergents *Pearline* and *Soapine*). The long "ayn" was even worse; the committee members anticipated that it would be an uphill battle to convince anyone to pronounce *chlorine* or *morphine* in this way. They therefore settled on the short "in," already used in such terms as pepsin and tannin, and phonetically close enough to the long "een," "into which it may easily be strengthened if preferred."[85] Like the Geneva Congress, this American committee sweated the details, but the kinds of details that each sweated were different. Taking the demands of spoken language seriously in a discussion of nomenclature reform changed the terms of this discussion considerably.

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[83] Norton, "Report of the Committee on Spelling and Pronunciation" (1890), 155–156.

[84] The philologists that the committee interviewed might have contributed to this line of reasoning, which is in line with the spelling reforms proposed by Melvil Dewey. Committee members Bolton and Howe might have known Dewey through their bibliographic activities. On Dewey's spelling reform efforts, see Wayne A. Wiegand, *Irrepressible Reformer: A Biography of Melvil Dewey* (Chicago: American Library Association, 1996), 242–250.

[85] Thomas H. Norton, "Report of the Committee on Spelling and Pronunciation of Chemical Terms," *Proceedings of the American Association for the Advancement of Science* 38 (1889): 194. Funk & Wagnalls, at least, took this guideline to heart, dutifully listing the –*in* versions of these chemical names as preferred variants in their dictionaries through at least the 1920s.

Americans also confronted the problem of chemical indexing that Baeyer, Richter, and Jacobson had tackled. There was no American publication like Beilstein's *Handbuch* or Richter's *Lexikon*, but American chemists working in other settings searched for a reliable means of keeping track of chemical substances. Edwin Hill, a chemist employed by the US Patent Office, encountered the problem in the context of patent examination.[86]

Hill proposed creating a formula index very much like Richter's.[87] No slouch when it came to matters of priority in invention, Hill insisted that he had come upon the idea independently, and, furthermore, that his method of compiling and organizing a formula index was superior to Richter's. For one thing, Hill's method included inorganic compounds as well as the organic compounds that Richter's method addressed. Hill also cited his adoption of simple alphabetical order (with the exception of the ubiquitous C's and H's) as a feature that made his system easy to understand and apply, especially for laymen. Richter's system, in contrast, required memorizing a chemically-motivated order for elements within formulas and grouped numerous derivatives and salts with a parent compound rather than strictly by formula (*Fig. 6, appended*). Hill granted that Richter and Jacobson thereby attained a certain degree of classification as well as ordering within their publications. But Hill cast that very hybridity as a weakness of Richter's method: "We cannot sit on two stools at once, we will surely fall between them; and too much classification grafted on the dictionary or digest idea will give what is neither a good digest nor a good classification."[88] If Jacobson and Stelzner had

[86] Under American law, a new chemical substance or process was patentable only if it had not been described in a publication either prior to the applicant's discovery or more than two years before the date of the application; Edwin A. Hill, "On a System of Indexing Chemical Literature; Adopted by the Classification Division of the U. S. Patent Office," *Journal of the American Chemical Society* 22 (1900): 478. Chapter 4 addresses chemical patents in more detail.

[87] In Hill's system, each compound was indexed according to its molecular formula, written beginning with carbon and hydrogen and then proceeding element by element in alphabetical order. These compounds then would be arranged in alphanumeric order, considering symbols and coefficients from left to right; Ibid., 478–494.

[88] Ibid., 480.

shown that nomenclature couldn't do what Baeyer had demanded of it, Hill critiqued the formula index they'd adopted instead. He did so on grounds analogous to those that Baeyer had advanced at Geneva. There were reference works for *classifying* things, and then there were reference works for *finding* arbitrary substances; each demanded its own notation.

William A. Noyes and *Chemical Abstracts*

It was not information overload but a publicity problem that drove the creation of the first American chemical abstract journal. In 1895, frustrated by the lack of coverage of American publications in European abstracts, MIT physical chemist Arthur Noyes decided to introduce a "Review of American Chemical Research" in *Technology Quarterly,* the journal of the Institute's scholarly society. Noyes' aim was to abstract "all original work having a chemical bearing published in the United States after the beginning of the year 1895."[89] Noyes edited the *Review* for several years, but in 1901, as he shifted his sights to establishing a research laboratory of physical chemistry at MIT, he sought to divest himself of the burden of compiling these abstracts.[90] He passed on the job to the man he'd just nominated to take over editorship of the *Journal of the American Chemical Society*: his distant relation William Noyes.[91]

William A. Noyes was one of the foremost institution-builders in American chemistry during the first third of the twentieth century. After completing a PhD with Remsen at Johns Hopkins in 1882, Noyes returned to his native Midwest, where he spent seventeen years as professor of chemistry at the new Rose Polytechnic Institute in Terre Haute, Indiana. He made the acquaintance of numerous European chemists on a sabbatical year abroad in 1888-89, during which he spent several months working in Baeyer's laboratory in Munich. In 1903, he

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[89] Arthur A. Noyes, ed., "Review of American Chemical Research," *Technology Quarterly* 8, no. 1 (1895): 90.

[90] Linus Pauling, "Arthur Amos Noyes, 1866-1936," in *Biographical Memoirs of the National Academy of Sciences*, vol. 31 (Washington, DC: National Academy of Sciences, 1958), 323–346.

[91] *Proceedings of the American Chemical Society* (1902): 25–26. The *Journal* had been reprinting the "Review" since 1897; Crane and Patterson, *A Guide to the Literature of Chemistry*, 86.

became Secretary of the American Chemical Society as well as editor of the Society's *Journal*; he also left Terre Haute to become the inaugural Chief Chemist of the new National Bureau of Standards in Washington, DC. Relieved of teaching responsibilities and with high-profile positions in government and in the rapidly growing Society, Noyes quickly gained an international reputation for his analytical and organic chemical research. In 1907, after a year of recruiting, the president of the University of Illinois convinced Noyes to return to the Midwest. Noyes took up chairmanship of the University's chemistry department that fall, and remained in Urbana-Champaign until his death in 1941. [92]

Noyes had a special passion for publications. Through his cousin's tenure as editor, the "Review of American Chemical Research" had held steady at about 150 pages of abstracts per year. By 1906, Noyes had recruited a staff of abstractors specializing in different chemical subfields. Together, his team produced over 600 pages annually, including abstracts of chemically-relevant patents. Like the abstracts of the British *Journal of the Chemical Society*, these were printed in a separately-paginated section bound with each monthly copy of the *Journal of the American Chemical Society* (*JACS*). In 1905, the growing scope of Noyes' abstracts led the ACS Council to propose that the Society join forces with its British counterparts, the Chemical Society of London and the Society for Chemical Industry. By uniting their abstracts into a single omnibus English-language chemical abstract journal, the societies could at once reduce administrative overhead and achieve economies of scale in printing by merging three editorial operations and three populations of subscribers into one.[93] The negotiations with the

[92] Roger Adams, "William Albert Noyes, 1857-1941," in *Biographical Memoirs of the National Academy of Sciences*, vol. 26 (Washington, DC: National Academy of Sciences, 1952), 178–208. On Noyes, see also Austin M. Patterson, "William Albert Noyes, 1857-1941," *Science* 94, no. 2447 (1941): 477–479; Roger Adams, "Dedication of the William Albert Noyes Laboratory of Chemistry at the University of Illinois," *Science* 90, no. 2326 (1939): 71–75 (including autobiographical remarks by Noyes); Austin M. Patterson, "American Contemporaries: William Albert Noyes," *Industrial & Engineering Chemistry* 16, no. 4 (1924): 420.

[93] *Proceedings of the American Chemical Society* (1905): 62.

British fell through, but Noyes wished to press on anyway. The membership of the Society overwhelmingly approved a referendum to raise annual dues from $5 to $8 to support the new abstract journal, which, like *JACS*, would be sent to all members at no additional charge as a benefit of membership.[94] In January of 1907, ACS members received their first issue of *Chemical Abstracts*, edited by W.A. Noyes.

Even with Noyes' expansion of its size, the "Review" had remained restricted to abstracts of publications in American and Canadian journals, in accordance with Arthur Noyes' original intention of publicizing American chemistry. In contrast, William Noyes formed *Chemical Abstracts* with the ambition of complete coverage of the entire field of chemistry, including industrial and applied topics and chemically relevant publications in allied fields like physics, abstracting papers from everywhere in the world that original chemical research was published.[95] Noyes urged the Society to "place before itself the ideal that we will, in the end, make the Journal both the most complete and the best Abstract Journal in the world."[96] Relative to its competitors, at least, Noyes judged that the journal was not far from achieving this aim. The 3,046 pages of the first volume of *Chemical Abstracts* contained 7,975 abstracts classified under thirty subject headings, in comparison to the 7,219 abstracts and fourteen divisions of that year's *Zentralblatt*. *Chemical Abstracts*' circulation of over 3,000 was half again as great as the *Zentralblatt*'s.[97] The participation of a salaried associate editor and 129 "volunteer" abstractors and section editors under the supervision of Noyes and made this possible.[98]

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[94] Noyes, "Chemical Publications," 2105.

[95] On the ambitions of the founders of *Chemical Abstracts*, see Dale B. Baker, Jean W. Horiszny, and Wladyslaw V. Metanomski, "History of Abstracting at Chemical Abstracts Service," *Journal of Chemical Information and Computer Sciences* 20 (1980): 193–194.

[96] *Proceedings of the American Chemical Society* (1908): 16.

[97] Ibid., 15–16.

[98] Baker, Horiszny, and Metanomski, "History of Abstracting," 198. Notwithstanding the oft-repeated claim that abstractors worked on a volunteer basis for the first two decades of the publication of *Chemical Abstracts*, the line

In the summer of 1907, Noyes moved to Urbana to take charge of the chemistry department of the University of Illinois. Given the demands of this new position and the compilation of *Chemical Abstracts*, Noyes negotiated relief from his position as secretary of the Society, as well as "competent assistance on his editorial staff."[99] To fill the latter role, he looked to a colleague who had already taken a marked interest in chemical language.

Noyes had first met Austin Patterson when Patterson came to Rose Polytechnic as an instructor in chemistry in 1901. The two men had much in common – Patterson, too had grown up in the Midwest and had received his PhD under Remsen, in 1900. The pair co-authored three papers in 1902, and Noyes exercised a strong influence upon his younger colleague.[100] When Noyes left Terre Haute for Washington, Patterson moved back to his family farm in central Ohio, never to publish another experimental or theoretical article. Patterson's scholarly career in chemistry, however, continued unabated. He had long had a particular facility with languages, and had studied Classics as well as chemistry as an undergraduate at Princeton. In 1903, he was named chemical editor of Webster's New International Dictionary, and served in that role until the end of 1908, when he took a position as Noyes' deputy at *Chemical Abstracts*. Noyes and the ACS Council considered the position important; Patterson was to receive $1,325 for the year, to Noyes' $650 as editor of both *JACS* and *Chemical Abstracts*.[101]

Sure enough, Noyes had brought in a deputy, but he wanted a successor. Only a few months later, Noyes handed over effective control of the abstract journal's editorial operations

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item for payments to abstractors in the annual budgets of the ACS, printed in the *Proceedings*, demonstrates that they were allotted a (small) fee for their piecework.

[99] *Proceedings of the American Chemical Society* (1907): 70

[100] In a 1941 biographical sketch of Patterson, E. J. Crane quoted Patterson as stating that "intimate contact with William A. Noyes, Sr., meant more to me than any university course." One example of that influence may be Patterson's decision to join the ACS in 1901; "List of Officers and Members," annex to *Proceedings of the American Chemical Society* (1908), 67.

[101] *Proceedings of the American Chemical Society* (1909): 28.

to Patterson.[102] The transition became official with Patterson's election as editor of *Chemical Abstracts* at the beginning of 1910. Patterson moved editorial operations from Urbana-Champaign to the campus of Ohio State University in Columbus, sixty miles from his Xenia homestead, and with the blessing of the ACS council hired Ohio State chemist J. J. Miller as his associate editor.[103]

Patterson carried on his mentor's agenda at *Chemical Abstracts*, continuing to expand the coverage of the publication. In his annual report for 1912, he wrote, "For the first time we believe we can now claim, with some show of truth, that [*Chemical Abstracts*] covers its field completely." That year, 262 abstractors and assistant editors had abstracted 598 periodicals. The catalog of these journals was itself a resource, as "the most complete list which we know of periodicals which are chemical or likely to contain original chemical articles." The volume, the journal's sixth, contained nearly three times as many abstracts as had its first.[104]

Patterson's new position also gave him occasion to continue to think about chemical language, though in a somewhat different manner than he had as an editor for Webster's. In the summer of 1909, around the time that he was taking the reins of *Chemical Abstracts*, he gave a lecture on chemical nomenclature at a meeting of the Illinois section of the American Chemical Society, probably at the invitation of Noyes.[105] He may have been testing out ideas that he wished to bring to bear on his steadily growing publication; in 1910, Patterson expanded the two pages of nomenclature guidelines at the beginning of the 1908 volume to an 8-page, 122-point pamphlet.[106] Noyes was soon directing authors of papers for *JACS*, which he

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[102] Noyes to Crane, 10 May 1941, W.A. Noyes papers, Box 15, Folder "General Correspondence, 1940-41."

[103] *Proceedings of the American Chemical Society* (1909): 92–93, 95; *Proceedings of the American Chemical Society* (1910): 39.

[104] *Proceedings of the American Chemical Society* (1913): 5–6.

[105] *Proceedings of the American Chemical Society* (1909): 73.

[106] Baker, Horiszny, and Metanomski, "History of Abstracting," 199.

continued to edit until 1920, to write Patterson in Columbus for copies of the *Chemical Abstracts* rules of nomenclature and "style guide."[107] At the beginning of 1912, Patterson resuscitated the American Chemical Society's nomenclature committee, stipulating that its members include the editors of *JACS*, *Chemical Abstracts,* and the new *Journal of Industrial and Engineering Chemistry*.[108] The first of those editors was Noyes, who happily delegated authority to Patterson whenever possible; the second was Patterson himself; the third, Milton Whitaker, made a habit of requesting nomenclature assistance, not providing it.[109] Patterson had a passion for language befitting the nation's leading expert in chemical nomenclature, and as editor of *Chemical Abstracts*, the role fell to him almost by default.

The same year, Patterson arranged for a portion of his salary to be reallocated to help finance a second paid associate editor. To fill the position, the Ohio State chemistry faculty recommended Evan Jay Crane, a bright young man from Columbus with an aptitude for both chemistry and writing. Crane had just graduated from Ohio State, but from the perspective of the Society his correspondingly modest salary demands likely made up for any concern about lack of experience.[110] He accepted the offer and set to work under the tutelage of Patterson and associate editor J.J. Miller.

In late 1914, trouble struck. First, the outbreak of war in Europe – a war in which the US remained neutral until the spring of 1917 – interrupted the transatlantic traffic in chemistry journals. For a young publication whose value lay in providing American chemists with access

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[107] *Proceedings of the American Chemical Society* (1912): 20.

[108] Ibid., 35. The committee also included a small number of additional appointed members. The previous incarnation of the ACS Committee on Nomenclature and Spelling had been disbanded in 1905; *Proceedings of the American Chemical Society* (1905): 62. The new commission was created in part as a way of coordinating the work of the American subcommittees to new international committees on inorganic, organic, and biological nomenclature, formed under the International Association of Chemical Societies, founded the previous year (see Chapter 5).

[109] *Proceedings of the American Chemical Society* (1912): 24; *Proceedings of the American Chemical Society* (1913): 7.

[110] *Proceedings of the American Chemical Society* (1912): 44; Austin M. Patterson, "An Appreciation of E. Jay Crane," *Chemical & Engineering News* 25, no. 48 (1947): 3561.

to research reported in these journals, this was a crisis.[111] Second, in November, Patterson contracted tuberculosis, and resigned his position as editor-in-chief to move south to convalesce.[112] Third, barely a month after Patterson resigned, Miller also stepped down.[113] The twenty-five-year-old Crane found himself the only person left to run *Chemical Abstracts* as the wartime challenges to journal circulation deepened. It would take less than a year, however, for the young editor to flex his muscles.

Indexing *Chemical Abstracts*

In October 1915, the young editor of *Chemical Abstracts* presented a plan to the Council of the American Chemical Society. Crane proposed that it had become necessary to print a collective index to *Chemical Abstracts*, covering the entire contents of the journal from its founding in 1907 through the upcoming tenth volume to be published in 1916. With the Council's blessing – provided that he could demonstrate sufficient interest and financial support – Crane took his case to the Society's membership, circulating subscription cards and presenting his idea at the meetings of local sections.[114]

The Council may not have understood the scope of Crane's ambitions. The following June, Crane estimated that it would cost $30,000 to print the approximately 3,000 copies that he estimated would be needed, plus $6,500 to compile the index.[115] This was more than the total 1916 budget for *Chemical Abstracts* itself, and half again as much as the Society allocated

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[111] *Proceedings of the American Chemical Society* (1915): 4–5.

[112] *Proceedings of the American Chemical Society* (1914): 101; E. J. Crane, "Austin McDowell Patterson," *Chemical & Engineering News* 19, no. 13 (1941): 743.

[113] According to Dale Baker, another Ohio State graduate who rose through the ranks of *Chemical Abstracts*, Miller had a history of psychiatric problems, and an acute episode of psychological distress soon after his elevation to editor led to his decision to step down. Dale B. Baker, interview by Robert V. Williams and Leo B. Slater at Columbus, Ohio, 9 June 1997 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0160), 8.

[114] *Proceedings of the American Chemical Society* (1915): 79.

[115] *Proceedings of the American Chemical Society* (1916): 68, 70.

to its flagship journal, *JACS*.[116] However, Crane had by then already raised $20,000 in subscriptions, and the Council authorized him to proceed.[117] The price of the index was commensurate with its cost: $10 for early subscribers and $15 for non-members, whereas *Chemical Abstracts* cost non-members $6 per year and was included, along with the society's two other journals, in the annual membership fee of $10.[118]

Indexing was central to Patterson's and Crane's idea of the kind of resource that *Chemical Abstracts* should be. In 1927, they wrote:

> "Because of the belief that a record such as an abstract journal constitutes is of greatly lessened value if the information is not made readily and certainly available by thorough indexing, more attention has been given to the indexing, particularly the subject indexing, of *Chemical Abstracts* than to any other feature."[119]

The journal's editors accordingly devoted significant resources to the compilation of the index, an annual crunch that absorbed the attention of abstractors and editors every November through January.[120] The 1907 subject index contained 19,000 entries; by 1912, it contained 70,000, a number that prompted Patterson to praise "the faithful work of the office force and the printers who now face annually a herculean task in the index."[121]

The indexers of *Chemical Abstracts* faced challenges common to subject indexing in most technical fields: how to categorize the field's myriad concepts under a consistent scheme of subject headings and subheadings, whether to lump or split potential near-synonyms, and so

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[116] Ibid., 10.

[117] The Council also authorized the Secretary to begin to solicit chemical manufacturers for subscriptions. By November 1916, the financing for the Index was secure enough that the ACS Board of Directors voted not to include advertisements; Ibid., 108.

[118] Ibid., 68; "Increase the Dues," *Journal of Industrial and Engineering Chemistry* 12, no. 9 (1920): 824.

[119] Crane and Patterson, *A Guide to the Literature of Chemistry*, 89, quoted in D. F. Zaye, W. V. Metanomski, and A. J. Beach, "A History of General Subject Indexing at Chemical Abstracts Service," *Journal of Chemical Information and Computer Sciences* 25, no. 4 (1985): 394.

[120] Crane wrote of "the index rush period which keeps our hands overfull here during November, December, and January"; Crane to Members of the ACS Committee on Nomenclature, Spelling and Pronunciation (hereafter NSP), 13 Feb 1935, W.A. Noyes papers, Box 12, Folder 1935.

[121] *Proceedings of the American Chemical Society* (1913): 6.

on.[122] However, a large majority of subject headings in the index of *Chemical Abstracts* were not technical terms but chemical names. The indexing of chemical names presented an entirely different sort of challenge, one in which, wrote Crane and Patterson, the "**first prerequisite** … is the adoption and consistent application of a **definite system of nomenclature.**"[123] The principle was born of experience; Crane identified the "lack of uniformity in nature and quality of the indexes from year to year" as the greatest shortcoming of the early volumes of *Chemical Abstracts*. Crane saw the collective index not only as a text that would unite a decade's worth of references in one place, but also as a means of correcting poor indexing practices of the past.[124]

Further, the collective index could be more than just a means for more effective use of *Chemical Abstracts*. In 1900, Edwin Hill (the creator of the Patent Office formula index system) called for the ACS and the Smithsonian Institution to work together to enlist the "vast army of chemical workers in our universities, laboratories, and corporate and government offices and institutions" to build a master index of chemical substances and references.[125] No such effort ever came to pass, but businesses and universities began to assemble such catalogs on a smaller scale. Patterson and Crane felt that "a good well indexed abstract journal should go a long way toward making such [proprietary] indexes unnecessary in most instances."[126] The index to *Chemical Abstracts* could become a compact, English-language index to the chemical world – if its creators could come up with a suitable system of nomenclature.

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[122] Zaye, Metanomski, and Beach, "History of General Subject Indexing."

[123] Crane and Patterson, *A Guide to the Literature of Chemistry*, 178. (Bold in original.)

[124] Crane remarks quoted in Noyes, "Chemical Publications," 2111. Subsequent collective indexes continued to serve the purpose of standardizing naming and terminology over ten-year and later five-year periods. In 1963, an editor wrote that these collective works were "not just a compilation of annual and semiannual indexes. [They] correlate developments which have occurred in the interim and incorporate important changes brought about by the standardization of nomenclature, the introduction of new terms and the crystallization and refinement of others." John Troan, *An Adventure in Knowledge: The Story of the Chemical Abstracts Service* (Washington, DC: American Chemical Society, 1963), 17, quoted in Zaye, Metanomski, and Beach, "General Subject Indexing," 397.

[125] Hill, "On a System of Indexing Chemical Literature," 492–494.

[126] Crane and Patterson, *A Guide to the Literature of Chemistry*, 194.

This task fell to Patterson, who by 1916 had recovered from his bout with tuberculosis and returned to his Xenia, Ohio home. Patterson was aided in his efforts by Carleton Curran, yet another Midwesterner who had recently received his M.A. in chemistry at Ohio State and took a position at *Chemical Abstracts* in 1916 as he worked toward his PhD.[127] Patterson and Curran had the same goal as had the Geneva delegates twenty-five years before: building a system of nomenclature for use in an index. But their approach to the task was very different. Whereas Baeyer and the Geneva delegates had been out to create a basis for a nomenclature of future indexes, Patterson and Curran were engaged in wrestling with existing ones. They experimented with the raw material of nine years' worth of abstracts and index entries, renaming and rearranging these compounds into the best order they could come up with – ideally one that would minimize the amount of work that had to be repeated when the time came to make another collective index ten years later. The Geneva rules were a nomenclature created for indexes. Patterson and Curran's system was a nomenclature created through indexing.

After about a year of work, Patterson and Curran published an article detailing the results of their efforts. Their description of their aims contrasted sharply with that of the Geneva Congress. They had "not tried to invent a new system." Rather, they were publishing their principles for the information of index-users and as a contribution to the general discussion of chemical nomenclature. Furthermore, they sought to create index names "suitable for general use, and not only for the Index."[128] Patterson and Curran did not accept that a system of nomenclature for indexes could not be used in everyday chemistry – the principle

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[127] Carleton Edgar Curran, "Autobiography," in "A Comparative Study of the Reactions of Nitrosophenol and N-Chloroquinonimine with Aromatic Amines" (PhD Dissertation, Ohio State University, 1921), 32; "Necrology, Carleton E. Curran," *News Edition, American Chemical Society* 19, no. 1 (1941): 36.

[128] Austin M. Patterson and Carleton E. Curran, "A System of Organic Nomenclature," *Journal of the American Chemical Society* 39, no. 8 (1917): 1623.

that Baeyer installed as the basis of the Geneva nomenclature, and a key part of Jacobson and Stelzner's argument for rejecting the Geneva rules.

Patterson and Curran's plan was based first and foremost on a practical insight: a system of nomenclature need not be entirely systematic – that is, every name need not be rigorously determined by rules – in order for the names that made it up to be unique. Instead of replacing trivial names with systematic ones, Patterson and Curran used trivial names as often as they could, both as index names for the substances known by these names and as starting points for naming their derivatives.[129] They noted that this approach was "opposite to that of a work like Richter's 'Lexikon,' which reduces each compound to the derivative of as simple a parent substance as possible."[130]

Similarly, they determined that the parts of the name did not need to be assembled exclusively according to chemical considerations. With "over two hundred radicals the names of which are in common use" and more likely on the way, learning an order of chemical precedence for arranging the prefixes representing these substituents in a chemical name (not to speak of creating such an order) would be a daunting task. On the other hand, "the alphabetic system gives an instant means of deciding their order, in any combination." This simple alphabetical ordering of prefixes in each chemical name was "as much to be preferred to any order based on chemical considerations as an alphabetic index to a general chemistry is to one arranged according to the periodic system." This meant that the order of prefixes in some names would change from one language to another, a concern of which Patterson would have been well aware, having just published a German-English dictionary for chemists.[131] But the

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[129] For example, Patterson and Curran used "acetic, isobutryic, acrylic, glutaric, cinnamic, gallic, etc. [acid]" for the compounds whose Geneva-style names were ethanoic, 2-methylpropanoic, 2-propenoic, pentandioic, 3-phenyl-2-propenoic, and 3,4,5-trihydroxy-1-methyl-1[1]-benzenoic acid; Ibid., 1633.

[130] Ibid., 1623.

[131] Austin M. Patterson, *A German-English Dictionary for Chemists*, 1st ed. (New York: John Wiley & Sons, 1917).

immediate task facing Patterson and Curran was building an index to an American publication, not developing international nomenclature standards. They dismissed this concern as the business of translators, not indexers.[132]

Though Patterson and Curran sought names for general use, they also sought ways to adapt these names to the particular demands of indexing. The most important of these was the "inverted index." In the index to *Chemical Abstracts*, they explained, each chemical name would be written with the root or "index compound" first (e.g., "acetic acid, chloro-" for "chloroacetic acid"). This approach had occasionally been put to use in the *Berichte* and in the *Journal of the Chemical Society*, but Patterson and Curran applied it exhaustively. The method had "the great advantage of bringing related compounds together to a much larger extent than is possible in the ordinary way, while yet preserving the advantages of an alphabetic arrangement."[133]

This inverted format made chemical names somewhat more legible, but not always perfectly so, especially in the case of molecules whose substituents had their own substituents. Accordingly, the index-makers also made generous use of punctuation – parentheses, brackets, and even braces – to make names more readable and avoid ambiguity in cases of nested prefixes (e.g., the index names *Malonic acid, chloromethyl-* and *Malonic acid,* (*chloromethyl-)* corresponded to two different compounds; see *Fig. 7*). "That there should be different names of like sound and spelling which must be distinguished to the eye by enclosing marks is not ideal," the two Ohioans acknowledged. "Yet they are scarcely to be avoided in naming organic compounds. Attempts to do this often result in making the name unintelligible."[134] Patterson and Curran did not dismiss the importance of spoken names, but their index-making enterprise

[132] Patterson and Curran, "A System of Organic Nomenclature," 1626–1627.
[133] Ibid., 1624.
[134] Ibid., 1627.

dealt with written ones. In any case, they rejected measures that would distinguish names from one another at the cost of rendering all of them incomprehensible.

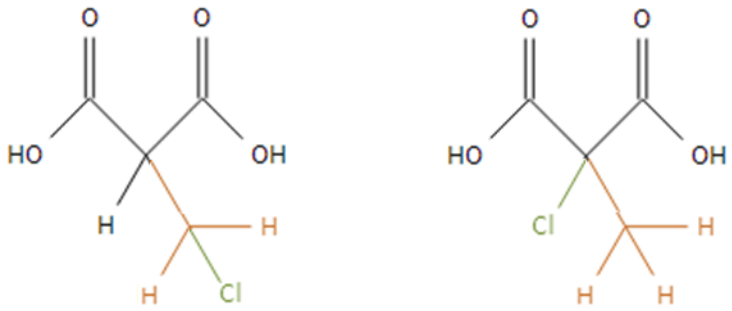


Figure 7. According to Patterson and Curran's index nomenclature, the name of the compound represented by these two structural formula diagrams were, at left, "malonic acid, (chlormethyl-)," and at right, "malonlic acid, chlormethyl-." The parentheses indicated that the (green) chloride was a substituent of (orange) methyl; the absence of parentheses indicates that both chloride and methyl groups were substituents of (black) malonic acid. (Per convention, unlabeled vertices of structural formulas represent carbon atoms.)

Patterson and Curran prioritized the simplicity and accessibility of the index for its users, not for its compilers.[135] This sometimes meant that their rules of nomenclature were neither simple nor consistent. In certain cases, such as in their procedure for precisely determining what counted as the preferred "smallest" numbering of a compound's atoms, they resorted to logically intricate nests of rules within rules. "These may seem very fine points," they wrote, "yet it is of some importance to have definite rules for numbering a compound so that index entries may come absolutely together."[136]

[135] A decade and a half earlier, Noyes had criticized Richter for requesting, in the introduction to the second edition of his *Lexikon*, that authors facilitate the process of compiling formula indexes like Richter's by including empirical formulas for all of the compounds mentioned in their articles. Noyes wrote: "In the opinion of the writer it would be a misfortune if this request should lead to the use of fewer structural formulas. The needs and advantage of the average reader are of greater importance than the time of the lexicographer." William A. Noyes, "Lexikon der Kohlenstoff Verbindungen," *Journal of the American Chemical Society* 22, no. 4 (1900): 224.

[136] Patterson and Curran, "A System of Organic Nomenclature," 1628.

More often, however, Patterson and Curran dealt with problem areas not by multiplying rules but by tolerating inconsistency. They created an entirely new class of nomenclature, "additive names," to deal with situations in which different principles for determining what part of a molecule should count as root and as substituent came into conflict. Thus instead of selecting *naphtylacetic acid* (which violated their principle that the largest portion of the molecule make up the index compound) or *carboxymethylnaphthalene* (which violated their principle of expressing the chief function of the molecule in the main part of the name, not a prefix), they settled upon simple combination of the index names of the two sections of the molecule, forming *Naphthaleneacetic acid* (*Fig. 8*). Patterson and Curran's description of their device for determining the chief function of a compound applied just as well to their nomenclature in general: "a fixed order (which, however, need not be applied slavishly)."[137]

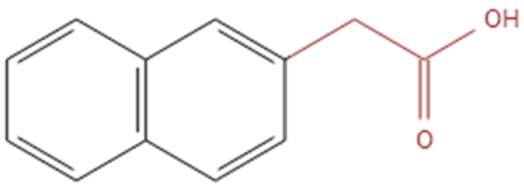


Figure 8. Instead of deciding upon a rule to select either naphthalene (black) or acetic acid (red) as the root of the name for the compound represented by this structural formula and indicating the other portion with a prefix, Patterson and Curran elected to make an exception, simply combining the two index names to form *Naphthaleneacetic acid*.

To make an index that would be useful and durable, Patterson and Curran needed rules of nomenclature. When rules led to absurdities, their predecessors had either justified the absurdities as a small price to pay for a consistent set of rules (the choice of the Geneva Congress) or cited them as reason to set aside rule-bound names entirely in favor of an alternative means of ordering an index (Jacobson, Stelzner, and Richter's approach). Patterson

[137] Ibid., 1625–26.

and Curran followed a third way. They came up with a system of unique index names, but not by the application of rules whose very rigor excluded existing names and confined the new names' use to indexes alone. Rather, they generated unique names by embracing existing usage, cutting through Gordian knots of chemical logic wherever possible through devices like inverted names and the alphabetical ordering of prefixes, and setting aside their own rules when necessary to conserve clarity and common sense. They made unique index nomenclature acceptable by means of exceptions.

Applying Patterson and Curran's nomenclature to ten years' worth of abstracts was no easy task. "We expected this index to be completed sooner," Crane admitted in his annual report to the ACS council at the beginning of 1919. However, the new nomenclature and indexing standards had required entirely re-indexing the first nine volumes of abstracts, and the *Chemical Abstracts* staff had to do this while also compiling their usual bimonthly numbers and annual indexes. As a result, the editors were only then "emerging from burial under index cards."[138] They took the lessons of the revision process to heart: "Preparations for a second collective index are now being made during each year so that it will not be necessary to start at the beginning in compiling such an index."[139] The last of the four volumes of the collective index was finally published in October 1919. It was this index that made it possible for Crane and Patterson to assert that for "searches which do not need to go back further than 1907 *Chemical Abstracts* is regarded as the best starting point. For many purposes it is the only abstract journal which one needs to use."[140]

[138] *Proceedings of the American Chemical Society* (1919): 19

[139] *Proceedings of the American Chemical Society* (1920): 22.

[140] Crane and Patterson, *A Guide to the Literature of Chemistry*, 89.

Conclusion

The 1890s through about 1920 was a pivotal period for national chemical societies. The Deutsche Chemische Gesellschaft sought to organize the nation's productive but fragmented chemical enterprises. The American Chemical Society sought to create a national chemical discipline, both through bringing more domestic and foreign attention to chemistry done in America and by making chemical research from around the world more accessible to American chemists. As Chapters 5 and 6 will discuss, the organization of chemical research communities along national lines established the conditions for the organization of chemical research and production during World War I, and for the formation of international associations of these national societies before and after the "Chemists' War."

Such stories are usually cast in opposition to accounts of contemporaneous utopian, cosmopolitan efforts to order the world's knowledge. As this chapter has shown, however, for the Deutsche Chemische Gesellschaft and the American Chemical Society at the turn of the twentieth century, information universalism and chemical nationalism were of a piece. Both Societies invested unprecedented resources in constructing sweeping reference enterprises. In the process, they invented a new chemical profession: the editor of chemical reference texts. Though drawing upon existing texts and models, these abstract journals, handbooks, and lexica were not simply updated editions but new processes, aimed at integrating various parts of chemical literature into reference systems. Nomenclature and notation were the crucial elements that could either make these reference systems sustainable and useful or render them illegible and obsolete.

This was not always the case. The chemical dictionaries of the mid-nineteenth century were compilations of current theory and practice, their articles bound to age into obsolescence in the alphabetical order in which they were published, to be completed within a decade or two

and replaced (or ignored) soon thereafter. They were synchronic cross-sections of an ever-changing discipline.

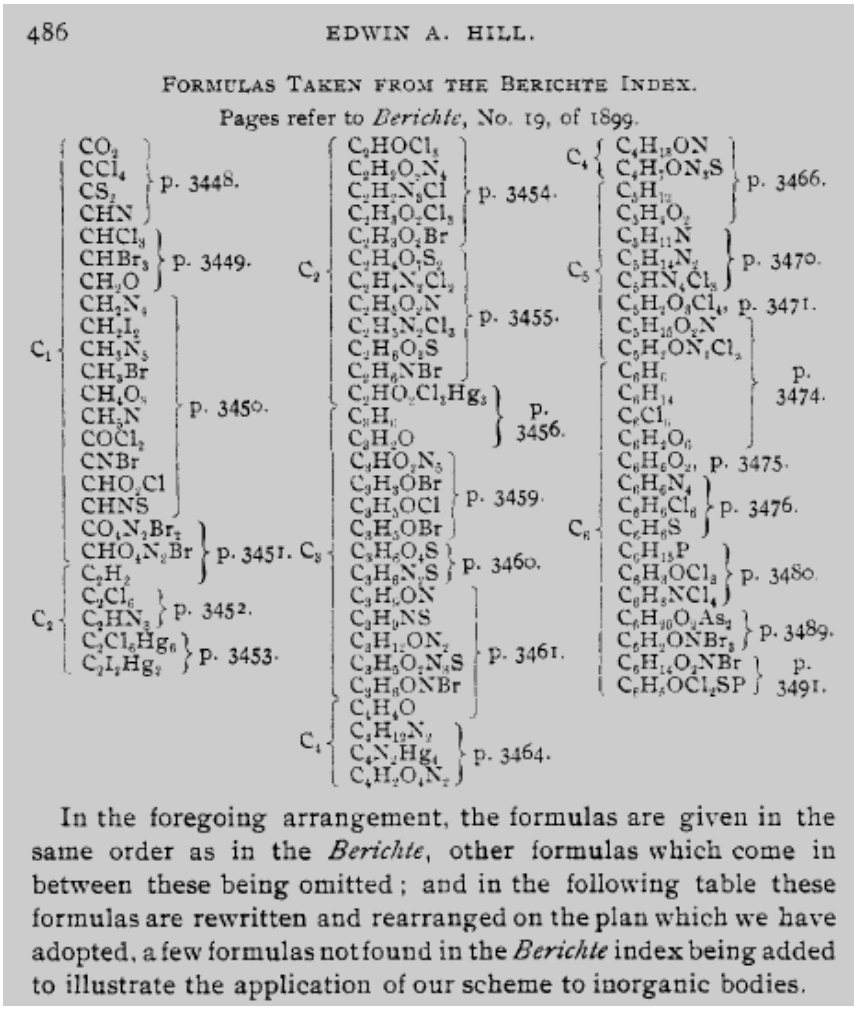
The chemical reference works discussed in this chapter entailed a different kind of historical consciousness on the part of the chemists who created and used them. These texts divided the field of organic chemistry according to its objects of inquiry, comprising tens of thousands of discrete, continually growing repositories for all that chemists had learned about each of these substances, regardless of when they'd learned it. Reference works like Beilstein's *Handbuch* did not make single broad slices across history, but thousands of longitudinal cuts through it; recent findings did not replace past results, but sat alongside them, accessible to chemists in the present and the future.[141]

The ambition of assimilating the past rather than overturning it, in turn, entailed a special importance for names and formulas. The system of nomenclature or notation applied in one of these indexes was not just a means of identifying compounds, but the scaffold in which past results were preserved and on which future results would accrete. It had to be built with care. Chemistry would continue to change, but this organizing mode of representation could not. As these reference works grew, their editors could hardly afford to reshuffle facts that had already been compiled or compounds that had already been put in order. The stability of nomenclature and notation was a guarantee to both compiler and user that the index would remain relevant, useful, and up to date, and that it could therefore be safely incorporated into

[141] In this respect, although chemistry is the laboratory science par excellence, it surely also belongs among the disciplines that Lorraine Daston has described as the "sciences of the archive." Daston's descriptions of the "collective empiricism" of such sciences as "an intrinsically collective undertaking—and one that extends into both past and future" and of its results as "data strewn across the flat expanse of time" capture the view of chemistry presented in the reference works described in this chapter; Daston, "The Sciences of the Archive."

the everyday practice of chemists. The manner in which a reference work dealt with the past determined whether it had a future.

Important as these reference enterprises were, however, they were far from the only setting in which chemists dealt with names and notation during this period. In fact, from the 1890s through the 1910s, the division carved out at Geneva between the nomenclature of reference works and everyday chemical nomenclature widened, and experiments with rational nomenclature proliferated in the latter field as well as the former. Before turning to the future of the reference enterprises described in this chapter, and to organic chemical nomenclature in international war and peace, we will explore practices of nomenclature in everyday chemical research and teaching.



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FORMULAS TAKEN FROM THE BERICHTE INDEX.

Pages refer to *Berichte*, No. 19, of 1899.

C_1: CO_2, CCl_4, CS_2, CHN — p. 3448.
$CHCl_3$, $CHBr_3$, CH_2O — p. 3449.
CH_2N_2, CH_2I_2, CH_3N_3, CH_3Br, CH_4O_3, CH_5N, $COCl_2$, $CNBr$, CHO_2Cl, $CHNS$ — p. 3450.
$CO_4N_2Br_2$, CHO_4N_2Br — p. 3451.

C_2: C_2H_2 — p. 3452.
C_2Cl_6, C_2HN_3 — p. 3452.
$C_2Cl_6Hg_6$, $C_2I_2Hg_2$ — p. 3453.

C_2: C_2HOCl_5, $C_2H_3O_2N_4$, $C_2H_2N_3Cl$, $C_2H_3O_2Cl_3$ — p. 3454.
$C_2H_3O_2Br$, $C_2H_4O_2S_2$, $C_2H_4N_2Cl_2$ — p. 3455.
$C_2H_5O_2N$, $C_2H_3N_2Cl_3$, $C_2H_6O_2S$, C_2H_6NBr — p. 3455.
$C_2HO_2Cl_3Hg_3$, C_2H_6, C_2H_2O — p. 3456.

C_3: $C_3HO_2N_5$, C_3H_3OBr, C_3H_3OCl, C_3H_3OBr — p. 3459.
$C_3H_6O_4S$, $C_3H_8N_2S$ — p. 3460.
C_3H_5ON, C_3H_9NS, $C_3H_{12}ON_2$, $C_3H_5O_2N_3S$, C_3H_8ONBr — p. 3461.

C_4: $C_4H_2O_4$, $C_4H_2N_4$, $C_4H_4O_3$, $C_4H_4N_2S$, $C_4H_4O_2Cl_2$, $C_4H_6N_2Cl_2$, $C_4H_6O_2S$, $C_4H_8N_2$, $C_4H_8O_2$, $C_4H_{10}N_2$ — p. 3464.

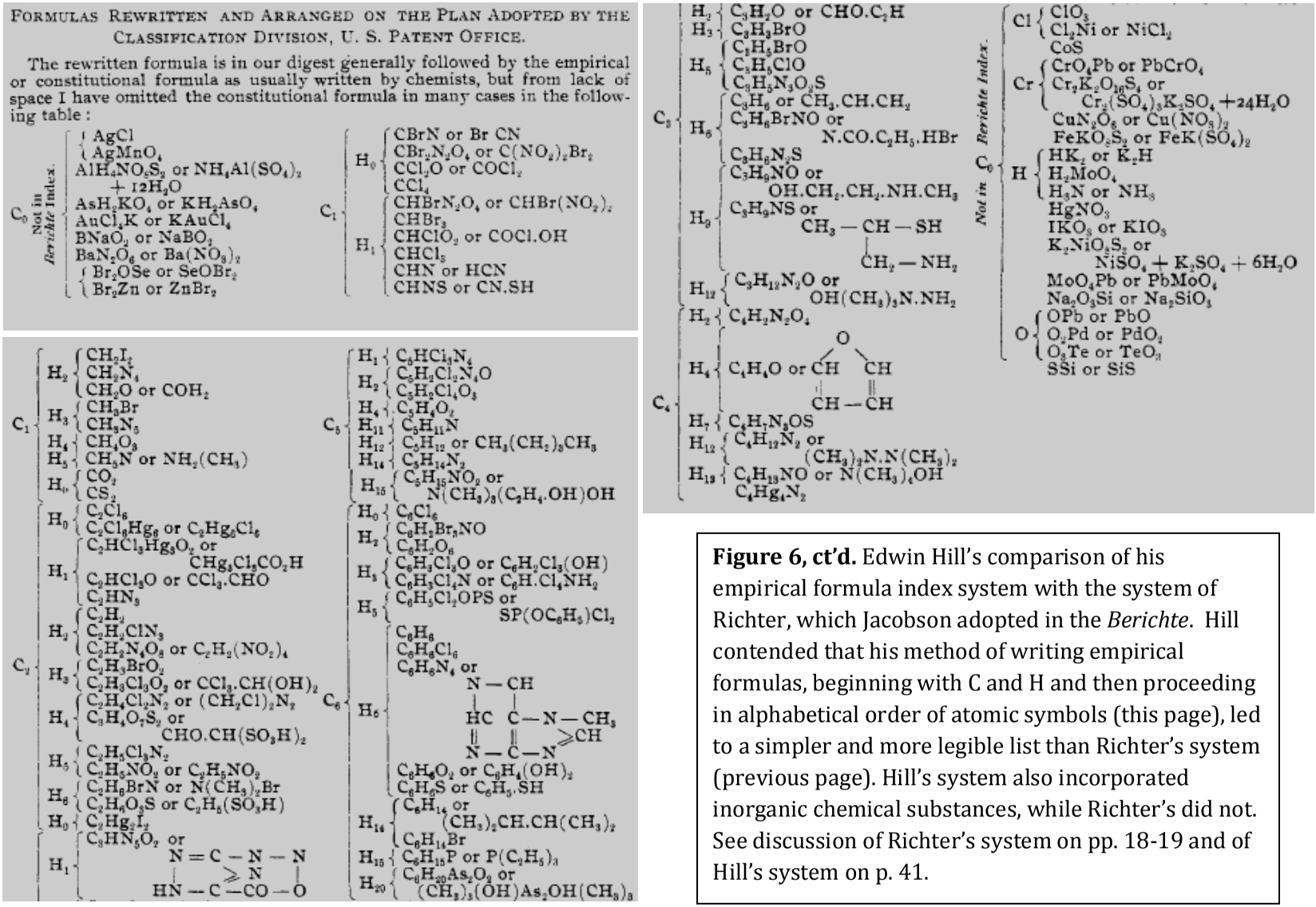
C_4: $C_4H_{13}ON$, $C_4H_7ON_3S$ — p. 3466.
C_5H_{12}, $C_5H_4O_2$ — p. 3466.
$C_5H_{11}N$, $C_5H_{14}N_2$, $C_5HN_4Cl_3$ — p. 3470.
$C_5H_2O_3Cl_4$ — p. 3471.
$C_5H_{10}O_2N$, $C_5H_7ON_3Cl_2$ — p. 3471.

C_6: C_6H_6, C_6H_{14}, C_6Cl_6, $C_6H_2O_6$ — p. 3474.
$C_6H_6O_{21}$ — p. 3475.
$C_6H_6N_4$, $C_6H_6Cl_6$ — p. 3476.
C_6H_8S — p. 3476.
$C_6H_{15}P$, $C_6H_3OCl_3$ — p. 3480.
$C_6H_8NCl_4$ — p. 3480.
$C_6H_{10}O_4As_2$, $C_6H_7ONBr_3$ — p. 3489.
$C_6H_{14}O_2NBr$, $C_6H_5OCl_2SP$ — p. 3491.

In the foregoing arrangement, the formulas are given in the same order as in the *Berichte*, other formulas which come in between these being omitted; and in the following table these formulas are rewritten and rearranged on the plan which we have adopted, a few formulas not found in the *Berichte* index being added to illustrate the application of our scheme to inorganic bodies.

Figure 6. Edwin Hill's comparison of his empirical formula index system with the system of Richter, which Jacobson adopted in the *Berichte*. Hill contended that his method of writing empirical formulas, beginning with C and H and then proceeding in alphabetical order of atomic symbols (next page), led to a simpler and more legible list than Richter's system (above). Hill's system also incorporated inorganic chemical substances, while Richter's did not. See discussion of Richter's system on pp. 18-19 and of Hill's system on p. 41.[142]

[142] Hill, "On a System of Indexing Chemical Literature," 486–487.



FORMULAS REWRITTEN AND ARRANGED ON THE PLAN ADOPTED BY THE CLASSIFICATION DIVISION, U. S. PATENT OFFICE.

The rewritten formula is in our digest generally followed by the empirical or constitutional formula as usually written by chemists, but from lack of space I have omitted the constitutional formula in many cases in the following table :

C_0 Not in *Berichte* Index.

- AgCl
- $AgMnO_4$
- $AlH_4NO_5S_2$ or $NH_4Al(SO_4)_2 + 12H_2O$
- AsH_2KO_4 or KH_2AsO_4
- $AuCl_4K$ or $KAuCl_4$
- $BNaO_2$ or $NaBO_2$
- BaN_2O_6 or $Ba(NO_3)_2$
- Br_2OSe or $SeOBr_2$
- Br_2Zn or $ZnBr_2$

C_1

- H_0: CBrN or Br CN; $CBr_2N_2O_4$ or $C(NO_2)_2Br_2$; CCl_2O or $COCl_2$; CCl_4
- H_1: $CHBrN_2O_4$ or $CHBr(NO_2)_2$; $CHBr_3$; $CHClO_2$ or COCl.OH; $CHCl_3$; CHN or HCN; CHNS or CN.SH

C_1

- H_2: CH_2I_2; CH_2N_4; CH_2O or COH_2
- H_3: CH_3Br; CH_3N_3
- H_4: CH_4O_3
- H_5: CH_5N or $NH_2(CH_3)$
- H_6: CO_2; CS_2

C_2

- H_0: C_2Cl_6; $C_2Cl_8Hg_6$ or $C_2Hg_6Cl_6$
- H_1: $C_2HCl_3Hg_3O_2$ or $CHg_3Cl_3CO_2H$; C_2HCl_3O or $CCl_3.CHO$; C_2HN_3
- H_2: C_2H_2; $C_2H_2ClN_3$; $C_2H_2N_4O_8$ or $C_2H_2(NO_2)_4$
- H_3: $C_2H_3BrO_2$; $C_2H_3Cl_3O_2$ or $CCl_3.CH(OH)_2$
- H_4: $C_2H_4Cl_2N_2$ or $(CH_2Cl)_2N_2$; $C_2H_4O_7S_2$ or $CHO.CH(SO_3H)_2$
- H_5: $C_2H_5Cl_3N_2$; $C_2H_5NO_2$ or $C_2H_5NO_2$
- H_6: C_2H_6BrN or $N(CH_3)_2Br$; $C_2H_6O_3S$ or $C_2H_5(SO_3H)$
- H_8: $C_2Hg_2I_2$
- H_1: $C_3HN_5O_2$ or

C_3

- H_1: $C_3HCl_3N_4$
- H_2: $C_3H_2Cl_2N_4O$; $C_3H_2Cl_4O_3$
- H_4: $C_3H_4O_3$
- H_{11}: $C_3H_{11}N$
- H_{12}: C_3H_{12} or $CH_3(CH_2)_3CH_3$
- H_{14}: $C_3H_{14}N_2$
- H_{15}: $C_3H_{15}NO_2$ or $N(CH_3)_3(C_2H_4.OH)OH$

C_6

- H_0: C_6Cl_6
- H_2: $C_6H_2Br_2NO$; $C_6H_2O_6$
- H_3: $C_6H_3Cl_3O$ or $C_6H_2Cl_3(OH)$; $C_6H_3Cl_4N$ or $C_6H.Cl_4NH_2$
- H_5: $C_6H_5Cl_2OPS$ or $SP(OC_6H_5)Cl_2$
- H_6: C_6H_6; $C_6H_6Cl_6$; $C_6H_6N_4$ or

$C_6H_6O_2$ or $C_6H_4(OH)_2$; C_6H_6S or $C_6H_5.SH$

- H_{14}: C_6H_{14} or $(CH_3)_2CH.CH(CH_3)_2$; $C_6H_{13}Br$
- H_{15}: $C_6H_{15}P$ or $P(C_2H_5)_3$
- H_{20}: $C_6H_{20}As_2O_2$ or $(CH_3)_3(OH)As_2OH(CH_3)_3$

C_3

- H_2: C_3H_2O or $CHO.C_2H$
- H_3: C_3H_3BrO
- H_5: C_3H_5BrO; C_3H_5ClO; $C_3H_5N_3O_3S$
- H_6: C_3H_6 or $CH_3.CH.CH_2$; C_3H_6BrNO or $N.CO.C_2H_3.HBr$; $C_3H_6N_2S$
- H_9: C_3H_9NO or $OH.CH_2.CH_2.NH.CH_3$; C_3H_9NS or

- H_{12}: $C_3H_{12}N_2O$ or $OH(CH_3)_3N.NH_2$

C_4

- H_2: $C_4H_2N_2O_4$
- H_4: C_4H_4O or

- H_7: $C_4H_7N_3OS$
- H_{12}: $C_4H_{12}N_2$ or $(CH_3)_2N.N(CH_3)_2$
- H_{13}: $C_4H_{13}NO$ or $N(CH_3)_4OH$; $C_4Hg_4N_2$

Not in Berichte Index.

- Cl: ClO_3; Cl_2Ni or $NiCl_2$; CoS
- Cr: CrO_4Pb or $PbCrO_4$; $Cr_2K_2O_{16}S_4$ or $Cr_2(SO_4)_3K_2SO_4 + 24H_2O$; CuN_2O_6 or $Cu(NO_3)_2$; $FeKO_2S_2$ or $FeK(SO_4)_2$
- H: HK_2 or K_2H; H_2MoO_4; H_3N or NH_3; $HgNO_3$; IKO_3 or KIO_3; $K_2NiO_2S_2$ or $NiSO_4 + K_2SO_4 + 6H_2O$; MoO_4Pb or $PbMoO_4$; Na_2O_3Si or Na_2SiO_3
- O: OPb or PbO; O_2Pd or PdO_2; O_2Te or TeO_2; SSi or SiS

Figure 6, ct'd. Edwin Hill's comparison of his empirical formula index system with the system of Richter, which Jacobson adopted in the *Berichte*. Hill contended that his method of writing empirical formulas, beginning with C and H and then proceeding in alphabetical order of atomic symbols (this page), led to a simpler and more legible list than Richter's system (previous page). Hill's system also incorporated inorganic chemical substances, while Richter's did not. See discussion of Richter's system on pp. 18-19 and of Hill's system on p. 41.

Chapter 4

Systematic Names and Chemical Pedagogy, Patents, and Products

"The knowledge of the language of a science is an important part of the study of that science," Amé Pictet told a class of aspiring chemists at the beginning of the winter term of 1895-96. "In chemistry, in particular," he went on, "it is indispensable to know the rules according to which names have been formed and which it is necessary to follow to name the new compounds that we synthesize."[1] As a newly-minted assistant professor of organic chemistry at the University of Geneva, the former Geneva Congress secretary was much occupied with teaching. In Pictet's view, and in the view of many others at the turn of the twentieth century, rules of chemical nomenclature had to be brought into contact with chemical instruction and research.

The Geneva Congress had not devoted much time to considering what chemists might do with systematic chemical names in such settings. On the contrary, as detailed in Chapter 2, the Congress delegates crafted their resolutions as a method of nomenclature exclusively for use in ordering chemical indexes. Chapter 3 showed one way in which the development of systematic nomenclature diverged from this intention. Editors found the form of the Geneva rules – the algorithmic transformation of a structural formula into a unique name, expressing chemical identity and relationships according to certain patterns in chemical diagrams – ill-suited to the delegates' stated purpose. In the process of compiling prodigious chemical reference texts, these editors reworked the Geneva rules and developed alternative approaches to naming and ordering chemical compounds. In pursuit of the same end that the Geneva

[1] Amé Pictet, "Nomenclature et terminologie chimiques (hiver 1895-96)," BGE, Ms. Fr. 3407 (Cours divers professés à l'Université), 201.

Congress had articulated as the object of its rules for naming – putting chemical indexes in order – chemical editors established different systems of nomenclature and notation.

Turning from editorial offices to other sites of chemistry, this chapter addresses the development of systematic nomenclature in an inverse direction.[2] Numerous chemists engaged in tasks for which the Geneva rules were explicitly *not* intended nevertheless took up this form of nomenclature for their own purposes. Such repurposing of the Geneva nomenclature was not unanticipated. After all, Charles Friedel had launched the nomenclature reform effort that led to the Geneva Congress with chemical pedagogy in mind; delegates promoting the accomplishments of the Congress expressed confidence that the new naming conventions would take hold throughout chemistry. Still, such uses put Geneva names into contexts whose requirements – euphony and pronounceability, for example – the Congress had chosen to ignore. The Geneva nomenclature was no better a fit for chemical practice outside the pages of reference works than editors had found it to be within them. However, chemists using Geneva names in such settings dealt with the mismatch between systematic nomenclature and their own purposes in a different manner than editors did – even when these chemists and those editors were one and the same.

Chemists applying the Geneva rules outside of reference works took the incompatibility of systematic nomenclature with their particular needs as an occasion to reflect on how their accustomed forms of chemical representation worked, how they related to Geneva-style names, and how each approach did or did not serve the chemist's purposes. Whereas editors sought to close the gap between systematic nomenclature and their needs, chemists who applied Geneva names in other settings explored this gap. Often, as some of the Geneva delegates had

[2] On "sites of chemistry," see John Perkins, ed., "Sites of Chemistry in the Eighteenth Century," special issue of *Ambix* 60, no. 2 (2013): 95–178; Antonio Garcia-Belmar and John Perkins, ed. "Sites of Chemistry in the Nineteenth Century," special issue of *Ambix* 61, no. 2 (2014): 109–186; Antonio Garcia-Belmar and John Perkins, ed., "Sites of Chemistry in the Twentieth Century," special issue of *Ambix* 62, no. 2 (2015): 109–188.

anticipated, this process led to a gradual convergence of chemical names used in teaching, research, and commerce with the principles of the Geneva rules. Elsewhere, however, new forms of chemical representation and new ways of using chemical representations arose in the gap between how chemical identity had been understood in a particular setting and how it was represented in a systematic name.

These developments were especially pronounced in three areas: chemical pedagogy, the patent protection of molecular inventions, and research and commerce involving pharmaceutical products. In part, this reflected the central importance of teaching, patents, and pharmaceuticals in chemical science and industry at the turn of the twentieth century. These were fields in which chemical identity and relationships took on particularly high stakes. Some of the authors of the most widely-read chemical textbooks were intimately familiar with systematic nomenclature, either as editors of reference works or former Geneva Congress delegates; others became engaged in nomenclature debates through textbook writing. In the classroom and laboratory, the boardroom and patent office, bringing Geneva names into a new setting also brought into existence a space between systematic names and the ways in which chemical representation and identity had typically, often implicitly, been addressed. In each of these areas – pedagogy, patents, and pharmaceuticals – chemists exploring this space brought about an incremental spread of Geneva-like names throughout chemistry; they also brought into existence entirely new forms of chemical representation and new ways to do things with chemical names.

Pedagogy

Just as the "knowledge of the language of a science is an important part of the study of that science," as Pictet told his students, the language of scientific instruction can also shape scientific knowledge. Recent scholarship on the history of scientific pedagogy has illuminated

numerous ways in which tools and methods for teaching science have shaped the practice of science, for students and instructors alike.[3] Much of this work has focused on the significance of names, formulas, tables, and diagrams for scientific instruction, and, reciprocally, on the role of pedagogical circumstances in the development of approaches to scientific representation and classification.

Textbooks have played a crucial role in entwining pedagogy and the practice of modern science.[4] Antoine Lavoisier's new chemistry spread throughout Europe and the US at the turn of the 19th century by means of his *Traité Elementaire de Chimie* and its many translations.[5] Dmitri Mendeleev originally developed his periodic system of the elements as a means of organizing a textbook.[6] Friedrich Beilstein's *Handbuch der Organischen Chemie* began as a textbook, too.[7] Charles Friedel and his atomist colleagues used textbooks to publicize research in structural organic chemistry that they could not get past the anti-atomist gatekeepers of the French chemical periodicals.[8] In the early-to-mid twentieth century, Linus Pauling and Charles Coulson introduced distinctive forms of diagrammatic representation in their respective textbooks *The Nature of the Chemical Bond* and *Valence*. Pauling's book and its diagrams played

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[3] David Kaiser, ed., *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives* (Cambridge, MA: MIT Press, 2005); David Kaiser, *Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics* (Chicago: University of Chicago Press, 2005); Andrew Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (Chicago: University of Chicago Press, 2003); Kathryn M. Olesko, *Physics as a Calling: Discipline and Practice in the Koenigsberg Seminar for Physics* (Ithaca, NY: Cornell University Press, 1991); Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life* (Chicago: University of Chicago Press, 1994).

[4] Anders Lundgren and Bernadette Bensaude-Vincent, ed., *Communicating Chemistry: Textbooks and Their Audiences, 1789-1939* (Canton, MA: Science History Publications, 2000); Marga Vicedo, "Introduction: The Secret Lives of Textbooks," *Isis* 103, no. 1 (2012): 83–87.

[5] Maurice P. Crosland, *Historical Studies in the Language of Chemistry* (New York: Dover, 1978), 184–214; Bernadette Bensaude-Vincent and F. Abbri, eds., *Lavoisier in European Context: Negotiating a New Language for Chemistry* (Canton, MA: Science History Publications, 1995).

[6] Michael D. Gordin, "Translating Textbooks: Russian, German, and the Language of Chemistry," *Isis* 103, no. 1 (2012): 88–98; Michael D. Gordin, *A Well-Ordered Thing: Dmitrii Mendeleev and the Shadow of the Periodic Table* (New York: Basic Books, 2004), 15–48.

[7] Michael D. Gordin, "Beilstein Unbound: The Pedagogical Unraveling of a Man and His Handbuch," in Kaiser, *Pedagogy and the Practice of Science*, 11–39.

[8] José Ramon Bertomeu-Sánchez, Antonio Garcia-Belmar, and Bernadette Bensaude-Vincent, "Looking for an Order of Things: Textbooks and Chemical Classifications in Nineteenth Century France," *Ambix* 49, no. 3 (2002): 227–250.

a key role in introducing quantum mechanics into chemistry, as did Coulson's in encouraging chemists to embrace the molecular orbital interpretation of quantum chemistry.[9]

Such illuminating stories of the alignment of pedagogy and scientific practice suggest that their *mis*alignment merits explanation as well. Such was the case with the Geneva nomenclature and textbooks of organic chemistry published in the decades following the Geneva Congress. Textbooks and courses engaged with systematic nomenclature in different ways. Some presented the Geneva rules just as the Congress had codified them; some taught these rules as one method of chemical naming among many. Some offered instruction in the various methods of organization actually used in reference works. Other authors and instructors did not teach systematic nomenclature so much as teach *with* nomenclature, sometimes by means of the Geneva rules, often in defiance of them. In at least one case, an instructor used nomenclature as a means for instilling in students a sensitivity to the ongoing historical development of chemical theory.

Teaching nomenclature rules

In 1892, as the delegates to Friedel's Congress gathered in Geneva, Paul Jacobson was not yet an empire-building editor but an assistant professor of chemistry at the University of Heidelberg. With his mentor and Heidelberg superior Victor Meyer, Jacobson was busy completing the first volume of their *Lehrbuch der Organischen Chemie*. Meyer and Jacobson's textbook was a prodigious work – its first edition, published in two volumes between 1893 and 1902, ran to more than 2,000 pages – and it was an influential as it was massive. Numerous other textbooks in various languages singled out Meyer and Jacobson as an authoritative

[9] Buhm Soon Park, "In the Context of Pedagogy: Teaching Strategy and Theory Change in Quantum Chemistry," in Kaiser, *Pedagogy and the Practice of Science*, 287–322; Ana Simões, "A Quantum Chemical Dialogue Mediated by Textbooks: Pauling's The Nature of the Chemical Bond and Coulson's Valence," *Notes and Records of the Royal Society* 62, no. 3 (2008): 259–269; Kostas Gavroglou and Ana Simões, *Neither Physics nor Chemistry: A History of Quantum Chemistry* (Cambridge, MA: MIT Press, 2012).

source.[10] "If what I am interested in is a type of compound rather than any single one, I naturally turn to Beilstein, but often I have found Meyer-Jacobson…more satisfactory," wrote the anonymous chemical reader quoted at the beginning of Chapter 3.[11] The text was one of the first and most prominent textbooks to make systematic use of theories of stereochemistry (a term coined by Meyer), that is, the physical arrangement of a molecule's atoms in space.[12] Meyer and Jacobson also took care to provide as much detail as possible regarding "the compounds which have taken on importance in practical life," that is, in the chemical industry.[13]

The Heidelberg chemists presented the full text of the Geneva resolutions in an appendix to the first volume of the text, published in 1893.[14] They had not enforced the rules in the text, they explained in their introduction, "since a great part of our book was already published before the Geneva Congress, and because the rules agreed upon so far are far from sufficient for the naming of all compounds anyhow."[15] Instead, they had formed names "according to the principles that have gradually accumulated in the course of the development of our science." Marrying Friedel's emphasis on the usefulness of flexible nomenclature in pedagogical settings and Adolf von Baeyer's quietism regarding the nomenclature of chemical instruction, Meyer and Jacobson explained that they selected from a "diversity of possible names… one or another name as particularly suitable for an explanation." Nevertheless, they had included the text of the Geneva rules, in anticipation of their prospective use in indexes and

[10] William Noyes, for example, referred to Meyer and Jacobson as "one of the best of the larger text-books." William A. Noyes, *A Text Book of Organic Chemistry*, 2nd ed. (New York: Henry Holt, 1909), 6.

[11] Evan Jay Crane and Austin M. Patterson, *A Guide to the Literature of Chemistry* (New York: Wiley, 1927), 223.

[12] Peter J. Ramberg, *Chemical Structure, Spatial Arrangement: The Early History of Stereochemistry, 1874-1914* (Burlington, VT: Ashgate, 2003), 157.

[13] Victor Meyer and Paul Jacobson, *Lehrbuch der organischen Chemie*, 1st ed., vol. 1 (Leipzig: Veit, 1893), v.

[14] Ibid., 1091–1097. Friedel had invited Meyer to the Geneva Congress, but Meyer had not attended. "Congrès de Nomenclature Chimique," *Bulletin de la Société Chimique de Paris* 7 (1892): xxiv.

[15] Meyer and Jacobson, *Lehrbuch der organischen Chemie*, 1st ed., vol. 1, vi.

because they were "likely to find a widespread application and gradually to supplant the older naming principles in spoken nomenclature as well."[16]

The French chemist and Geneva delegate Auguste Béhal penned a French analog to Meyer and Jacobson's work, his *Traité de chimie organique, d'après les théories modernes*, published in two volumes in 1896-97. Earlier, in his summary of the Geneva Congress for a French scientific periodical, Béhal had made it clear that he and his fellow members of Friedel's subcommission didn't intend to let the preoccupation with index-nomenclature get in the way of their plans to reform the language of French chemical instruction. "Not only did we arrive at a complete agreement on the names designed for registers, which will facilitate bibliographic research," he wrote, but also, "entered into practice in education, the new nomenclature will singularly facilitate the task of beginners."[17]

Béhal accordingly used names formed according to the Geneva rules throughout his textbook. He included a brief section summarizing the algorithmic procedure for forming chemical names in strict accordance with the Geneva rules, acknowledging that "the names are not always euphonic, but, on the other hand, they indicate the entire constitution of the compound."[18] However, Béhal did not constrain himself to teaching the rules formalized at Geneva. As was typical, the textbook was organized according to different classes of organic compounds. In most of these sections, Béhal discussed various approaches to naming members of each chemical family. He often extrapolated the procedures of the Geneva nomenclature

[16] Ibid., 1091.

[17] Auguste Béhal, "La nomenclature chimique au congrès international de Genève," *Moniteur scientifique* 39, no. 606 (June 1892): 416–417.

[18] Auguste Béhal, *Traité de chimie organique, d'après les théories modernes,* 1st ed., vol. 1 (Paris: O. Doin, 1896), 192–193.

beyond the rules that the Congress had formalized, and he sometimes discussed the advantages of alternative naming procedures, such as the "carbinol" nomenclature for alcohols.[19]

One striking but little-read textbook was dedicated entirely to teaching chemists how to apply a version of the Geneva rules extended to address all organic compounds. From his relatively isolated post in Bucharest, the Romanian Geneva delegate (and former Friedel student) Constantin Istrati took it upon himself to construct a comprehensive system of nomenclature upon the foundation of the Geneva rules. Istrati, who had also written a textbook that followed the Geneva rules in thoroughgoing fashion (about which more below), did so with the conviction that one could not order organic compounds without effectively naming them, or vice versa: "*Both nomenclature and classification complement each other and cannot be conceived nor understood separately. They are different ways of uniting and grouping the same bodies based on a unique character, that is to say on the mode of functioning of these bodies.*"[20]

Istrati spent almost two decades assembling his key to all terminology, aided by a junior colleague at the University of Bucharest. The resulting work laid out an extraordinarily detailed system of nomenclature, building upon the Geneva rules but introducing numerous procedures for naming more complex classes of compounds that the Congress had not addressed. The book was developed without the input of other chemists or chemical editors, weighed in at 1200 pages, and was in Romanian. Just before its publication in 1913, Istrati intimated that a French translation was in the works, but the onset of World War I scotched any such plans. "One could

[19] Ibid., 269. As an example of Béhal's extension of the Geneva nomenclature, see, e.g., his elaboration of the Geneva Congress' treatment of hydrocarbon rings; Auguste Béhal, *Traité de chimie organique, d'après les théories modernes,* 1st ed., vol 2., (Paris: O. Doin, 1897), 488–612.

[20] Constantin Istrati, "Études sur la nomenclature de la chimie organique. Additions, compléments, propositions," *Buletinul Societăţii de Ştiinţe Fizice din Bucureşti-România* 5 (1896): 141–234 (quotation on 144), quoted in Pieter Eduard Verkade, *A History of the Nomenclature of Organic Chemistry*, trans. S. G. Davies (Boston: Reidel, 1985), 325 (Istrati's italics; Verkade's translation).

say without exaggeration that it was almost immediately forgotten," wrote the future IUPAC nomenclature committee chairman P.E. Verkade.[21]

Teaching nomenclature as practice

As the 1890s wore on, the Geneva rules did not become the standard rubric for index-nomenclature that the Congress had intended them to be. However, more and more names formed at least approximately according to the Geneva rules made their way into the literature. In keeping with these trends, chemical authors stopped including the text of the Geneva resolutions in their textbooks, but at the same time they included more and more Geneva-style names. Perhaps as a result of the heightened attention to systematic nomenclature that the use of such names encouraged, some textbook authors gave increasing space to descriptions of nomenclature as practice. These authors explicitly discussed the Geneva nomenclature alongside other commonly-used approaches to chemical naming without presenting any particular method as preferred.

In the second edition of his and his late mentor's textbook, for example, Jacobson naturalized the Geneva names. Whereas the second volume of the first edition had singled out "official" Geneva names with italics, the 1907 first volume of the second edition listed many more such names but did not italicize them, instead presenting them in the same typeface as other sorts of rational and trivial names.[22] Jacobson also advocated the use of semi-systematic or "half-rational" names – names that identified a compound in relation to a familiar close chemical relative, using the trivial name of the latter as a root and applying systematic prefixes and suffixes to express the structural differences of the former. Such an approach was

[21] Verkade inherited his own copy of Istrati's enormous textbook from Arnold Holleman. In decades as a textbook author and an international nomenclature commission president, Holleman had never troubled even to cut open the book's pages; Verkade, *A History*, 320.

[22] Victor Meyer and Paul Jacobson, *Lehrbuch der organischen Chemie*, 2nd ed., vol. 1 (Leipzig: Veit, 1907).

"expedient," Jacobson wrote, "since it facilitates the formation of shorter names, whereas a strictly rational nomenclature necessarily leads to very cumbersome names for complex compounds."[23]

Dutch chemist Arnold Holleman's *Textbook of Organic Chemistry*, one of the most popular organic chemistry textbooks of the early twentieth century, illustrates the gradual shift away from teaching rules of systematic nomenclature and toward teaching alternative practices of systematic nomenclature. First published in Dutch in 1898, fifty-two editions of Holleman's textbook were published in nine languages over the following three decades.[24] The book was intended to serve the needs of undergraduate classes in organic chemistry, as an up-to-date instructional text that, in implicit contrast to Meyer and Jacobson's magisterial work, made "no claim to be a 'Beilstein' in a very compressed form."[25]

The first German edition of Holleman's textbook, published in 1899, contained a section dedicated to explaining the workings of the Geneva nomenclature. Toward the beginning of each of the book's chapters, Holleman specifically addressed how the Geneva rules were to be applied to the class of compounds that the chapter addressed. These explicit discussions of the Geneva nomenclature disappeared in subsequent editions of the textbook. However, Holleman continued to address the nomenclature conventions most often used to name each class of compounds, including (without identifying them as such) procedures codified by the Geneva Congress. As time went by, such Geneva-style names took on more and more prominence in Holleman's text. For example, in the fifth English edition, published in 1920, Holleman began to name branched hydrocarbons according to the Geneva method as derivatives of the longest carbon chain rather than by arbitrarily selecting a central carbon atom, treating it as

[23] Ibid., 146.

[24] Arnold F. Holleman, *A Textbook of Organic Chemistry*, 7th English ed. (New York: Wiley, 1930), vii.

[25] A.F. Holleman, *A Text-book of Organic Chemistry*, 1st English ed., trans. A. Jamieson Walker (New York: Wiley, 1903), iii.

"methane," and regarding the rest of the compound as a collection of side chains, as in previous editions.[26]

Still other textbooks addressed the various and often complex approaches to organizing reference works that chemical editors developed in lieu of the Geneva rules during the 1900s and 1910s. For example, Jacobson took the second edition of his textbook as an opportunity to describe and promote the molecular formula index that Max Moritz Richter had used in his *Lexikon der Kohlenstoff Verbindungen* and that Jacobson had applied in the index to the *Berichte* and, later, in other reference works.[27]

During the 1920s, a new genre of textbook addressed itself exclusively to instructing chemists in how to search the chemical literature. *Chemical Abstracts* editor E.J. Crane teamed up with nomenclature guru Austin Patterson to publish *A Guide to the Literature of Chemistry*, a 1927 work providing an overview of the numerous reference works that had become such important aids to chemical research. The most extensive and widely-used of these works, Beilstein's *Handbuch*, spawned a genre of reference works all its own. Various chemists published articles over the course of the 1920s instructing their colleagues in the intricacies of the "Beilstein system."[28] In 1929, the creators of this system, Dora Stern and Bernhard Prager, published a 250-page *Leitfaden für die Benutzung von Beilsteins Handbuch* ("User's Guide to Beilstein's Handbuch"). Instruction in the finer points of the Beilstein system became a well-established part of the organic chemistry curriculum.[29]

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[26] A.F. Holleman, *A Text-book of Organic Chemistry,* 5th English ed., trans. A Jamieson Walker (New York: Wiley, 1920), 50; compare Holleman, *A Text-book of Organic Chemistry*, 1st English ed., 48.

[27] Meyer and Jacobson, *Lehrbuch der organischen Chemie*, 2nd ed., vol. 1, 148.

[28] Donald W. MacArdle, "Classifying Organic Compounds by the Prager-Jacobson System," *Chemical and Metallurgical Engineering* 22, no. 6 (1920): 256–258; Paul Jacobson, "Beilsteins Handbuch der Organischen Chemie, ein Spiegel ihrer Entwicklung," *Naturwissenschaften* 7, no. 14 (1919): 222–25.

[29] A fictional professor of organic chemistry in a 1956 detective story by Isaac Asimov (himself a PhD in physical chemistry) described the organization of Beilstein as follows: "The various compounds are listed according to a logical system which is clear but not obvious. I myself give several lectures in my course on organic synthesis

Teaching with nomenclature

A final group of chemical instructors did not teach nomenclature rules or methods, but rather employed nomenclature as a pedagogical tool. As discussed in Chapter 1, Friedel had launched the program of nomenclature reform that led to the Geneva Congress with the goal of recasting "the language itself" of French chemical education in terms of structure theory.[30] While Béhal's text did so to a certain extent, the textbook that most fully realized this aim was written by Friedel's Romanian protégé, Istrati.

Just after the Geneva Congress, Istrati returned to an elementary chemistry textbook that he had just published in 1891 and rewrote and expanded it, hewing rigorously to the Geneva nomenclature in discussions of organic compounds throughout the text. Istrati published the revised edition in Romanian in 1893; Friedel arranged for its translation and penned a laudatory preface for the French edition of the textbook, published in 1895.

Like Meyer and Jacobson, Istrati printed the text of the Geneva resolutions at the end of his textbook, and like Béhal, he briefly described procedures for naming a compound's hydrocarbon skeleton according to the Geneva rules.[31] In the remainder of the work, however, Istrati employed the Geneva nomenclature without comment. He presented the systematic Geneva terms simply as the names proper to all organic compounds, relegating chemical names in common use to parentheses. In many cases, this required extrapolating from the rules to address compounds not formally addressed at Geneva; Istrati did so in accordance with the proposals of Friedel's nephew Alphonse Combes, who took charge of refining and extending the

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which deal entirely with methods for finding a particular compounds somewhere in the sixty volumes." (Spoiler alert: Beilstein proved to be the key to solving the mystery of the murder of a chemistry librarian.) Isaac Asimov, "What's In A Name?" [1956], in *Asimov's Mysteries* (Garden City: Doubleday, 1968), 38–53, quotation on 44.

[30] Charles Friedel, "La vie et les travaux de Wurtz," introduction to Charles Adolphe Wurtz, *La théorie atomique*, 4th ed. (Paris: F. Alcan, 1886), xxvi.

[31] Constantin I. Istrati, *Cours élémentaire de chimie*, trans. Alphonse Adam (Paris: G. Carré, 1895), 347–348.

Geneva rules in the years following the Congress.[32] Like other textbook authors, Istrati described well-known methods for synthesizing many of the compounds that he described. However, Istrati illustrated all of these synthetic sequences with both structural formulas and systematic names of reactants, intermediates, and products. In this way, he used systematic names to reinforce the chemical relationships displayed by structural formulas (*Fig. 1*).

In his introduction to Istrati's text, Friedel acknowledged that the long Geneva names might seem laughable to some – as had the nomenclature of Lavoisier and Guyton a century before, he pointed out. However, Friedel went on, the Geneva nomenclature "has the great advantage of being logical, and consequently mnemonic. It will be better appreciated by those whose memory is not encumbered by the names currently in use, names that the old chemists are obliged to forget in order to learn [new ones], as has been the case for all those who have taken part in the transformations in chemistry over the past fifty years."[33] Nomenclature was not a subject to be taught, in this view, but a means of teaching. New chemists, using new chemical names, would do new chemistry.

[32] Alphonse Combes, "Chimique (nomenclature)," in *Dictionnaire de chimie pure et appliqué*, 2nd supp., vol. C (Paris: Hachette, 1894), 1060–1076.

[33] Istrati, *Cours élémentaire de chimie*, vi-vii.

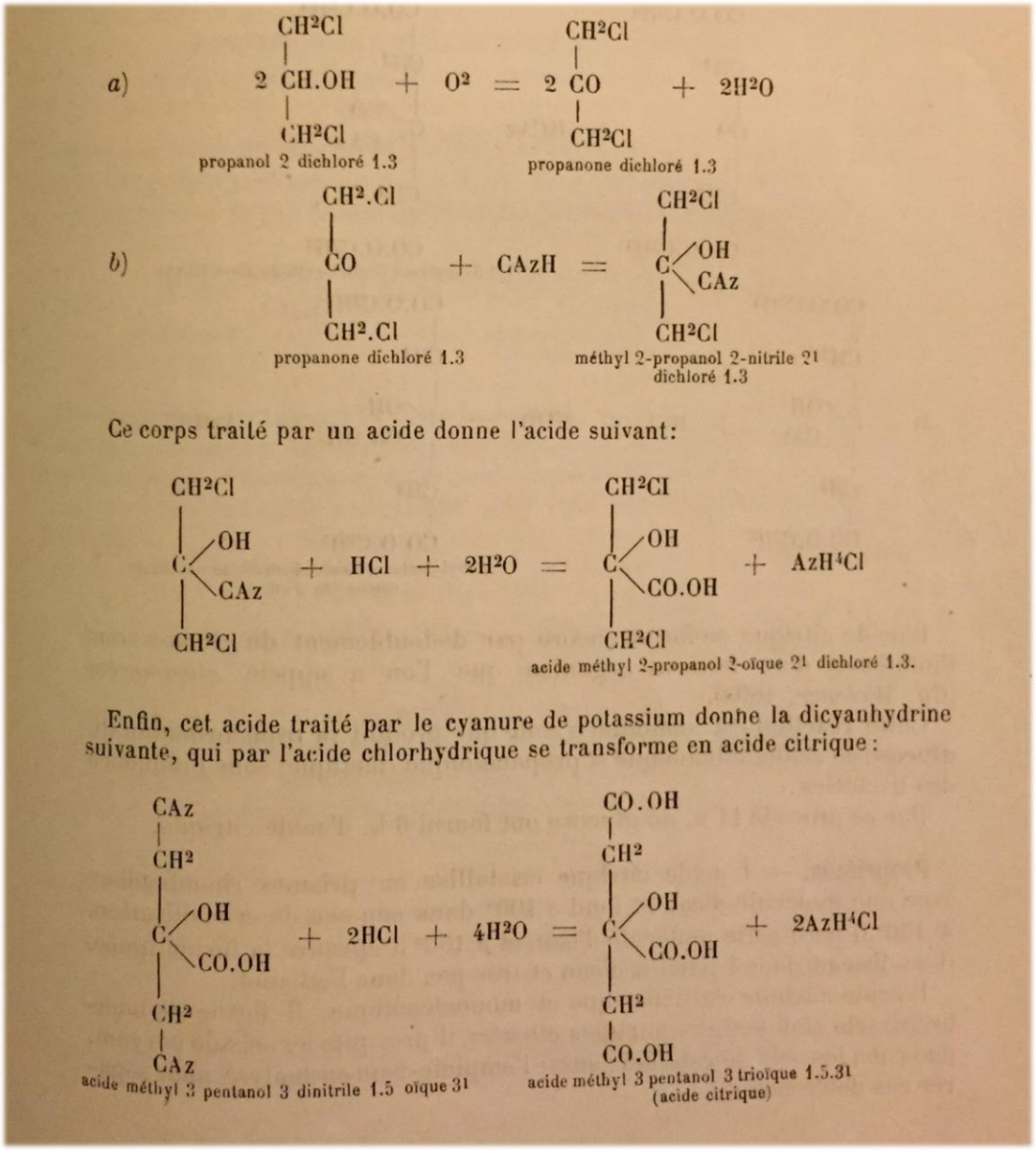

Figure 1. Istrati's presentation of the steps in the synthesis of citric acid in the French translation of his *Cours élémentaire de chimie*. The synthetic sequences proceeds from top left, left to right and top to bottom, to the final product (*acide citrique*, bottom right). Note how the relationships among the systematic names that Istrati uses to refer to each intermediate compound in this synthesis reinforce the relationships among the structural formulas used to represent these compounds.

However, radical linguistic rupture on the model of the Chemical Revolution was not the only way to teach with nomenclature. During the fall of 1895, the *Faculté des sciences* of the

University of Geneva put its chemistry students on notice. "Most theses which have been submitted recently are very defective from the linguistic point of view," explained Amé Pictet, and the circulation of these shoddy theses was damaging the international standing of the University's *École de chimie*.[34] Buttressed by the reflections on the importance of chemical language discussed in the introduction to this chapter, Pictet took on a new semester-long course dedicated exclusively to chemical nomenclature and style.

While he did present an outline of the Geneva system, Pictet did not focus on this latest development in the nomenclature of organic chemistry. Rather, his approach was historical. "Many attempts have been made at various times to give carbon compounds names expressing their constitution," he explained to his students at the outset of the course. He divided these attempts into five categories, arranged in chronological order: "1. Adaptation of inorganic nomenclature, 2. Theory of radicals, 3. Designation of functions, 4. Theory of substitution, and 5. Reform of 1892." Each of these methods of nomenclature, Pictet continued, "carried the trace of the prevailing ideas of the period."[35] The succession of methods of nomenclature thus mapped the historical development of the discipline.[36]

But Pictet did not simply wish to deepen his students' understanding of the chemistry of the past. "Each of these systems leaves something of itself to the following systems," he explained, "so that the current nomenclature is a summary of all the preceding nomenclatures."[37] Put another way, "the current name is a mélange of all of the previous systems."[38] Teaching chemical nomenclature was a means of teaching students that each chemical name was a palimpsest of various conceptions of the molecular world. To use a

[34] Pictet, "Nomenclature et terminologie chimiques," 201.
[35] Ibid., 249.
[36] Cf. Crosland, *Historical Studies in the Language of Chemistry.*
[37] Pictet, "Nomenclature et terminologie chimiques," 249.
[38] Ibid., 207.

chemical name was to invoke different and sometimes conflicting ideas about chemical structure and relationships. Pictet employed nomenclature as a teaching tool in precisely the opposite manner than that advocated by Friedel and pursued by Istrati – not to impose a new order and sweep away a burdensome past, but to provide students with insight into how that past continued to inform their own research and communication.

Neither method proved especially enduring. University records suggest that Pictet taught the nomenclature course only once. Prospects for the application of the Geneva rules in their most rigorous form waned, and Istrati's textbook was not reprinted. With the exception of Istrati himself, in his little-read nomenclature opus of 1913, no other textbook author hewed so completely to the guidelines of the Congress. Nomenclature remained a teaching tool, but of a more practical sort, as in the nomenclature-focused quiz questions scattered through 1922 textbook of the MIT chemist James Norris. The solution of these questions, Norris explained, "involves, in most cases, a careful study of the principles discussed and their intelligent application."[39] The principles that Norris discussed were not the rules of any specific system of nomenclature, but the diverse approaches of many different methods of nomenclature. Nomenclature was a means of giving students a feel for structural formulas as both abstractions and sites of chemical identity: how to disassemble them, reassemble them, and recognize when they were equivalent and how they differed. The Geneva rules disappeared from chemical pedagogy, but the union of chemical substance, structural formula, and chemical name deepened.

Patents

"As in a newly discovered gold country," wrote the physical chemist Wilhelm Ostwald of his *Organiker* colleagues in 1887, "the current of researchers in the field of organic chemistry

[39] James F. Norris, *The Principles of Organic Chemistry* (New York: McGraw-Hill, 1922), viii.

pushes on toward the single goal of finding gold, that is, synthesizing new substances; fierce fighting breaks out everywhere over the ownership and value of individual discoveries, and, given the seemingly inexhaustible wealth of the soil, the work inevitably in the end assumes a somewhat mechanical character."[40]

As Ostwald noted, synthetic organic compounds were not just objects of chemical research. Through the efforts of chemical engineers and the emerging apparatus of high-tech industrial capitalism, they could be produced in large quantities as dyes and drugs, incorporated into luxury goods, consumer products, and commercial pharmaceuticals, and thus converted into wealth for chemists and entrepreneurs.

Unlike the gold nuggets of Ostwald's epic simile, however, synthetic organic compounds were not simply lying dormant, waiting to be discovered. By the turn of the twentieth century, a critical link in the chain of transmutations that converted coal tar to gold (that is, the raw materials of synthetic organic chemistry to industrial riches) was making a chemical into intellectual property – or, at least, making sure that nobody else could stake such a claim.

This process was mediated by patent law. Like the discipline of chemistry, national patent systems were developing their own institutions, rules, experts, and information technologies.[41] As with chemical pedagogy, the modes of order and the research practices of

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[40] Wilhelm Ostwald, "Die Aufgaben der physikalischen Chemie," *Humboldt Monatsschrift für die gesamten Naturwissenschaften* 6 (1887): 252. Such "prospecting" among synthetic organic chemists might be compared to the shift among field scientists of the late nineteenth and early twentieth centuries toward explorations along what Robert Kohler has termed "inner frontiers"; Robert E. Kohler, *Landscapes & Labscapes: Exploring the Lab-Field Border in Biology* (Chicago: University of Chicago Press, 2002); Robert E. Kohler, "Reflections on the History of Systematics," in *The Evolution of Phylogenetic Systematics*, ed. Andrew Hamilton (Berkeley: University of California Press, 2014), 17–46. On empire, the closing of frontiers, and the topography of scientific inquiry, see also Rosalind Williams, *The Triumph of Human Empire: Verne, Morris, and Stevenson at the End of the World* (Chicago: University of Chicago Press, 2013).

[41] On the history of patent law vis-à-vis the history of science and technology, see Mario Biagioli, "Patent Republic: Specifying Inventions, Constructing Authors and Rights," *Social Research* 73, no. 4 (2006): 1129–1172; Mario Biagioli, Peter Jaszi, and Martha Woodmansee, ed., *Making and Unmaking Intellectual Property: Creative Production in Legal and Historical Perspective* (Chicago: University of Chicago Press, 2011); Peter Galison, *Einstein's Clocks and Poincaré's Maps: Empires of Time* (New York: W.W. Norton, 2003), 221–293.

patent law dovetailed in certain ways with systematic nomenclature and chemical reference literature, and diverged in others. At both the intersections and the gaps, new chemical objects and information technologies came into being.

Patents and chemical literature

Where they exist, systems of patent law in modern liberal democracies are grounded in a bargain between the entrepreneur and the public interest, as represented by the state – a bargain sealed with a description of a novel invention. The public gets valuable information: a detailed account of a useful new technology that the inventor might otherwise have kept secret. In exchange, the inventor receives a temporary monopoly over commercial exploitation of the invention. Everyone benefits from innovation and economic growth, spurred on by the accessibility of information, the allure of future patent monopolies, and the limited time-frame of existing ones. That's the idea, at least.[42]

Patent laws are thus founded on the premise that they will make technical information publically accessible. In the area of chemical patents, however, during the early twentieth century, the modes of order employed by patent offices in their publications often seemed to fall short of this goal. In their 1927 guide to the use of chemical reference literature, Patterson and Crane noted that the publications of most national patent offices were ordered by patent

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On patents and the life sciences, see Myles Jackson, *The Genealogy of a Gene: Patents, HIV/AIDS, and Race* (Cambridge, MA: MIT Press, 2015); Sheila Jasanoff, *Designs on Nature: Science and Democracy in Europe and the United States* (Princeton: Princeton University Press, 2005), 203–224; Graham Dutfield, *Intellectual Property Rights and the Life Science Industries: Past, Present and Future*, 2nd ed. (Hackensack, NJ: World Scientific, 2009).

On patents in the chemical industry, see Anthony S. Travis, *The Rainbow Makers: The Origins of the Synthetic Dyestuffs Industry in Western Europe* (Bethlehem: Lehigh University Press, 1993); Joseph M. Gabriel, *Medical Monopoly: Intellectual Property Rights and the Origins of the Modern Pharmaceutical Industry* (Chicago: University of Chicago Press, 2014); Johann Peter Murmann, *Knowledge and Competitive Advantage: The Coevolution of Firms, Technology, and National Institutions* (New York: Cambridge University Press, 2003); Kathryn Steen, *The American Synthetic Organic Chemicals Industry: War and Politics, 1910-1930* (Chapel Hill: University of North Carolina Press, 2014); Arndt Fleischer, *Patentgesetzgebung und chemisch-pharmazeutische Industrie im deutschen Kaiserreich (1871-1918)* (Stuttgart: Deutscher Apotheker Verlag, 1984).

[42] There is a substantial amount of scholarship in economic history addressing the relationship between intellectual property rights, innovation, and economic growth. For a recent review, see Petra Moser, "Determinants of Creativity and Innovation—Evidence from Economic History," *NBER Reporter* no. 3 (2015): 23–26.

number, which provided no information about the subject of the patent. Indexes of patent titles were often available, but inventors had an interest in giving away as little information as possible in their patent applications. Patent titles were typically too general to be of much use and were sometimes actively misleading. As a remedy, patent offices classified patents, often in great detail. As of the 1920s, for example, American patents were separated into fifty-five divisions, 298 classes, and thousands of subclasses. However, these classifications were of limited use for chemists; chemical patents were scattered among numerous different divisions and hundreds of subclasses, none of which corresponded to the approaches to classification by substance or by subdiscipline most often used in the chemical literature. And that was just one patent office; most nations classified patents in according to entirely different systems (although Crane and Patterson noted that a countries in central Europe and Scandinavia had adopted the German system, "which is regarded as excellent").[43]

In this area, as for the journal literature, an enterprising German chemist had taken on the challenge of gathering the disorderly mass of print information and putting it into a useful order. This Beilstein of chemical patents was Paul Friedländer, a soft-spoken dye chemist and longtime instructor at well-regarded *Technische Hochschulen* (polytechnic colleges) in Karlsruhe and, later, Vienna.[44] Alongside his teaching and laboratory research, Friedländer compiled the *Fortschritte der Teerfarbenfabrikation und verwandter Industriezweige*, a running catalog of all German patents addressing dyes, drugs, perfumes, flavorings, and chemicals used in the manufacture of these products. Friedländer grouped patents according to a somewhat heterogeneous set of categories drawn from chemical industry usage: intermediates, triphenylmethane dyes, naphthalene derivatives, azo dyes, indigo, pharmaceuticals, vanillin,

[43] Crane and Patterson, *A Guide to the Literature of Chemistry*, 109–111, quotation on 109.
[44] Walter Roth, "German Letter," *Industrial & Engineering Chemistry* 16, no. 1 (1924): 99.

and so on.[45] Entries included the full text of each patent – a crucial matter since, as Patterson and Crane put it, "a fortune may hang on the wording of a single phrase or sentence."[46]

Useful as Friedländer's reference was, it had its limits. For one thing, it made only occasional reference to non-German patents. Moreover, not even a complete collection of patents issued around the world would satisfy the information requirements of chemical entrepreneurs and patent examiners. Both groups had to be able to determine what was *patentable* as well as what was already patented. Most importantly, products and processes previously described in publications were usually ineligible for patent protection.

In order to facilitate thorough and efficient searches for the patentability of chemical products, some patent offices developed their own resources for identifying and ordering chemical compounds. As described in Chapter 3, for example, Edwin Hill of the US Patent Office developed a method for writing molecular formulas of compounds for the purposes of ordering and searching information about large numbers of compounds using index cards. Hill first worked out this method in 1899; within eight years, the Patent Office had assembled an index of 500,000 index cards based on information from a wide variety of chemical publications. As Hill had noted from the start, his system for writing molecular formulas was superficially quite similar to the formula indexes of Richter and Jacobson. However, Hill emphasized that his approach was simpler, better adapted to ordering inorganic as well as organic compounds, and more suitable for the assembly of a card index rather than a printed reference work. In these ways, Hill explained, his card index had served admirably "for the use of the corps of Assistant

[45] Paul Friedländer, *Fortschritte der Theerfarbenfabrikation und verwandter Industriezweige*, Part 4, 1894–1897 (Berlin: Springer, 1899), v-viii.

[46] Crane and Patterson, *A Guide to the Literature of Chemistry*, 123.

Examiners in making their searches on patent applications pertaining to chemical substances."[47]

Of course, patents were of interest to others, too. From the 1880s on, an increasingly large proportion of research in organic chemistry – especially research carried out in the laboratories of chemical firms – was published *exclusively* in patents. Even when questions of intellectual property were not at stake, noted Patterson and Crane, "few searches with an industrial aspect are complete without the examination of this immense field." Indeed, "even the investigator in pure science cannot afford to neglect the patent literature entirely," although there was "a tendency on the part of some to underestimate the value of patents as a source of chemical information."[48]

While Friedländer's *Fortschritte* and the publications of patent offices were vital resources for chemists with patent-specific questions, chemists simply interested in patents as a source of chemical information could turn to some of the same reference works that they used to sift through the journal literature. Many abstract journals, including *Chemical Abstracts* and *Chemisches Zentralblatt*, the two foremost such publications, abstracted patents as well as articles. Indeed, by the mid-1920s about a quarter of the abstracts in the *Zentralblatt* addressed patents.[49] The various other reference publications of the Deutsche Chemische Gesellschaft – Beilstein's *Handbuch*, Richter's *Lexicon*, Stelzner's *Literatur-Register* – drew in data from patents and provided numerous patent citations.[50]

Patent identity and chemical identity: Intersections and divergence

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[47] Edwin A. Hill, "The Chemical Card Index of the Patent Office," *Journal of the American Chemical Society* 29, no. 6 (1907): 936–937.

[48] Crane and Patterson, *A Guide to the Literature of Chemistry*, 103.

[49] Ibid., 115.

[50] For example, the 1920 second volume of the 4th edition of Beilstein contained about 300 references to the *Journal of the American Chemical Society*, but well over 400 references to German patents.

At times, the institutions and technologies that dealt with information about chemical patents and with chemical information in general converged. In 1907, Hill decided to open the card file of the US Patent Office for consultation by the chemical general public, either by visiting the patent office in Washington in person or, for a nominal fee, by correspondence. As for patent examiners, Hill wrote, so too for other chemists, the index would provide a "quick and easy method of finding the exact printed matter in existence relating to the substance they are interested in." Indeed, he described the aim of the card index in terms similar to the highest ambitions of the contemporaneous institution-building activities of German and American editors: no less than "to perform the same office for the whole field of chemical literature that the index of any particular book does for the book itself."[51]

By 1920, the card index had fallen on hard times. As costs mushroomed throughout the patent office, appropriations for maintaining the card index diminished. With a backlog of 100,000 cards remaining to be filed among the million already sorted, the office stopped trying to keep the index up to date. That very year, *Chemical Abstracts* began publishing annual formula indexes. Notwithstanding Hill's earlier emphasis on the distinctions between his card catalog and a print reference work, the scope of the literature covered by *Chemical Abstracts* meant that its indexes could credibly satisfy the omnibus chemical search needs of chemists and patent examiners alike, just where Hill's card index left off.

In other ways, however, the conception of chemical identity reinforced by systematic names and chemical reference works diverged from the developing legal definition of chemical inventions. In general, according to US patent law as it had developed in the nineteenth century, inventors were not permitted to recite a list of alternative embodiments of an invention in a patent claim. Rather, they were required to file separate applications treating

[51] Hill, "Chemical Card Index," 940–941.

each version as a distinct invention. An 1870 ruling clarified that it was permissible for applicants to make a claim using a single generic term encompassing a range of specific embodiments. Thus, well before the widespread use of structural formulas and the decisions of the Geneva Congress linked chemical identity to a pattern of atoms and bonds, chemists had a legal mechanism for defining the identity a chemical invention at a higher level of generality.[52]

However, lists of specific, equivalent embodiments of an invention were still barred.[53] This was a problem for chemical inventors who wished to claim miscellaneous collections of compounds of related structure as functionally equivalent for the purposes of the invention detailed in the patent. Such claims were an important means of preventing competitors from bringing a structurally-modified but functionally equivalent versions of products to market. (Just such a conflict had led to a protracted legal battle and, eventually, a joint venture between the German dye firms Bayer and Agfa during the 1880s.)[54] Chemists sometimes claimed such alternative structures in their patent applications, anyway. Often, examiners allowed the claims to stand.

In 1923, however, one examiner did not. New Jersey chemist Eugene Markush filed for a patent on a dye produced by coupling halogen substituted pyrazolones (a permissible generic term) with aniline, aniline homologues, or halogenated derivatives of aniline. The examiner rejected the patent, on the basis that such a list of alternatives were not permissible. Markush responded by replacing this list with the generic term "a diazotized unsulphonated mono-amine." Once again, the examiner rejected the patent, on the basis that the generic term now encompassed many chemical compounds that would not behave in the manner indicated in the

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[52] Harold C. Wegner, "The Right to Generic Chemical Coverage," *APLA Quarterly Journal* 6 (1978): 257–270.

[53] "Ex parte Eagle," *Decisions of the Commissioner of Patents* (1870): 137.

[54] Carsten Reinhardt and Anthony S. Travis, *Heinrich Caro and the Creation of Modern Chemical Industry* (Boston: Kluwer Academic Publishers, 2000), 275–278. See also Henk van den Belt and Arie Rip, "The Nelson-Winter-Dosi Model and Synthetic Dye Chemistry," in *The Social Construction of Technological Systems*, 2nd ed., ed. Wiebe E. Bijker, Thomas P. Hughes, and Trevor Pinch (Cambridge, MA: MIT Press, 2012), 147–148.

patent. Markush settled on "a diazotized unsulphonated material *selected from the group* consisting of aniline, homologues of aniline and halogen-substitution products of aniline." The examiner deemed this an evasive and unsuccessful attempt to thread the needle between the improper generic term and the disallowed list. An applicant couldn't establish a legitimate generic term just by referred to a list of objects as "a group."[55]

On appeal, the commissioner of patents overturned the decision. He reasoned that the infelicitous mismatch of the conventions of chemical nomenclature with the terminology of patent law should not force an applicant to file a burdensome number of patents for the same invention – additional patents which might well be rejected anyway, on the grounds that they were equivalent to the first one. In this decision, the commissioner formally recognized the permissibility of a practice that had become somewhat common in patent claims – representing a generic carbon chain in a structural formula with the letter "R."[56]

In this way, a new kind of chemical object, the "Markush structure," came into being. Each represented a single legal chemical substance that encompassed many distinct systematic names and structural formulas. Markush structures became familiar parts not only of chemical patents but of chemical pedagogy and practice (the "R" group is a familiar sight to every organic chemist). However, the decision cleaved a division between the infrastructure of chemical information and patent law. Over the course of the twentieth century, this division became so substantial and significant that methods for performing "Markush searches" on

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[55] My emphasis. "Ex Parte Markush," *Decisions of the Commissioner of Patents* (1925): 127; Eugene A. Markush, "Pyrazolone dye and process of making the same," US1506316 A, filed January 9, 1923, and issued August 26, 1924, <http://www.google.com/patents/US1506316>.

[56] "Ex parte Markush," 128–129; Helen Cooke, "A Historical Study of Structures for Communication of Organic Chemistry Information prior to 1950," *Organic & Biomolecular Chemistry* 2:22 (2004): 3179–3191.

chemical databases became a highly complex and lucrative area of chemical information technology.[57]

Pharmaceuticals

Organic chemical compounds mattered to many people other than organic chemists. That was why synthetic chemicals garnered such careful attention as intellectual property, and inspired Ostwald's colorful denunciation of organic chemistry. From the scientific perspective of the physical chemist Ostwald, organic chemical compounds were fool's gold; as dyes and drugs, they were the real thing.

Making a compound into a dye or, especially, into a drug took more than a patent. Creating a pharmaceutical was the work not only of chemists but also bacteriologists, clinical researchers, marketers, and physicians. All were engaged in identifying and classifying chemical drugs, but they did so based on different concerns and from different perspectives, many of which sharply diverged from the principles of chemical identity and relationships encoded in systematic names and chemical reference works. This section will address three pharmaceutical products: Trypan Red, Aspirin, and Vitamin C. Naming was a means for developing and contesting non-chemical conceptions of these chemical substances: as objects of bacteriological and clinical research; as distinctive brands on the market; as ethical therapeutics. The negotiation of nomenclature for these three pivotal pharmaceuticals sheds light on the economic, ethical, and epistemic priorities that have shaped modern biomedicine.

Trypan Red

In 1902, the German chemist-physician Paul Ehrlich launched an ambitious research program in experimental chemotherapy.[58] His aim was to identify synthetic chemical

[57] J.M. Barnard and P.M. Wright, "Towards In-House Searching of Markush Structures from Patents," *World Patent Information* 31, no. 2 (2009): 97–103.

substances capable of damaging disease-causing microorganisms while doing relatively little harm to their hosts, and then tinkering with the structure of those substances to maximize their efficacy and minimize their toxicity.[59] Ehrlich's first chemotherapeutic target was the trypanosome, a unicellular protozoon that microbiologists had recently identified as the cause of an epidemic of sleeping sickness then raging in sub-Saharan Africa. Ehrlich assigned the microbiological portion of this project to Kiyoshi Shiga, a Japanese bacteriologist who had recently come to work in Ehrlich's Institute for Experimental Therapy after discovering the bacteria (later named *Shigella*) responsible for bacterial dysentery (*Shigellosis*).

Ehrlich directed the work of Shiga and the other scientists at his Institute through a constant stream of *Blöcke* – short memos that were a primary medium of informal, quotidian communication within the lab.[60] As Ehrlich prepared to put Shiga to work on trypanosomes, he wrote down lists of substances to be tested on the protozoa. These substances included antibody-laden blood serum, as well as arsenic and arsenic-based pharmaceuticals. Most, however, were synthetic dyes, which Ehrlich referred to by the combination of semisystematic names and trade names – "Dioxyacridine," "Brilliant Blue" – by which these dyes were typically known in commerce (*Fig. 2*). In his instructions to Shiga regarding these "color experiments" [*Farbversuche*], Ehrlich singled out specific dyes by reference to their color, a feature of their chemical structure, or both. In a typical memo, Ehrlich wrote: "Please let me know whether the

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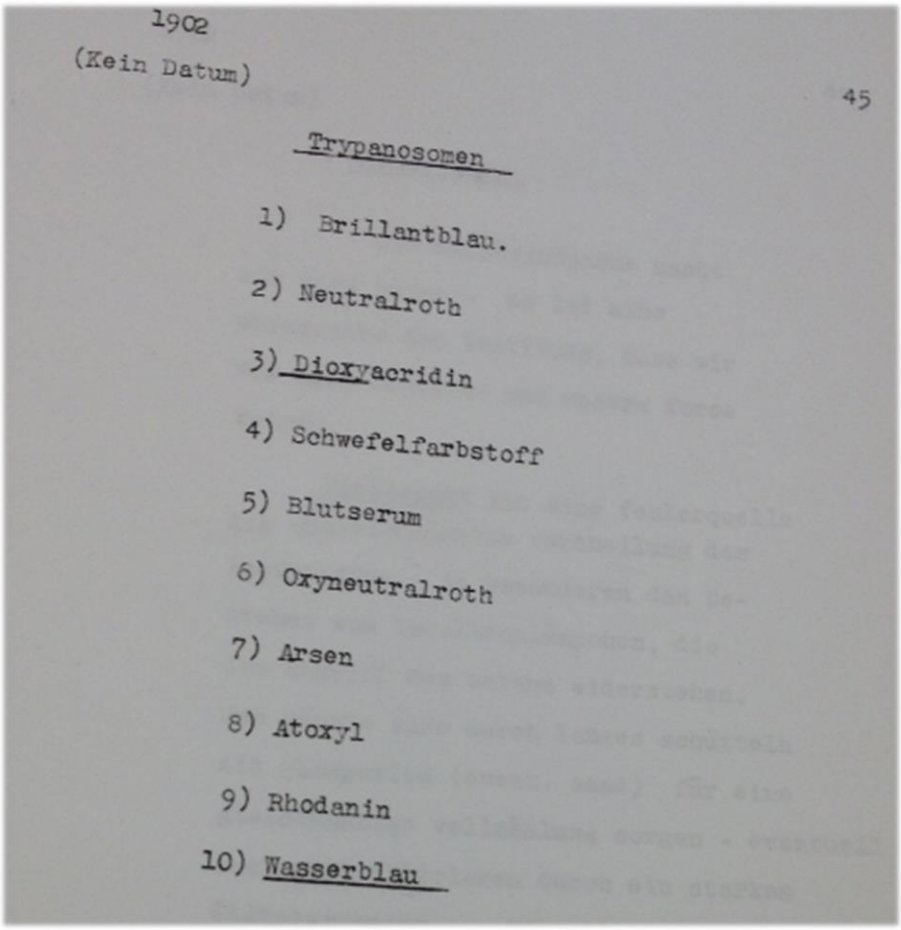
[58] This research culminated in the 1909 discovery of Salvarsan, the celebrated arsenic-based syphilis remedy widely regarded as the realization of Ehrlich's chemotherapeutic vision: a drug that would "sterilize the infected organism and break the neck of the disease." Paul Ehrlich, "Address Delivered at the Dedication of the Georg-Speyer-Haus [1906]," in Paul Ehrlich, *Gesammelte Arbeiten*, ed. and trans. F. Himmelwelt, Martha Marquardt, and Henry Dale, vol. III (Berlin: Springer, 1956), 59.

[59] John Parascandola, "The Theoretical Basis of Paul Ehrlich's Chemotherapy," *Journal of the History of Medicine and Allied Sciences* 36, no. 1 (1981): 19–43.

[60] Ehrlich sent his scientists a constant stream of these memos – often more than one a day – and wrote still more as reminders to himself. Ehrlich's secretary Martha Marquardt took down dated typescript copies. These typescripts thus provide access to something much closer to laboratory conversation than most written sources.

new dyes killed trypanosomes - probably not. Have you studied vital dyes!! How good is the yellow nitroso compound?"[61]


1902

(Kein Datum)

45

Trypanosomen

1) Brillantblau.

2) Neutralroth

3) Dioxyacridin

4) Schwefelfarbstoff

5) Blutserum

6) Oxyneutralroth

7) Arsen

8) Atoxyl

9) Rhodanin

10) Wasserblau

Figure 2. Typescript copy of Paul Ehrlich note to self on substances to be tested on trypanosomes, Dec 1902.[62]

When Shiga hit upon a particularly effective benzopurpurin dye, Ehrlich used different sorts of names for its derivatives in directing Shiga to submit them to intensive tests. In notes to himself, Ehrlich set his sights on one such dye, to which he gave the names "the new red" and "Benzopurpurin 8."[63] To Shiga, he referred to it as "Dye 8," "8," "red dye 8," and "red no. 8."[64]

[61] Memo, Ehrlich to Shiga, 25 Jan 1903, Paul Ehrlich papers, Series 2 – Zettel Book IV, Box 82–83, 170.

[62] Ehrlich note to self, Dec 1902, Paul Ehrlich papers, Series 2 – Zettel Book IV, Box 82–83, 45. By permission of Rockefeller Archive Center.

[63] Ehrlich notes to self, 16 Mar and 1 Apr 1903, Paul Ehrlich papers, Series 2 – Zettel Book IV, Box 82–83, 300, 366.

Shiga continued to test other sorts of dyes, which Ehrlich referred to as before: "the new yellow preparation" and "my new methylene blue," for example.[65] But as Red 8 proved itself over the course of more and more trials, Ehrlich dropped the number. By June 1903, two months after they had begun investigating it, Ehrlich was referring to the experimental compound simply as "Red" (*Roth*); the name was particularly apt, as the skin of experimental animals administered the substance took on that color.[66] The name "Red" linked the substance with the sign of its administration, and reinforced Ehrlich's conception of the common chemical mechanism responsible for an effective dye and an effective drug.

Ehrlich obtained precisely modified chemical substances for his therapeutic trials from Arthur Weinberg, director of the Cassella dye firm and, in Ehrlich's judgment, a "first-rate chemist."[67] Ehrlich lamented to a colleague in late 1903 that he was "so busy in the Institute that I cannot allow myself to relax by doing a little chemistry."[68] In his chemical correspondence with Weinberg, he had occasion to scratch that organic chemical itch, through the copious use of semisystematic chemical names.

As Shiga's work began to show promising results for the benzopurpurin series, Ehrlich turned to Weinberg to obtain related compounds. He wrote to his friend asking for certain kinds of derivatives – "tri- and tetra-sulfonic acids of the benzopurpurin type" – and suggested a list of subunits that Weinberg might substitute for the constituent parts of benzopurpurin

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[64] Ehrlich to Shiga, memos of 1 Apr and 2 Apr, Paul Ehrlich papers, Series 2 – Zettel Book IV, Box 82–83, 365, 372; Ehrlich to Shiga, memos of 31 May, 10 Jun, 14 Jun 1903, Paul Ehrlich papers, Series 2 - Zettel Book V, Box 83, 68, 116, 134.

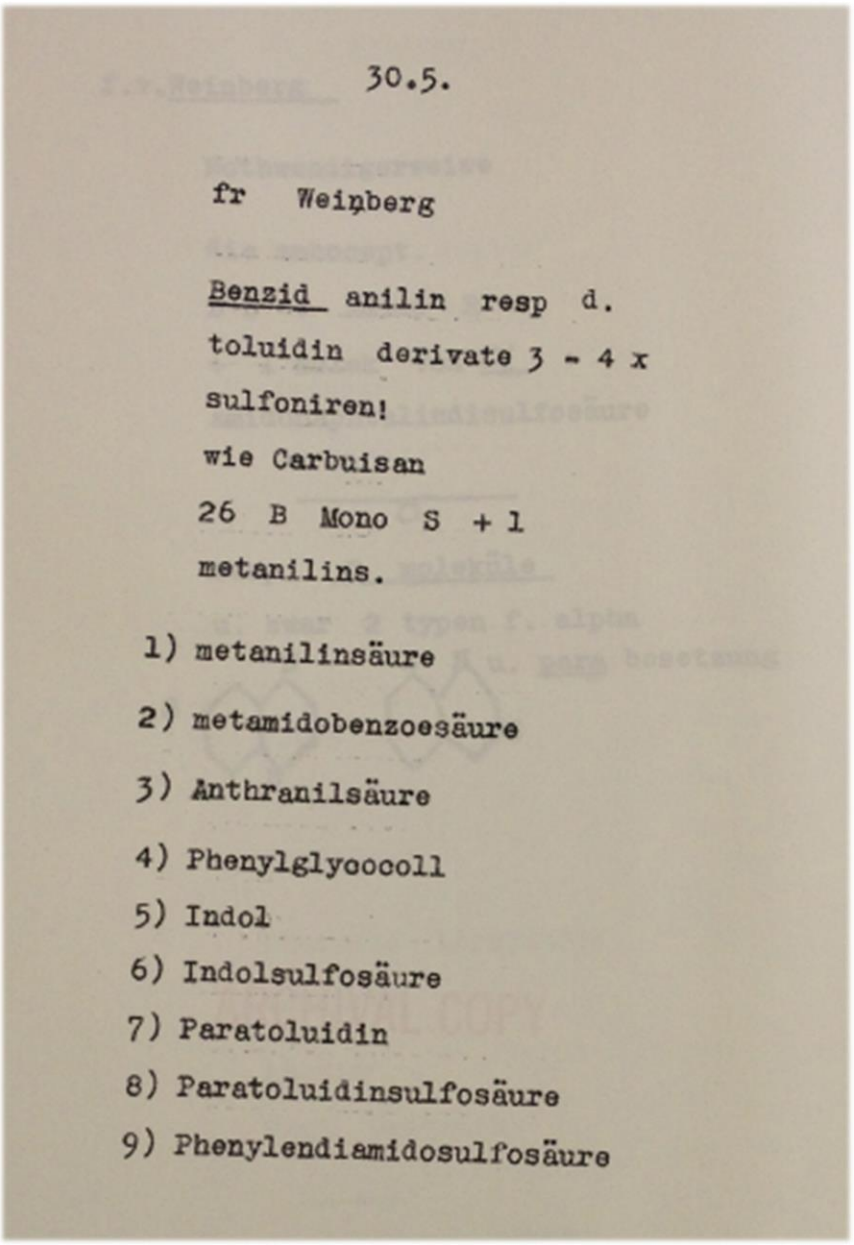
[65] Ehrlich to Shiga, memo of 10 May 1903, Paul Ehrlich papers, Series 2 – Zettel Book IV, Box 82–83, 494; Ehrlich to Shiga, memos of 10 May and 14 Jun 1903, Paul Ehrlich papers, Series 2 – Zettel Book V, Box 83, 134.

[66] Ehrlich explained this nomenclature to one new laboratory assistant, glossing "red animal" with a parenthetical "treated with dye." Ehrlich to Levaditi, 12 Jan 1901, Paul Ehrlich papers, Series 2 – Zettel Book II and Carcinom, Box 81–82, 77.

[67] In 1904, Cassella merged with Hoechst, a firm with a longstanding relationship with Ehrlich's Institute.

[68] Ehrlich to Bruhl, 31 Dec 1903, Paul Ehrlich papers, Series 5 – Copy Book XIII: Direktor Ehrlich, Box 23, 51.

(*Fig. 3*). In both cases, Ehrlich's names suggested operations that Weinberg might perform, but did not explicitly enumerate specific compounds.



30.5.

fr Weinberg

Benzid anilin resp d.
toluidin derivate 3 - 4 x
sulfoniren!
wie Carbuisan
26 B Mono S + 1
metanilins.

1) metanilinsäure
2) metamidobenzoesäure
3) Anthranilsäure
4) Phenylglycocoll
5) Indol
6) Indolsulfosäure
7) Paratoluidin
8) Paratoluidinsulfosäure
9) Phenylendiamidosulfosäure

Figure 3. Typescript copy of Paul Ehrlich note listing variable chemical subunits with which he wished Weinberg to modify benzopurpurin to form additional compounds for Shiga's trials.[69]

[69] Ehrlich note to self, 30 May 1903, Paul Ehrlich papers, Series 4 – Direktor Ehrlich I, Box 85, 65. By permission of Rockefeller Archive Center.

In 1904, Ehrlich and Shiga published a preliminary summary of this research. They had begun, they wrote, by studying "dyes of the benzopurpurin series," and discovered a weak but promising effect with one of these dyes, "the combination of tetrazotized benzidine and two molecules of sodium β-napthylamine-disulfonic acid (2 amido, 3, 6 sulfo)." Slightly modifying this substance yielded a compound with improved solubility and dramatically greater therapeutic efficacy; Ehrlich and Shiga printed the structural formula of this compound. As for its name, "for the sake of brevity," wrote Ehrlich and Shiga, and in light of the unmistakable effect of its administration on the appearance of lab animals, "we therefore wish to name it *Trypan Red*."[70] The series of different names in Ehrlich and Shiga's article make visible the forms of interdisciplinary communication and collaboration that went into making this compound.

As Trypan Red gained a broader reputation, Ehrlich began to use this trivial name in correspondence with Weinberg as the basis for naming derivatives, such as "symmetric diamidosulfotrypan red" and "desulfodiamidotrypan red."[71] Yet when referring to Trypan Red itself, Ehrlich was unable to resist incorporating at least one systematic name into his letters to Weinberg. In one such request, he asked Weinberg "to please make a new portion of Trypan Red from benzidinemonosulfonic acid," the central subunit of the molecule, as Weinberg well knew.

Though Ehrlich used systematic and semisystematic chemical names in his correspondence with Weinberg, he avoided drawing structural formulas. The ubiquity of

[70] Paul Ehrlich and Kiyoshi Shiga, "Farbentherapeutische Versuche bei Trypanosomenerkrankung [1904]," in Ehrlich, *Gesammelte Arbeiten,* vol. III, 24–37, quotations on 24–25.

[71] Ehrlich to Weinberg, 25 Feb 1905, 5 Apr 1905, 6 Apr 1905, Paul Ehrlich papers, Series 5 – Copy Book XVI: Direktor Ehrlich, Box 23, 176–177, 404, 412–414.

structural formulas in chemical communication at the time makes their absence from one of Ehrlich's most chemically-intensive exchanges striking.[72]

Ehrlich had a reason to use chemical names rather than structural formulas in his letters to Weinberg: they made room for the chemist to bring his expert judgment and his firm's resources to bear on Ehrlich's requests. The early list of substructures that Ehrlich sent to Weinberg, for example, likely corresponded to chemical intermediates that Weinberg may have had in stock at the Cassella laboratory. By listing the names of these substances, Ehrlich was able to delineate a number of relevant substances that Weinberg might be able to produce quickly using starting materials he already had on hand.

Further, Ehrlich's use of names gave Weinberg room to propose alternatives to his requests. For example, when Ehrlich asked Weinberg to prepare a particular sort of highly sulfonated (and thus, Ehrlich hoped, more soluble) version of benzopurpurin, Weinberg responded within a day with an alternative plan. Ehrlich was grateful: "That the naphtylaminetrisulfonic acids are so little capable of coupling was a detail not known to me. I accept therefore your kind offer to make the dyes in question out of benzidine-mono- as well as disulfonic acids, with special thanks."[73] It was through this particular application of Weinberg's knowledge of the chemistry of azo-coupling that he and Ehrlich arrived at the compound that Ehrlich named benzopurpurin 8 and, eventually, Trypan Red. The success encouraged Ehrlich to conduct subsequent correspondence along similar lines. The following year, he asked Weinberg to prepare derivatives using "various Naphtylendiamine disulfonic acids," but acknowledged that he was not "sufficiently oriented in the chemistry of naphtylamine-

[72] Ehrlich's practice of dictating most of his voluminous correspondence certainly played a part in reducing his use of formulas, which had to be added after Marquardt finished typing the letter. This does not explain, however, why they are less frequent in his correspondence with Weinberg than in his letters to other chemist colleagues such as his nephew Franz Sachs.

[73] Ehrlich to Weinberg, 9 Mar 1903 and 11 Mar 1903, Paul Ehrlich papers, Series 5 – Copy Book X: Direktor Ehrlich, Box 88, 427–428, 450–451.

derivatives and their coupling ability to present proposals to you in this regard, and would therefore ask you which you consider most appropriate."[74]

In his correspondence with Weinberg, the value of systematic chemical names to Ehrlich lay not their absolute precision but in their adjustable imprecision. Such names allowed Ehrlich to calibrate the specificity of his requests according to the progress of his research, the exigencies of synthesis in an industrial laboratory, and Weinberg's superior knowledge of the chemistry of azo dyes.

Through Shiga's experiments on Weinberg's series of synthetic compounds, Ehrlich had been able to transform "benzopurpurin dyes" into a therapeutic capable of eliminating trypanosomes from the blood of laboratory animals. To complete the transformation of this chemotherapeutic into a drug to treat sleeping sickness, Ehrlich brought a clinical researcher, E.D.W. Greig, into his research network.[75] Greig was a physician and tropical medicine researcher in the British army. From a previous post in Bombay, he had provided Ehrlich with cobra venom for antivenin research; since then, Greig had moved on to a position in the British protectorate of Uganda.[76]

Discovering that he had a contact working in the midst of the sleeping sickness epidemic, Ehrlich rushed to ship his experimental compound to Greig. In a letter that accompanied the parcel, Ehrlich referred to the substance only as "the dye." The minimum therapeutic dose, Ehrlich wrote, would be one that brought about "a clear definite staining of the mucous membranes."[77] The next day, however, Ehrlich followed up with a letter to clarify

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[74] Ehrlich to Weinberg, 1 Nov 1904, Paul Ehrlich papers, Series 5 – Copy Book XV: Direktor Ehrlich, Box 23, 49–51.

[75] On the history of epidemiological, biological, and ecological research on sleeping sickness in colonial Africa, see Helen Tilley, "Ecologies of Complexity: Tropical Environments, African Trypanosomiasis, and the Science of Disease Control in British Colonial Africa, 1900-1940," *Osiris* 19 (2004): 21–38.

[76] Deborah Joy Neill, *Networks in Tropical Medicine: Internationalism, Colonialism, and the Rise of a Medical Specialty, 1890-1930* (Stanford: Stanford University Press, 2012), 165–181.

[77] Ehrlich to Greig, 1 Nov 1904, Paul Ehrlich papers, Series 5 – Copy Book XV: Direktor Ehrlich, Box 23, 48.

the identity of the dye as Trypan Red.[78] In his subsequent letters, Ehrlich used the term Trypan Red quite frequently, only occasionally referring to it as "the dye." He did the same in letters accompanying shipments of the drug to researchers in Java and India. As the name circulated through tropical medicine community, it became synonymous with the promise of chemotherapy.

This promise looked very different from the perspective of the subjects of these trials. For these African patients, as Deborah Neill has written in her history of tropical medicine, Trypan Red "was extremely unpleasant, was of dubious efficacy, and had horrible side effects." Doctors sometimes administered Ehrlich's experimental drug under threat of force, or to prisoners.[79] It's not clear by what name the subjects of Greig's trials would have known Trypan Red; Ehrlich was not concerned about coordinating with them.

The disparate names that Ehrlich used to coordinate Shiga's, Weinberg's and Greig's research eventually converged on the name by which the therapeutic is remembered: Trypan Red. This name could only have emerged after the drug left Ehrlich's Institute. After all, Ehrlich would have hardly thought to add the preface "Trypan" to the name "Red" in correspondence with researchers working exclusively on trypanosome chemotherapy. But as it circulated more broadly, the name of the compound instantiated the success of Ehrlich's vision for "color therapy." Indeed, it recalled the trade name of the blockbuster dye that first brought the benzopurpurin series to prominence, "Congo Red."[80]

A few weeks before Weinberg sent him the first batch of "benzopurpurin 8," Ehrlich wrote to the chemist Heinrich Bechhold, "I am still convinced that the pursuit of the dye area

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[78] Ehrlich also gave the name for a second dye that he had sent, Traga Red, but he wrote Greig that this dye would probably be less effective than Trypan Red. Ehrlich did not discuss Traga Red in subsequent letters.

[79] Neill, *Networks in Tropical Medicine*, 180.

[80] On "Congo Red" and the European imperial imagination, see D. P. Steensma, "Congo Red: Out of Africa?" *Archives of Pathology & Laboratory Medicine* 125, no. 2 (2001): 250–252.

from a medicinal point of view is not only good, but is absolutely necessary for the development of modern pharmacology. This is the only way to obtain knowledge on the relationships between constitution, action and distribution, which in my view is the heart of the matter."[81] For Ehrlich, in other words, dyes were both *tracers* and *agents* of pharmaceutical action.

Analogously, chemical names are both tracers and agents of *historical* action. The different names that Ehrlich used to identify drugs-in-them-making also make visible the communities and processes of communication and collaboration that constituted Ehrlich's chemotherapeutic research. Furthermore, these names also played an active role in the making of drugs, as the ever-shifting contents of a verbal toolkit that Ehrlich developed and drew upon to coordinate his heterogeneous research enterprise.

Aspirin

From the Geneva Congress on, most chemists saw the primary virtue of systematic nomenclature as the precise correspondence between chemical name, structural formula, and the identity of a chemical substance. To the same chemists, the primary drawback of these names was that they were often unintelligible. For chemical firms with fortunes staked on a particular pharmaceutical product, however, the polarity of these qualities of systematic nomenclature was inverted. A name that precisely indicated the structure of a compound suggested that no particular manufacturer's version was different from any other's. Pharmaceutical firms therefore rested their hopes for maintaining market share against competitors on the indecipherability of systematic chemical names, and on promoting a memorable, trademark-protected alternative: a trade name.

[81] Ehrlich to Bechhold, 21 Feb 1903, Paul Ehrlich papers, Series 5 – Copy Book X: Direktor Ehrlich, Box 88, 356–357.

One of the best known and most jealously protected trademarks of all time is a trade name of this sort: "aspirin." The story of aspirin is well-told.[82] In 1897, Felix Hoffmann, a chemist working for the pharmaceutical firm Bayer, synthesized a compound that he referred to as acetylsalicylic acid, and that showed remarkable promise as an analgesic and antipyretic with minimal side effects. The following year, Bayer brought the resulting drug to market as Aspirin. (Bayer brought another momentous pharmaceutical product synthesized by Hoffmann to market in 1898: diacetylmorphine, or "Heroin.")

Hoffmann had not been the first to synthesize acetylsalicylic acid, and through Bayer filed for a patent on the product, the application was either rejected or overturned on review in many countries, including Germany and Britain.[83] The patent was approved in the US, however, and in 1903, Bayer executives Friedrich Bayer (son of the firm's founder) and Carl Duisberg (research director and future chairman) visited America. The primary aim of their trip was to acquire a facility to manufacture aspirin and other drugs in the US, in order to avoid the steep customs duties on imported drugs.[84] However, they also took the opportunity to meet with the firm's US representative Hugo Schweitzer to discuss further steps to protect Bayer products on the American market.

In a meeting in New York in April of 1903, they developed an approach to intellectual property that would become the core of Bayer's marketing and legal strategy in America.[85] The minutes of the meeting reveal the formation of a corporate strategy that hinged on names. The general assumption in American patent law, noted the executives, was that the protection of a

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[82] Diarmuid Jeffreys, *Aspirin: The Remarkable Story of a Wonder Drug* (New York: Bloomsbury, 2004); Charles C. Mann and Mark L. Plummer, *The Aspirin Wars: Money, Medicine, and 100 Years of Rampant Competition* (New York: Knopf, 1991); Gabriel, *Medical Monopoly*, 198–229; Steen, *The American Synthetic Organic Chemicals Industry*.

[83] Jeffreys, *Aspirin*, 77–80, 84–91.

[84] Mann and Plummer, *The Aspirin Wars*, 16–17.

[85] "Protokoll ueber die pharmaceutische Konferenz," 1 Apr 1903, Bayer Corporate Archives, Leverkusen, Germany, 169/3. (The date and location of this meeting are noted in pencil on the typescript, based on a reference to 1 April as "now" within the text of the typescript.)

name was tied to the protection of the substance, and that the two expired together. "However," the minutes went on, "this perspective is based on various decisions that have no direct bearing on patents and name protection in relation to chemical products."[86] For such products, the Bayer directors determined, there was no decisive precedent; the firm would therefore pursue litigation designed "to bring about a decision that makes the name of a chemical product independent of patent protection." They acknowledged that this strategy would be unlikely to work when the same name appeared in the patent text and on the market, as in the case of another Bayer analgesic, Phenacetin. The shared name linked the patented substance to the brand in a bond that was likely too tight for Bayer's lawyers to sever.

In the future, therefore, the executives decided that the firm should scrupulously employ systematic chemical names in the text of American patents. That way, "this chemical name will be the trade name [Handelsbezeichnung] under which the product can eventually be brought onto the market [i.e., by competitors] after the expiration of the patent, while after the expiration of the patent, it will still be the case that this product can be marketed under the protected name by the earlier patent holder alone, because this name is simply an alias for the manufacturer."[87]

[86] Ibid., 1. Emphasis in original.

[87] Ibid. On the history of non-proprietary names and generic drugs, see Jeremy A. Greene, *Generic: The Unbranding of Modern Medicine* (Baltimore: Johns Hopkins University Press, 2014).

Source	Section	Name(s)
Subject Index to the *Journal of the Chemical Society,* 1893-1902 (2v., vol. 1, 822p.)	p. 30 p. 47 p. 180	*Acetylsalicylic acid* *o-Acetoxybenzoic acid* *Aspirin*
Subject Index to the *Journal of the Chemical Society,* 1893-1902 (2v., vol. 2, 898p.)	p. 1417 (under *salicylic acid (o-hydroxybenzoic acid)*, pp. 1416-1419)	*acetyl derivative: see aspirin*
Lexicon der Kohlenstoff Verbindungen, 2nd ed., 2v., vol. 1 (1900), 1264p.	p. 649 (entry 14 of 43 under the formula $C_9H_8O_4$, pp. 648-650)	*2-acetoxylbenzol-1-carbonsäure*
Handbuch der Organischen Chemie, 3rd ed., 4v., vol. 2 (1896), 2210p.	p. 1496 (within salicylic acid derivatives, pp. 1488-1516)	*Essigsalicylsäure*
Handbuch der Organischen Chemie, supp. to 3rd ed., 5v., vol. 2 (1903), 1251p.	p. 889 (within salicylic acid derivatives, pp. 885-902)	*Essigsalicylsäure, Acetsalicylsäure, o-acetoxybenzoësäure ("Aspirin")*
Erdmann, *Berichte der Deutschen Chemischen Gesellschaft* vol. 32 (1899).	(Cited in *Handbuch*, 3rd ed. vol. 2)	*acetsalicylsäure*
Gerhardt, *Liebigs Annalen* vol. 87 (1853), p. 162.	(Cited in Erdmann 1899)	*wasserfreie salicylsäure-essigsäure*

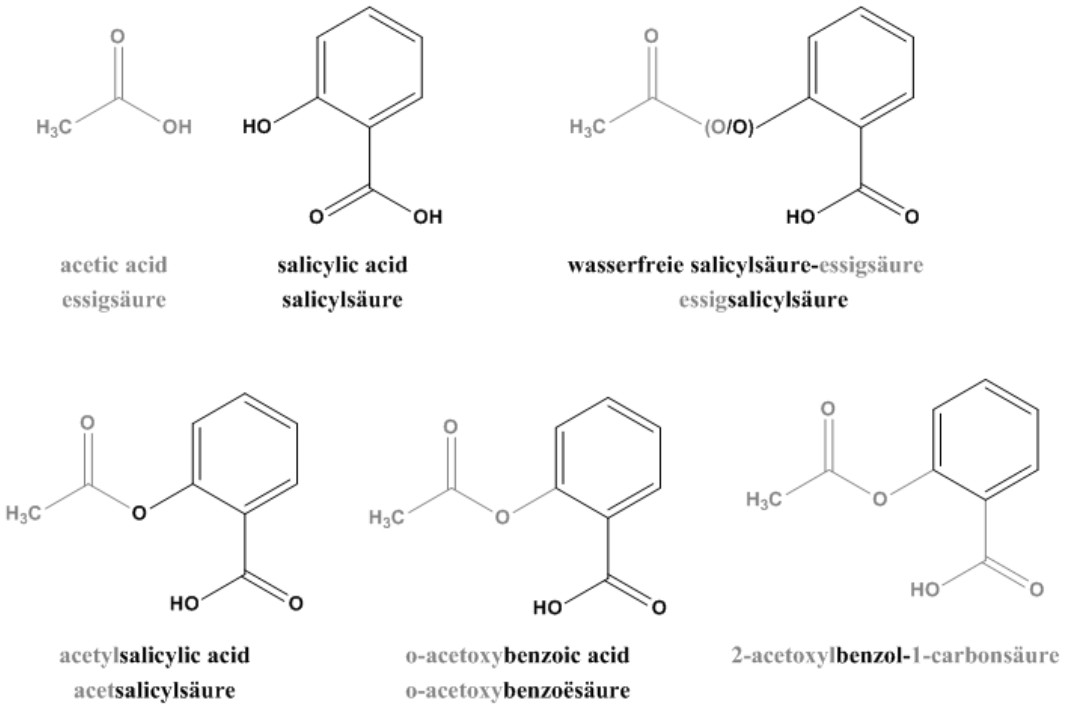

Figure 4. Alternative rational names for acetylsaclicylic acid (aspirin), circa 1900.

The many different systematic names for Bayer's pharmaceutical crown jewel (*Fig. 4*) made the name Aspirin an even more powerful piece of intellectual property. Indeed, Patterson and Crane singled it out as an exception to the general rule that chemical indexes avoided using trade names; the use of "aspirin" as an index term was, they wrote, "justified on the principle that entries in an index should be made where the most users may be expected to look first."[88] Eventually, this proved the firm's undoing; in 1918, a US court rules that aspirin (with a lower-case "a") had entered into sufficiently generic use to be in the public domain.[89] Names, usage, and law could combine to make a new sort of object, a branded drug; they could also unmake it.

Vitamin C

Chemical industry executives were interested in what names didn't do – trade names that didn't express structure; systematic names that weren't intelligible – for reasons of intellectual property and profit. Physicians, too, were interested in such negative qualities of names, but for a different purpose. American Medical Association committee members wanted to protect patients – and to safeguard the authority of physicians – by making sure that the names of drugs did not imply particular therapeutic applications.

In the fall of 1937, the delightfully-named Dr. Paul Nichols Leech, secretary of the AMA Council on Pharmacy and Chemistry, wrote to the secretary of the American Chemical Society asking for advice in selecting a nonproprietary name for Vitamin C, which the pharmaceutical firm Merck had recently brought to market. The commonly-used trivial name "ascorbic acid" was out, he explained: "One of the cardinal principles of the Council on Pharmacy and Chemistry, since its formation in 1905, is that therapeutically suggestive names are not

[88] Crane and Patterson, *A Guide to the Literature of Chemistry*, 184.

[89] The capitalized term "Aspirin" remained the intellectual property of Bayer, all of which, however, had just been seized by the US government under the wartime Trading with the Enemy Act; Steen, *The American Synthetic Chemicals Industry*, 138–171.

acceptable." As a contraction of the adjective "anti-scorbutic," to name the product name ascorbic acid "suggests its use against scurvy," on these grounds, "the Council was forced to the decision that the name was not acceptable."

Dr. Leech acknowledged that, as suggestions of therapeutic efficacy went, the link between "ascorbic" and "scurvy" was pretty tenuous, and that the risks of therapeutic suggestiveness in this case were rather minor, since the substance wasn't known to cause harm. Nevertheless, he insisted that accepting the name would set a problematic precedent "which would be embarrassing to the Council in further decisions on therapeutically suggestive names for drug products." And so, he reported, after consultation with Merck, "the Council adopted the term 'Cevitamic acid' as a nonproprietary designation." However, Leech had been told that chemists found this name unsatisfactory for myriad reasons, including for its perpetuation of the chemically-meaningless category of the "vitamin."

This was why Leech, on behalf of the AMA Council, was writing to the ACS for help figuring out a better way to deal with Vitamin C. They had no interest or desire, Leech hastened to add, to get involved in matters of chemical nomenclature more generally: "This problem has arisen because the function of the Council makes it necessary to consider the nomenclature of products having promise of therapeutic merit. The Council is not concerned primarily with the nomenclature of chemical or biological preparations which do not have therapeutic merit."[90]

E. J. Crane, the chair of the ACS Committee on Nomenclature, Pronunciation, and Spelling and longtime head of *Chemical Abstracts*, wrote back to Leech with a few comments based on his two decades of experience in matters of nomenclature. In pragmatic midwestern prose, the Ohioan gently explained the pitfalls of treating naming as a matter of universal principles. "In the conduct of our work here we have a number of basic rules but we go on the

[90] Leech to Parsons, 13 Sept 1937, W. A. Noyes papers, Box 14, Folder "General Correspondence, 1937–38."

theory that rules are sometimes to be ruled instead of always to apply. Perhaps I should say that rules are sometimes considered here to be overruled by circumstances."

"I am prompted to take this stance," Crane went on "chiefly because I know by experience how bad it is to have in scientific literature more than one name for a substance.... I am afraid that ascorbic acid is so well established now it is going to continue to be used rather extensively no matter what any of us interested in chemical nomenclature may attempt to do."[91]

The AMA Council finally agreed to accept ascorbic acid.[92] Crane and his colleagues were relieved. They had their hands full as it was trying to curb synonymy within the chemical sciences without having to deal with other organizations attempting to coin new names and to forbid the ones that *Chemical Abstracts* had established as canonical.

But Leech did not want to set a precedent, nor to cede the ground of naming pharmaceuticals to the chemists and their values. In his capitulation on the matter of ascorbic acid, Leech also requested that "in the future the American Chemical Society will not recognize therapeutically suggestive names for products coming within the purview of the Council on Pharmacy and Chemistry and that the Council shall be the body to determine whether a name is therapeutically suggestive according to its rules."[93]

Crane and his committee responded by expressing a lukewarm "desire to cooperate." The chemical index-makers among them had learned through hard experience that they couldn't even bind themselves to their own rules in all cases. They certainly weren't going to bend over backwards to accommodate absolute rules of nomenclature imposed by another

[91] Crane to Leech, 26 Oct 1937, W.A. Noyes papers, Box 14, Folder "General Correspondence, 1937–38."
[92] Leech to Crane, 27 Apr 1938, W.A. Noyes papers, Box 14, Folder "General Correspondence, 1937–38."
[93] Ibid.

body. All in all, Crane judged in bringing the affair to a conclusion, "authority is a thing which can be, and is, taken too seriously at times."[94]

For Crane, as longtime editor of *Chemical Abstracts*, maintaining an effective system of chemical nomenclature was of utmost importance to the institution to which he'd dedicated his working life. Nevertheless, he cautioned against taking authority over nomenclature too seriously. The AMA Council on Pharmacy and Chemistry actively avoided dealing with chemical nomenclature whenever possible, and yet they tried to impose their rules for the nomenclature of therapeutics upon Crane and the chemical reference works that he oversaw.

Committees of physicians and of chemists thought about names quite differently. To Leech, the *absence* of undesirable connotations was of paramount importance. To Crane, the manner in which the name fit into a nomenclature system spanning all of organic chemistry and its literature was what mattered. In each regime, *rules* had a different status. For the AMA Council, rules were absolute. In contrast, the experience of chemical nomenclature reform had taught Crane that nomenclature rules were made to be broken, and that it was the exceptions that held the whole system together.

Conclusion

Over the decades following the Geneva Congress, chemists sought to adopt and adapt systematic organic chemical nomenclature in a wide range of areas of chemical practice. One effect of these encounters was an incrementally closer alignment of chemical education, patent records, and chemical products with the global chemical order expressed in systematic names and reference works. At least as significant, however, were the fruits of the misalignment of systematic nomenclature with the demands of these fields: ways of teaching *by means of*

[94] Crane to Members of the Committee on Nomenclature, Spelling, and Pronunciation, 16 Mar 1938, W.A. Noyes papers, Box 14, Folder "General Correspondence, 1937–38."

conflicting methods of nomenclature, a new category of chemical entities generated by the mismatch between systematic nomenclature and patent law, and names that distinguished pharmaceutical objects from their chemical substrata. Like sparks discharging across an epistemic gap, these objects and methods emerged at the disjunction of systematic names and other forms of order for chemical pedagogy, patents, and products.

Over the same period, similar conjunctions and disjunctions arose within the *social* order of the national and international institutions that took up nomenclature reform. The following two chapters will turn to the ways in which chemists took up the organization of chemicals as an occasion for organizing and reorganizing international chemistry, under radically different political circumstances, before, during, and after World War I.

Chapter 5

Nomenclature and Reference Works in War and Peace, 1910-1923

In 1910, Albin Haller was chair of organic chemistry at the Sorbonne, president of the Société Chimique de France, and a leading proponent of the application of chemistry to the advancement of French industry. In all of these particulars, Haller was in just the position that his predecessor at the Sorbonne and fellow Alsatian Charles Friedel had occupied in 1888. And like Friedel in 1888, Haller capitalized on his standing to launch an international effort to reform the nomenclature of organic chemistry.[1] However, what it meant to engage in such collaboration had changed since the Geneva Congress. After two decades of national and international institution-building, Haller was not interested in planning a singular congress focused on nomenclature alone. Rather, in concert with British and German colleagues, he incorporated the standardization of organic chemical nomenclature into a plan for an ongoing program to promote coordination and cooperation in chemistry within a new International Association of Chemical Societies (IACS).

The German chemical lexicographer Paul Jacobson might have appeared to be in the same position in 1910 as Friedrich Beilstein had been two decades before: editor-in-chief of *Beilsteins Handbuch der Organischen Chemie* (*Beilstein*). But Jacobson had resources that Beilstein could only have dreamed of – institutional support, a salary, and a high professional standing, all thanks to his official position within the Deutsche Chemische Gesellschaft. He had a staff of editors and indexers working under him and plentiful funding from the German chemical industry to carry out the mammoth task of entirely recompiling the reference work

[1] On Haller, see Claude Viel, "Albin Haller," in Laurence Lestel, ed., *Itinéraires de Chimistes 1857-2007, 150 Ans de Chimie en France avec les Présidents de la SFC* (Paris: SFC, 2007), 245–250.

that Beilstein had founded. Chemical nomenclature played a crucial role in this undertaking, and from the association's founding meeting, Jacobson made sure that he held the reins of IACS work on organic nomenclature.[2]

However, the outbreak of war in 1914 brought an abrupt end to the fledgling international association. In Paris, Haller set aside his role as international institution-builder and donned the mantle of patriotic state servant. His former colleagues within IACS did the same. In the shadow of explosives and chemical weapons, nomenclature and compilation were forgotten – but only briefly. In the latter years of the war, Haller took on a leading role in building new institutions to govern international chemistry during peacetime, or rather during the economic and intellectual conflict that many anticipated would carry on after the guns fell silent. Chemists in France, Britain, and America searching for the secret to the productivity and prestige of German chemistry seized upon the dominant position of *Beilstein* and other German chemical compilations. The key to challenging the might of German chemical science and industry, the Allied chemists decided, was creating new English and French versions of these authoritative reference works. Through the efforts of Haller and others, the International Union of Pure and Applied Chemistry (IUPAC) was created to unite the chemical world – excluding Germany and its allies – in such projects.

Today, IUPAC and systematic organic chemical nomenclature are closely associated with each other. Although only one of IUPAC's eight divisions deals with naming standards, nomenclature is often taken as the Union's primary contribution to chemistry, even by the Union's own officers.[3] Conversely, although the institutional descendants of *Beilstein* and

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[2] On Jacobson's background, see Chapter 3.

[3] For example, in his foreword to a history of IUPAC, then-President Kirill Zamarev noted, "Particularly appreciated by the international community of chemists are the contributions of IUPAC to the creation of self-consistent nomenclature rules in various fields of chemistry." Roger Fennell, *History of IUPAC, 1919-1987* (Boston: Blackwell Science, 1994), xv.

Chemical Abstracts continue to rely on their own systems of nomenclature and notation, IUPAC is taken as the arbiter of the official nomenclature of organic chemistry.[4] This is not unique to IUPAC or chemistry. Numerous historians have distinguished a "utopian" scientific internationalism focused on the collective advancement of knowledge and international peace from a "pragmatic" internationalism focused on the standardization of nomenclature and terminology. By most metrics, the lion's share of the accomplishments of international scientific unions and congresses in the nineteenth and twentieth centuries have taken the latter form.[5] There is a negative explanation for this trend – nomenclature wasn't "utopian" – but it merits a positive account as well. Standardization projects like nomenclature reform and the international institutions that undertake them are not linked by necessity, but by historical entanglements through which they have shaped each other.

The politics of scientific internationalism could be adversarial as well as utopian. Wartime organizations supporting inter-Allied technical cooperation provided the institutional framework for the emergence of IUPAC at the war's conclusion. IUPAC likewise inherited assumptions formed during the war about the practical importance of chemical reference works, on the one hand, and the trifling intellectual value of such work as the construction of nomenclature systems, on the other. These conditions made for a challenging start to IUPAC work on organic nomenclature. This work finally took form through the intervention of American chemist W.A. Noyes, who saw the standardization of organic nomenclature as means

[4] In settings high and low; see, e.g., Caroline Cooper, "Nomenclature Fundamentals," in *Organic Chemist's Desk Reference*, 2nd ed. (Boca Raton: CRC Press, 2010), 41; Arthur Winter, *Organic Chemistry I For Dummies,* 2nd ed. (Hoboken, NJ: Wiley, 2014), 136.

[5] Anne Rasmussen, "Jalons pour une histoire des congrès internationaux au XIXe siècle: Régulation scientifique et propagande intellectuelle," *Relations internationales* 62 (1990): 115–133. See also Brigitte Schroeder-Gudehus, "Les congrès scientifiques et la politique de cooperation internationale des académies des sciences," *Relations internationales* 62 (1990): 135–148. On shifting modes and conceptions of scientific internationalism, see Geert J. Somsen, "A History of Universalism: Conceptions of the Internationality of Science from the Enlightenment to the Cold War," *Minerva* 46, no. 3 (September 1, 2008): 361–379; Robert Fox, *Science without Frontiers: Cosmopolitanism and National Identity in the World of Learning, 1870-1940* (Corvallis, OR: Oregon State University Press, forthcoming 2016).

toward an end diametrically opposed to that for which IUPAC had been established: bringing Germany back into international chemistry.

Throughout this story, those involved in nomenclature reform articulated a shifting series of answers to the questions of how it should be organized and carried out. What were nomenclature rules supposed to accomplish? Who was qualified to take part in making them? What kinds of institutions were appropriate for developing and enforcing them? Uncertainty about the order of chemical knowledge was also uncertainty about the fate of chemical science and technology during the years before, during, and after the Chemists' War.

"La Science est sans patrie!"

Albin Haller conceived the idea for forming an international association of chemical societies during a meeting in Switzerland in 1910, in conversation with the German chemist Wilhelm Ostwald. Though the founding father of physical chemistry had turned down his invitation to the Geneva Congress, by the 1900s, Ostwald had become a leading proponent of efforts to organize, standardize, and unify diverse elements of intellectual exchange. He had, for example, donated one portion of the prize money from his 1909 Nobel Prize in Chemistry to Die Brücke, an association aiming to organize and standardize every aspect of intellectual work, from the physical size of publications to how they were indexed. He donated another portion to a journal promoting the international constructed language Ido.[6]

Several new forums for international cooperation and standards-making focused specifically on chemistry had emerged over the two decades following the 1889 Paris Congress of Chemists and the 1892 Geneva Nomenclature Congress. In 1894, the Belgian Association of

[6] Brigitte Van Tiggelen and Danielle Fauque, "The Formation of the International Association of Chemical Societies," *Chemistry International* 34, no. 1 (2012): 9–11; Thomas Hapke, "Wilhelm Ostwald, the 'Brücke' (Bridge), and connections to other bibliographic activities at the beginning of the twentieth century," in *Proceedings of the 1998 Conference on the History and Heritage of Science Information Systems*, ed. Mary Ellen Bowden, Trudi Bellardo Hahn, and Robert Virgil Williams (Medford, NJ: Information Today, 1999), 139–147. On Ostwald's activities in constructed languages, see Michael D. Gordin, *Scientific Babel* (Chicago: University of Chicago Press, 2015), 131–158.

Chemists organized a Congress of Applied Chemistry to pursue the unification of analytical techniques for alimentary, agricultural, and pharmaceutical chemistry.[7] Six more such congresses followed through 1909, each organized by a national society of analytical or industrial chemistry, and often held in conjunction with a universal exhibition. As time went on, the primary aim of these Congresses shifted from standardizing chemical techniques to standardizing applied chemists. Drawing ever larger audiences and ramifying into more and more niches of chemical practice – more than 4,000 chemists attended the seventeen sections and subsections of the 1909 London Congress – the meetings highlighted and advanced the emerging professionalization and specialization of industrial chemistry, especially in each congress's home country.[8]

Other bodies focused on gathering chemical data and sifting through it to compile standard tables of chemical constants. The International Committee on Atomic Weights, the brainchild of Ostwald and two German colleagues in 1899, established reference standards for atomic weights and published an annually updated critical list of weights for known elements, replacing the various tables previously compiled and published by national societies.[9] The International Committee on Annual Tables of Constants and Numerical Data, founded by French chemist Charles Marie in 1909, brought together in a single yearly volume all such data

[7] Sugar analysis was of paramount interest, particularly in the first few of these congresses. On the history of technologies and standards for sugar analysis, see David Singerman, "Inventing Purity in the Atlantic Sugar World, 1860-1930" (PhD dissertation, MIT, 2014).

[8] D. Thorburn Burns and H. Deelstra, "The Origins and Impact of the International Congresses of Applied Chemistry, 1894–1912," *Chemistry International* 33, no. 4 (2011): 11–14.

[9] Norman E. Holden, "Atomic Weights and the International Committee —A Historical Review," *Chemistry International* 26, no. 1 (2004): 4–7. An influential early decision of the committee was its 1905 ruling that weights would be calculated on the O=16 standard rather than the H=1 standard; F. W. Clarke et al., "Report of the International Committee on Atomic Weights," *Journal of the Chemical Society (Abstracts)* 88 (1905): i–vii. (Since 1961, the standard has been $C^{12} = 12$.)

published in each year's physics and chemistry literature (or so the group's circulars claimed).[10]

Haller and Ostwald's association was to be a different sort of body. In April 1911, Haller, Ostwald, and six other chemists representing the chemical societies of Britain, France, and Germany met in Paris to lay the foundation of the International Association of Chemical Societies (IACS). In contrast to the sprawling Congresses of Applied Chemistry ("noisy and confusing" affairs, as the eminent German chemist Emil Fischer recalled), they envisioned an ongoing enterprise bringing together a small number of representatives of the upper echelons of international chemistry.[11] Its member organizations would be national chemical societies – organizations first and foremost representing the interests of academic research chemists – each of which would send three delegates to the international association.[12] Only one society was permitted to represent each nation; to be eligible to do so, a society had to control a journal. In this way, IACS limited its membership to leading societies and ensured that its decisions could be effectively publicized. A president would be elected each year from among the delegates, and, to facilitate the smooth progress of Association business, the offices of vice-president and general secretary would be filled by the other two delegates from the president's chemical society. IACS was built on the assumption that achieving consensus among the international chemical elite was more a matter of organization than of politics.[13]

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[10] Fennell, *History of IUPAC, 1919-1987*, 2–3; "Messrs. J. and A. Churchill Announce the Following Works for Early Publication," *Nature* 88, no. 2202 (1912): 357. The physicist Hans Heinrich Landolt and the chemist Richard Börnstein had compiled a German version of such tables since 1883.

[11] Emil Fischer, *Aus meinem Leben* (Berlin: Springer, 1922), 135.

[12] On the establishment of European national chemical societies focused on academic and on industrial chemistry in this period, see Anita Kildebæk Nielsen and Soňa Štrbáňová, eds., *Creating Networks in Chemistry: The Founding and Early History of Chemical Societies in Europe* (Cambridge: Royal Society of Chemistry, 2008).

[13] On the formation and brief history of IACS, see Van Tiggelen and Fauque, "The Formation of the International Association of Chemical Societies"; Fennell, *History of IUPAC, 1919-1987*, 3–10; William Ramsay, "The International Association of Chemical Societies," *Nature* 92, no. 2303 (1913): 453–454. On the work of the IACS commission on the nomenclature of organic chemistry, see Pieter Eduard Verkade, *A History of the Nomenclature of Organic Chemistry*, trans. S. G. Davies (Boston: Reidel, 1985), 75–87.

Under the guiding hand of Ostwald, elected president for 1911-12, national chemical societies were invited to apply for membership. By the association's next meeting in April 1912 in Berlin, chemical societies from Austria, Denmark, Italy, Japan, the Netherlands, Russia, Spain, Sweden, Switzerland, and the US had joined IACS.[14] Of the delegates appointed by these new member societies, only the Swiss and Russians attended, along with one of the three US delegates, the chemist and editor William A. Noyes.[15] Nevertheless, the leaders of the association felt that it was off to a strong start, both as a center for the international coordination of chemistry and as a sign of international goodwill among chemists. The Briton William Ramsay, second president of IACS, concluded his report on the association in *Nature* with an enthusiastic (mis)quotation of a French maxim: "*La science est sans patrie!*"[16]

Nomenclature was central to the mission of IACS. Haller's initial letter proposing the formation of such an association to the Chemical Society of London described it as "an International Committee to consider questions of chemical nomenclature and other matters."[17] All three French delegates, Haller, Hanriot, and Béhal, had attended the Geneva Congress. Gautier and Maquenne, both former members of Friedel's Subcommission, joined them on the committee charged with drawing up an initial list of activities for IACS, making the majority of this committee veterans of Friedel's nomenclature reform effort. Accordingly, the nomenclature of inorganic and organic chemistry was the first item on this list. Nomenclature was also the only topic on which the Association took concrete action at its initial meeting,

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[14] *Berichte über die beiden ersten zu Paris 1911 und zu Berlin 1912 veranstalteten Tagungen d. Conseil de l'Association Internationale des Sociétés Chimiques* (Berlin: Die Brücke, 1912), 3–9.

[15] *Berichte über die beiden ersten … Tagungen*, 46; "International Association of Chemical Societies," *Proceedings of the American Chemical Society* (1912): 92–96.

[16] Ramsay, "The International Association of Chemical Societies," 454. Ramsay presumably meant to quote the familiar saying "La science n'a pas de patrie."

[17] Chemical Society Council Minutes of 15 Dec 1910, RSC, AR0146.

forming international commissions on the nomenclature of organic chemistry, the nomenclature of inorganic chemistry, and the unification of notation for physical constants.[18]

The organic nomenclature commission, like the two others, was made up of one delegate from each member society. Each member society also formed a national commission on organic nomenclature, which was to submit a report recommending how the international commission should organize its work.[19] Though these national commissions agreed on the importance of the reform of organic nomenclature, they had different ideas of how it ought to be carried out. The French commission felt that the commission should build upon and extend the Geneva rules, especially to cover the ring-containing compounds whose challenging nomenclature the Geneva Congress had been unable to address.[20] Two members of the French commission, Auguste Béhal and Victor Grignard, took it upon themselves to begin this process in nomenclature proposals that they published in early 1912.[21]

The British commission advocated carrying forward the official, index-oriented focus of the Geneva Congress as well. To help secure broad acceptance and authority for whatever rules the IACS commission eventually decided upon, the British commission proposed that these rules be subject to approval by the council of each member society. Once approved, it called for member societies to apply the decisions of the international commission in their journal indexes, and for the commission's rules to constitute "the official nomenclature of Organic

[18] *Berichte über die beiden ersten ... Tagungen*, 4, 6, 9.

[19] Ibid., 9, 20.

[20] Ibid., 41.

[21] Auguste Béhal, "Propositions pour la nomenclature des chaînes fermées hétérogènes et extensions aux différentes chaînes fermées et à la nomenclature des chaînes acycliques," *Bulletin de la Société Chimique de France* 11 (1912): 264–75; Victor Grignard, "Sur la nomenclature des systèmes hydrocarbonés polycycliques," *Bulletin de la Société Chimique de France* 11 (1912): 124–129.

Chemistry employed in reference books and dictionaries, such as Beilstein's Handbuch and Richter's Lexikon."[22]

However, this was precisely the approach to systematic nomenclature that Jacobson had rejected in the 1898 article in which he and his fellow Deutsche Chemische Gesellschaft (DChG) editor Richard Stelzner renounced the use of the Geneva nomenclature.[23] Perhaps anticipating this push to resume a Geneva-style approach to nomenclature reform, Jacobson had presented a formal set of "Theses concerning the nomenclature of organic compounds" at the organizational meeting of IACS in 1911. In these theses and in the subsequent report of the German organic nomenclature commission, Jacobson advocated giving up on "a thoroughgoing unification of nomenclature and numbering" in favor of preserving "the historical character of our nomenclature."[24] Accordingly, he felt that the committee should restrict itself to offering recommendations when existing naming practices came into conflict, testing proposals for dealing with new compounds and challenges for their applicability in different languages, proposing means of simplifying names, where necessary, and reminding journal editors of their responsibility to check the nomenclature of their manuscripts.

In short, the French and British commission argued (or perhaps just assumed) that the IACS organic nomenclature commission should serve a *legislative* function. According to this perspective, the IACS organic nomenclature commission was to build up a comprehensive system of nomenclature rules, as the Geneva Congress had done two decades before. Once approved, these rules were to be considered binding for the publications of each member

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[22] Chemical Society Council Minutes, "Report on Organic Nomenclature," 11 Feb 1912, RSC, AR0147. Non-italicized titles are sic.

[23] See Chapter 3.

[24] *Berichte über die beiden ersten ... Tagungen*, 11–12, 40. The IACS minutes contain a French translation of Jacobson's theses. There is an English translation in Verkade, *A History*, 76–77. The original German text was published in Bernhard Prager, "Zur Nomenklatur der organischen Chemie," *Angewandte Chemie* 42, no. 45 (1929): 1058.

society – a set that included nearly all of the best-known chemical journals and reference works.

Jacobson, in contrast, advocated a *judicial* role for the IACS nomenclature commission. In his view, the commission should not create new rules, but only evaluate nomenclature proposals presented by individual chemists and render verdicts when conflicts among established practices of nomenclature led to confusion. Jacobson had just stepped down as General Secretary of the DChG to lead the organization's newly-created Division of Reference Literature (*Abteilung der Sammelliteratur*, literally Division of Compiled Literature).[25] In this role, his full-time job was to supervise enormous compilation projects, especially Stelzner's supplements to Richter's *Lexikon* and the fourth edition of *Beilstein*. As discussed in Chapter 3, this was an immense endeavor, involving new systems of chemical classification, an enormous amount of editorial labor, and the creation of an institution with the resources and structure to carry on chemical compilation as an ongoing process rather than a series of heroic one-off efforts.

In all of the activities of Jacobson and his colleagues, nomenclature played a crucial role. The making and use of these works depended on clear, unambiguous (though not necessarily systematic) chemical names and notation. Any new international nomenclature rules that conflicted with the chemical names used in *Beilstein* and its sister texts could undermine the usefulness and authority of these reference works. If the IACS commission could be convinced to take a judicial rather than a legislative function – adjudicating specific points of confusion, rather than imposing general rules – the commission would pose less of a threat to Jacobson's enterprise while still helping to stem the proliferation of chemical synonyms, one of the principal obstacles to chemical compilation.

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[25] "Paul Jacobson," in Dagmar Drüll, *Heidelberger Gelehrtenlexikon 1803-1932*. 2nd ed., in preparation.

As other national societies joined IACS in 1912 and 1913, their proposals revealed further divergence not only over *how* nomenclature reform should be carried out, but also over *who* should carry it out.[26] Jacobson recommended that only chemists with the editorial experience to anticipate the consequences of new proposals be appointed members of the international commission. He and his fellow German commission members further advocated that the international commission be composed of at most ten members, all residing in Europe, in order to promote its efficient operation."[27] The Americans did not like the idea of limiting the commission to Europeans, and pushed for the inclusion of at least one representative from the American Chemical Society (ACS) as well as the German, French, British, and Italian chemical societies. The Danish thought there was no need for all countries to be represented on the commission as long as it included English, German, and French-speaking delegates. However, they were adamant that commission members publish all of their proposals collectively, not as individuals.[28] The Swiss called for all of the national societies in IACS to be represented in the international commission; they also advocated holding another Geneva-style congress to begin the work of the commission.[29]

There were disagreements within national delegations as well. Jacobson was eager to have the commission's form settled so that it could get to work, and at the 1912 Berlin meeting, he reiterated his proposal for establishing a commission of no more than ten members. However, Ostwald – IACS President to Jacobson's IACS General Secretary – was against

[26] On the former question, the Dutch and Danish insisted that the commission find a way to maintain both the Geneva names and the trivial names then in use. The Austrians similarly called on the commission to put the Geneva nomenclature into practice but at the same time to keep established trivial names. The Japanese delegation intriguingly recommended focusing on general principles – "lines for development" – rather than rule-making, perhaps considering general principles more suitable for adaptation into a language as distinct from French, English, and German as Japanese. Czech delegate Emile Votoček summarized these reports after the war; Emile Votoček, "La réforme de la nomenclature," IUPAC Comptes Rendus 2 (1921): 80–81.

[27] The members of the national commissions are given in *Berichte über die beiden ersten … Tagungen*, 36–39; summaries of the organic nomenclature reports are on 40–42.

[28] Votoček, "La Réforme de la Nomenclature."

[29] Verkade judged this proposal "crazy"; Verkade, *A History*, 77.

immediately centralizing the association's nomenclature efforts in such a commission, preferring instead once again to solicit advice from the various national delegations to promote lively discussion within and among them. Haller brokered a compromise between the two Germans, dictating that the separate national commissions develop their proposals for one more year, at which point a central nomenclature commission would begin its work.[30]

A year later, the IACS commissions on inorganic nomenclature and on the unification of physico-chemical symbols were presenting substantive recommendations, but the association's council was still trying to figure out who was to make up its organic nomenclature commission and what work that commission was to undertake. A committee established for this purpose decided to follow the recommendation of the Swiss delegation and to appoint an International Commission on Organic Nomenclature made up of one delegate from each national chemical society in the association. The decision appeared to undercut Jacobson's plans to create a small commission of experts. However, a subsequent decision reinforced the German editor's authority. This commission was to appoint a five-member working group – not necessarily commission members themselves – to study the reports of the national committees and to propose how the international commission should organize and carry out its work. For this working group, the commission selected the prominent Italian chemist Giacomo Ciamician, the Danish chemist Einar Biilman, and the French and English editors Edmond Blaise and J.C. Cain. As chair of this working group, the commission selected Jacobson.[31]

Notwithstanding the veneer of multilateral democracy, then, Jacobson had obtained just the sort of group that he wanted: a small collection of European editors entrusted with charting the course of IACS organic nomenclature reform. Working by correspondence over the

[30] *Berichte über die beiden ersten ... Tagungen*, 20–21, 23–24.

[31] "International Association of Chemical Societies. Extract from the Minutes of the Third Session of the Council," *Proceedings of the Chemical Society, London* 29, no. 420 (1913): 332–341.

following year, this group settled upon a list of principles in accord with Jacobson's theses from 1911. It would conserve the "historical character" of nomenclature. The working group would resist the temptation to simply extend the Geneva rules to address the most urgent problems of nomenclature – for example, the pressing question of how to name polycyclic compounds – but would instead address nomenclature at a general level. And the working group itself – not the larger commission – would take charge of carrying out the plans that it proposed.[32]

As in 1892, the publication schedule of *Beilstein* shaped the organization of nomenclature reform. The group proposed that its nomenclature reform work proceed systematically through the whole of organic chemistry. Like the Geneva Congress, it would begin with acyclic hydrocarbons, then address cyclic hydrocarbons, then fundamental heterocycles, then functionally complex compounds.[33] This precisely matched the order of the classifications that Bernard Prager, Dora Stern, and Paul Schmidt had developed for the fourth edition of *Beilstein*, and thus the order in which the volumes of the prodigious new edition would be compiled and published. The usefulness and authority of *Beilstein* and the other reference works whose compilation Jacobson supervised would depend crucially on the consistency of the chemical names they contained with general usage (or, in cases of conflict, the ability of these reference works to *determine* general usage). As head of the IACS organic nomenclature working group, Jacobson coordinated the making of international nomenclature guidelines with the work of his German editorial teams, and was well-positioned to head off potential conflicts before they emerged.

As Jacobson's committee prepared for the September 1914 meeting at which it would begin the first phase of its nomenclature reform plan, IACS was going strong. Haller followed

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[32] Blaise, the French member of the working group, summarized these decisions in a report a decade later; Edmond E. Blaise, "La réforme de la nomenclature de chimie organique," IUPAC Comptes Rendus 3 (1922): 77.

[33] The working group estimated that this effort would take six years; Ibid. On the specific nomenclature proposals mooted by this working group, see Verkade, *A History*, 81–86.

Ramsay as president, completing the orderly succession of leadership through the association's three founding societies. The Association had brought other international projects for chemical standards and documentation into its orbit.[34] Most importantly, after careful negotiations conducted by Haller, the Association received a substantial endowment from Ernst Solvay, the Belgian industrialist who had also recently founded his eponymous series of conferences in physics.[35] With this funding in place, the association offered to cover the expenses of Jacobson's working group.[36] The British delegates were even willing to entertain the idea of an IACS subsidy for Stelzner's *Literatur-Register*, one of the reference works whose compilation Jacobson supervised for the DChG.[37]

These preparations were underway when, on June 28, 1914, the radical Serbian nationalist Gavrilo Princip killed Franz Ferdinand, heir to the emperor of Austria, in Sarajevo. During July, the ensuing diplomatic crisis propagated through alliances and military contingency plans, and the European powers alternately inched toward a war footing. On August 4, German troops streamed into Belgium. The September meeting of IACS was cancelled, and by October, German and French scientists were lobbing accusations of moral turpitude and cultural and intellectual bankruptcy across the freshly-dug trenches. The American committee on organic nomenclature reported "inactivity on account of the chaotic

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[34] The Atomic Weights and Annual Tables committees discussed above were among the groups folded into IACS.

[35] On the condition that IACS take part in the governance of a new International Institute of Chemistry in Brussels and establish permanent headquarters there, Solvay presented the Association with an otherwise unconditional gift of 250,000 francs, plus an annual income of 37,500 francs through 1940. The entire 1911-12 budget of the Association, including an estimate for printing the 1912 reports, was only 1,213 marks. (Marks and Belgian francs were approximately at par during this period; 1913 exchange rates were 4.2 marks and 5.2 Belgian francs to the dollar, per http://www.measuringworth.com/). This annual income was equal to about half of the non-publication expenses in the American Chemical Society's annual budget for 1913 (approximately $15,000). The lump-sum gift was on the order of the annual membership dues collected by the ACS from its 4,700 members and of the annual cost of compiling and publishing *Chemical Abstracts*. *Proceedings of the American Chemical Society* (1913): 3, 8–9. On the first Solvay Conference, see Diana Kormos Barkan, "The Witches' Sabbath: The First International Solvay Congress in Physics," *Science in Context* 6, no. 1 (1993): 59–82.

[36] "Extract from the Minutes of the Third Session," 341.

[37] "Report of the Committee appointed to consider certain questions of expenditure addressed by the International Association of Chemical Societies," 1914, RSC, AR0318.

conditions of scientific endeavor abroad."[38] Nomenclature reform, it seemed, would be suspended for the duration.[39]

Fighting the War to End All Wars, and preparing for the next one

"The present war," remarked the Belgian-born plastics pioneer Leo Baekeland in an August 1915 plenary address at a meeting of the American Chemical Society, "has been aptly called a 'chemical war,' because efficient work of every department of the fighting armies, from the Red Cross service to the manufacture of guns and explosives, involves incessantly chemical knowledge."[40] The outsized role of chemistry in World War I and of World War I in the history of chemistry was already apparent to contemporary observers. Within weeks of the outbreak of war, international cooperation among chemists ceased, and patriotic rhetoric, often tipping into chauvinism, took hold among chemists of the belligerent nations. They rushed to offer their services to the mobilizing state, following the formula of scientific loyalty attributed (perhaps apocryphally) to Fritz Haber, leader of the German chemical weapons program: "during peacetime a scientist belongs to the world, but during wartime he belongs to his country."[41]

State sponsorship and wartime production, in turn, transformed chemical science and industry, especially in the Entente powers of Britain and France, and their ally (after 1917), the United States. The race to catch up to Germany in the production of chemical war materiel and

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[38] *Proceedings of the American Chemical Society* (1915): 49.

[39] World War I began during a period that had seemed to be marked by increasing cooperation and détente among the European powers, as exemplified by the development of IACS. An enormous body of historical scholarship has sought to explain (and, often, to assign blame for) the genesis of this conflict. For a thorough and authoritative recent treatment of the many factors that shaped the July Crisis and the outbreak of war, see Christopher M. Clark, *The Sleepwalkers: How Europe Went to War in 1914* (New York: Allen Lane, 2012).

[40] L. H. Baekeland, "Applied Chemistry," *Journal of Industrial & Engineering Chemistry* 7, no. 11 (1915): 978.

[41] The definitive history of the chemical weapons programs of the major belligerents during World War I remains Ludwig F. Haber, *The Poisonous Cloud: Chemical Warfare in the First World War* (New York: Oxford University Press, 1986). (The author is the son of Fritz Haber.) A more recent non-academic treatment of gas warfare and other applications of chemistry during the war is Michael Freemantle, *The Chemists' War: 1914-1918* (Cambridge: Royal Society of Chemistry, 2015).

to replace German chemical imports cut off by wartime embargoes invigorated the chemical industries in these countries, generating new links to chemical science and new support from national institutions inside and outside the state.[42] Prewar *inter*national institutions like IACS, in contrast, faded into irrelevance; as the story usually goes, the Great War dealt a serious blow to international cooperation in science, and especially in chemistry.[43]

In addition to the production of explosives and chemical weapons for the trenches, and of pharmaceuticals, dyes, and synthetic foodstuffs at home, there was another front in the "Chemists' War." Institution-building chemists in the Allied nations made the liberation of chemical information from German control a cornerstone of their strategy to compete more effectively with German chemical sciences and industries. In doing so, they at once emphasized the practical significance of reference works and denigrated the value of the intellectual labor – including the making of nomenclature rules – that went into compiling them. Following these projects across the transition from war to peace illustrates that the war did not simply scuttle international cooperation in science. Under wartime conditions, scientists and bureaucrats also generated new forms of scientific internationalism that would shape the organization of science after the war's end. The emergence and the fortunes of projects to compile non-German chemical reference works illuminate the continuities – at the level of national and international institutions – and changes – at the level of projects and priorities – between wartime and interwar chemistry.

IACS had been a collaborative venture of the leaders of chemistry in France, Britain, and Germany. Before the onset of war, the influential national positions of its founders had

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[42] On the US, see Kathryn Steen, *The American Synthetic Organic Chemicals Industry: War and Politics, 1910-1930* (Chapel Hill: The University of North Carolina Press, 2014). On France, see Harry W. Paul, *From Knowledge to Power: The Rise of the Science Empire in France, 1860-1939* (New York: Cambridge University Press, 1985).

[43] Elisabeth Crawford argues that in certain respects, international science never recovered from the effects of World War I; Elisabeth T. Crawford, *Nationalism and Internationalism in Science, 1880-1939: Four Studies of the Nobel Population* (New York: Cambridge University Press, 1992).

contributed to the rapid development of the association; afterwards, the leaders of IACS were the first to bury international goodwill under a hail of recriminations. Ostwald, the first president of IACS, was one of the ninety-three German intellectuals who signed the October 1914 appeal "To the Cultured World." The infamous open letter, also known as the "Manifesto of the Ninety-Three," flatly denied accusations of misconduct on the part of the German military in Belgium and condemned those who dared to spread such calumnies.[44] Ostwald's successor as IACS president, Ramsay, turned away from the universalist French sentiment that he quoted in 1913 to the clause that Louis Pasteur had once appended to it: "although science has no nation, the man of science ought to have one."[45] By mid-1915, Ramsay was writing that future scientific congresses would be possible only if Germans and Austrians were excluded.[46] As the war began, Ramsay's successor Albin Haller set aside the office of IACS President to attend to his other post as the French government's chief technical adviser on explosives.[47] In a letter to the Geneva chemist Amé Pictet the following summer, Haller offered a lengthy justification of his resolution "to cease all relation with the barbarians [i.e., German scientists] and to avoid meeting them, for they fill me with horror" – and this was *before* Haller's son was killed in action.[48] Meanwhile, Pictet and his colleagues in neutral Switzerland sought to

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[44] Organic chemists Adolf von Baeyer, Emil Fischer, Richard Willstätter were among the high-profile signatories. On the Manifesto and its subsequent use as a blacklist, see Brigitte Schroeder-Gudehus, "Challenge to Transnational Loyalties: International Scientific Organizations after the First World War," *Science Studies* 3, no. 2 (1973): 93–118; Brigitte Schroeder-Gudehus, *Deutsche Wissenschaft und internationale Zusammenarbeit 1914-1928: Ein Beitrag zum Studium kultureller Beziehungen in politischen Krisenzeiten*, Université de Genève. Institut Universitaire de Hautes Études Internationales. Thèse, n°. 172 (Geneva: Dumaret & Golay, 1966); Brigitte Schroeder-Gudehus, *Les scientifiques et la paix: La communauté scientifique internationale au cours des années 20* (Montreal: Presses de l'Université de Montréal, 1978). Ostwald promptly abandoned Ido for the promotion of German as an international scientific language; Gordin, *Scientific Babel*, 156–75.

[45] "*Si la science n'a pas de patrie, l'homme de science doit en avoir une*." The quote is from Pasteur's speech at the dedication of the Institut Pasteur in 1888; "Inauguration de l'Institut Pasteur," *Bulletin administratif du ministère de l'instruction publique*, no. 830 (November 24, 1888): 735.

[46] "Wird die gemeinsame geistige Arbeit nach dem Krieg wieder aufgenommen werden?" *Süddeutsche Monatshefte* 12 (August 1915): 829, quoted in Schroeder-Gudehus, *Les scientifiques et la paix*, 94.

[47] Haller had held this position since 1908; Danielle Fauque, "French Chemists and the International Reorganisation of Chemistry after World War I," *Ambix* 58, no. 2 (2011): 130.

[48] Haller to Pictet, 31 Jul 1915 and n.d. [1916], BGE, Ms. Fr. 3440.

maintain relationships with colleagues on both sides of the trenches while avoiding the appearance of taking sides.[49]

Chemists from Entente nations who had participated in IACS work on organic nomenclature set chemical naming aside to join the general mobilization that aimed to narrow the gap with the Germans in industrial capacity and technical expertise. In France, while Haller supervised explosives research and production, Grignard was called from Nancy to Paris to carry out research into war gases.[50] To avoid shortages and forestall hoarding of chemical goods, Béhal convinced the French government to establish an office to organize chemical production for civil uses. He headed the office for the duration of the war; among the chemists he employed there were Edmond Blaise, the French member of Jacobson's short-lived organic nomenclature working group, and Raymond Marquis, a member of the French national commission on organic nomenclature.[51]

As the war stretched into its third and fourth years, some observers of science and technology in Allied nations shifted their focus from mobilization for the immediate challenges of combat to preparation for an economic and intellectual war to come. In a January 1917 letter to *Nature*, for example, the Italian polymath Eugenio Rignano reflected on the "terrible danger that they [the Allied nations] have run of being victims of German hegemony in all fields of human activity – economic, technical, and scientific." During the war, however, "the nations of

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[49] In 1917, Pictet and a few of his colleagues founded a new journal of chemistry intended to embody the identity and neutrality of Swiss chemistry. The journal's title, *Helvetica Chimica Acta*, was in Latin; it published articles in German, French and Italian, then the three official languages of Switzerland. Philippe A. Guye, "Avant-Propos," *Helvetica Chimica Acta* 1 (1918): 3–4; Fichter to Pictet, 13 Jan 1915 and 2 Dec 1917, BGE, Ms. Fr. 3438; Résolutions sur le journal de Chimie, 20 Oct 1917, BGE, Ms. Fr. 3459, Folder 2.

[50] Grignard first proposed war gas research in December 1914. However, in view of the dominant position of the German chemical industry, he was directed to begin such research only after the German army introduced chemical weapons on a large scale at Ypres in April 1915; Mary Jo Nye, *Science in the Provinces: Scientific Communities and Provincial Leadership in France, 1860-1930* (Berkeley, Calif: University of California Press, 1986), 187–189.

[51] Nicole Duchon, *Auguste Béhal: De Lens à Mennecy* (Le Mée-sur-Seine: Lys éditions presse/Editions Amattéis, 1991), 66–76. Another of the chemists who worked under Béhal at the Office was Adrien Fauçonnier, a longtime comrade from Friedel's laboratory and a fellow member of Friedel's nomenclature reform subcommission.

the Entente have hastened to adopt as remedies all kinds of measures of initiative [*sic*] – governmental and private – which will certainly be successful." Rignano cited new industries, trade policies, institutions of science and technology, and the application of "fundamental practical principles of organisation" as examples of this wartime initiative. However, he argued that the Entente nations had to do more than win the war. They also had to ready themselves for what Rignano called a "pacific war of liberation from German hegemony, that is being prepared even during this bloody war and that must be continued more vigorously than ever after peace comes."[52] To those of Rignano's mindset, the politics of international science was the continuation of war by other means.

Such sentiments were common, but they presented a conundrum, especially for chemists.[53] A plan to overcome German dominance in the chemical sciences rested on the premise that Germany could not have gotten ahead in the first place through the superiority of its intellectual culture, as implied by the Manifesto and other statements that had so infuriated Allied scientists. Rather, it must have gained the advantage though some "German method enabling the most mediocre brains to produce great works without thinking," as a French professor of surgery put it.[54] If such a "German method" could be identified, British, French, American, and other Allied nations' scientific establishments could build a strategy to undermine German chemical superiority around undermining or replicating this method.

For Rignano and many others, the answer to this riddle was the scientific reference literature. Wrote Rignano:

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[52] Eugenio Rignano, "A Plea for a Scientific Quadruple Entente," *Nature* 98, no. 2465 (January 25, 1917): 408.

[53] Daniel Kevles refers to preparation for postwar economic and intellectual combat as a "commonplace theme" of Allied rhetoric in the latter years of the war; Daniel J. Kevles, "'Into Hostile Political Camps': The Reorganization of International Science in World War I," *Isis* 62, no. 1 (April 1, 1971): 59.

[54] Pierre Delbet, quoted in Harry W. Paul, *The Sorcerer's Apprentice: The French Scientist's Image of German Science, 1840–1919* (Gainesville: University of Florida Press, 1972), 51–52.

> The numberless *Archivs*, *Jahrbücher*, *Zeitschriften*, *Zentralblätter*, and so on, which have been yearly increasing in number and volume, have gradually monopolised the whole of the scientific production of the world…. Thus were apparently built up international scientific organs, but in reality German instruments of control and monopoly of science.
>
> It is, then, in this domain that it seems necessary to prepare and begin our pacific war of liberation from German predominance.[55]

In chemistry, control of the reference literature provided an especially marked advantage, manifested in various ways. The first was simply access. Charles Herty, ACS President during the first years of the war, praised American chemists for their achievements in replacing embargoed German dyes and pharmaceuticals but lamented that they had to do so "under a serious handicap, the shortage, produced likewise by war conditions, of available sets of the greatest of all reference books in organic chemistry, Beilstein's *Handbuch der Organischen Chemie*."[56] A committee representing British chemical societies likewise noted that wartime restrictions on trade – imposed by Germany, not Britain, it pointedly noted – had kept the most recent volumes of these compendia out of the hands of British chemists. This committee also identified a second level of control that the German compilators exercised:

> [T]he evident wish to promote the sale of the *Zentralblatt* has led Stelzner, for example, to give references from 1910 onwards not to the original source, if non-German, but to the *Zentralblatt*, and these without the authors' names -- making reference impossible except through the medium of this publication, issued by the Deutsche Chemische Gesellschaft.[57]

The manner in which the DChG had woven together its reference publications made it necessary not only to have copies of one German reference work, but all of them.

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[55] Rignano, "Scientific Quadruple Entente," 408.

[56] Charles H. Herty, "Turn About Is Fair Play," *Journal of Industrial & Engineering Chemistry* 10, no. 9 (September 1918): 672.

[57] "Report of the General Committee of Chemical and Allied Societies RE the question of publishing Chemical Bibliographies in the English Language," 30 Jan 1919, RSC, AR0318, 1.

A third level of control was simply language. The interruption of access to journals had presented an "opportune time to break with the tradition of viewing chemistry 'through German eyes,'" as the British committee noted.[58] Asked an American commentator, "Are the English-speaking peoples of the world to return at the close of the war to the well-nigh complete dependence on Germany for their standard scientific reviews and handbooks, and thus make necessary the learning of the difficult German language for all young scientists?"[59] Furthermore, patriotic sentiment, and in some cases anti-German laws, had caused the study of German to plummet in the Allied nations during the war years.[60] "This fact makes it all the more unreasonable," wrote the British committee, "that English-speaking chemists should be compelled to learn the elements of a foreign language before they can consult compendia of the world's chemical literature."[61]

A final consequence of the German hegemony over chemical reference literature, argued observers in Allied nations, was an inflated sense of the prestige of German chemistry. "The habit of consulting German works of reference – in default of others – leads by mass suggestion to an appreciation of German effort which is not warranted by the facts," the British committee wrote.[62] This tendency had not only granted an excessive share of prestige to German science, but, Rignano contended, had also contributed to a dangerous scientific and

[58] "Report of the General Committee," 1.

[59] Edwin Bidwell Wilson, "Insidious Scientific Control," *Science* 48, no. 1246 (November 15, 1918): 492.

[60] Gordin, *Scientific Babel*, 180–83.

[61] "Report of the General Committee," 1. Some scientists argued for the German-language publications and even the German language itself to be cast outside of the circle of scientific authority; Gordin, *Scientific Babel,* 173–80. See also Schroeder-Gudehus, *Les scientifiques et la paix*, 95–97; G. A. Miller, "Scientific Activity and the War," *Science* 48, no. 1231 (August 2, 1918): 117–18. Even an article arguing against such a linguistic boycott was shot through with anti-German rhetoric; W. J. Holland, "Shall Writers upon the Biological Sciences Agree to Ignore Systematic Papers Published in the German Language Since 1914?," *Science* 48, no. 1245 (November 8, 1918): 469–471.

[62] "Report of the General Committee," 1.

cultural hubris that had emboldened Germany to wage war, and whose elimination was "one of the surest means of guaranteeing peace for the future."[63]

Rignano went so far as to argue that compilation was not only a misleading source of prestige, but was better understood as a sign of an intellectual deficit.[64] In his opinion, the enormous size of tomes like *Beilstein* were not a sign of intellectual gravity but craven profiteering among German publishers, coupled with "the excessively analytic spirit of the Germans, which robs them of broad views and a resulting true perspective of things, and predisposes them to put on the same plane the whole of a collection of facts and researches in which nobody can distinguish what is very important from what is less important or not important at all."[65]

While some chemists, like Rignano, took compilation as a sign of intellectual inferiority, others, like American mathematician Edwin Wilson, admired the power and savvy of this means of "insidious scientific control."[66] In either case, they had found an explanation for German chemical prestige that could be distinguished from authentic contributions to the advancement of science. The British committee's claim that for chemistry done since the early 1890s, "compendia embodying the results are to be found only in German" was obviously

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[63] Rignano, "Scientific Quadruple Entente," 408.

[64] On French critiques of German science during the war, see Paul, *The Sorcerer's Apprentice*, 29–76. There was a distinct but related wartime tendency to denigrate the German language. Repeating the familiar request that English-speaking chemists use the French-derived –ene rather than the German-derived –ol forms of the names of benzene and its derivatives, E. J. Crane noted that this was not just a matter of cultivating "good English chemical nomenclature," but of using "pure English terms," since the "appearance of aping the German is of course distasteful to us all." Evan J. Crane, "Notes and Correspondence: Chemical Nomenclature," *Journal of Industrial & Engineering Chemistry* 11, no. 1 (January 1, 1919): 72. Writing with regard to biological systematics, a British zoologist proposed that species descriptions published in German be ignored, since "German is, without doubt, a barbarous language only just emerging from the stage of the primitive Gothic character." He continued, "it would be to the advantage of science to treat it as such from the date August 1, 1914." It took more than barbarity of language, apparently, to disqualify a species description. G. F. Hampson, "Systematic Papers Published in the German Language," *Science* 49, no. 1260 (February 21, 1919): 193.

[65] Rignano, "Scientific Quadruple Entente," 408.

[66] Wilson, "Insidious Scientific Control."

hyperbolic. But even such exaggeration was evidence of the importance of the thesis to the postwar plans of Entente chemists.[67]

Wartime plans for post-war institutions

In 1917, the Chemical Society of London began to consider specific means of producing such works of reference literature after the war had come to an end.[68] Representatives of the Chemical Society and its industrial analog, the Society for Chemical Industry (SCI), had already begun to discuss the possibility of combining their abstract journals. By doing so, the two societies could economize on printing and labor and make for a more complete English-language guide to the latest periodical literature in both pure and applied chemistry.[69] In October, the Society's publications committee took up a more ambitious proposal for "publishing in English an equivalent of Richter's *Lexikon*."[70] In December, *Journal of the Chemical Society* editor J. C. Cain reported that the publications committee universally recommended publishing such a reference work.

Plans for compiling this and other English-language chemical reference works were entrusted to a joint committee representing the Chemical Society of London, the Society for Chemical Industry, and the Association of British Chemical Manufacturers. Critiques of the scientific prestige that had accrued to German reference works notwithstanding, the committee was not above looking to German models. For their organic chemical compilation, committee members considered "suggestions ranging from the equivalent of Richter's 'Lexikon' to a more

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[67] "Report of the General Committee," 1.

[68] No one framed the production of reference literature as an immediate existential necessity on the order of the crash programs to produce explosives, war gases, and gas masks, or the careful management of chemical production for the home front, but Herty came closest to doing so. Though he admitted that chemists could not undertake the compilation of such a work until after the war, he did propose a $40,000 scheme for producing 1,000 copies of the 11,126-page third edition of *Beilstein*, taking advantage of the Trading with the Enemy Act to get around copyright restrictions. Herty, "Turn About Is Fair Play."

[69] Chemical Society Council Minutes, 21 Dec 1916 and 18 Jan 1917, RSC, AR0148.

[70] Chemical Society Council Minutes, 11 Oct 1917, RSC, AR0148.

exhaustive and complete work on the lines of Beilstein's 'Handbuch,'" before deciding on "a work more on the lines of Stelzner's 'Literatur-Register.'" The committee's proposed compendia for inorganic chemistry, physico-chemical data, and organic chemical patents were similarly modeled on specific German texts.[71] The committee members seemed to take a German model for their budget, as well. They learned that the DChG claimed to have collected 2.5 million marks (the equivalent of £125,000) in mid-1918 to expand its reference publications. The committee correspondingly scaled up its plans, more than doubling the budget for the project from a previous estimate of £62,650 to £129,350. The scheme would be financed by advance subscriptions, donations from "firms, Government, etc.," and interest from the sponsoring societies' investments. The committee estimated that the scheme would break even with the sale of 1,550 of the proposed 2,000 sets of volumes; profits would be used to finance supplemental volumes at five-year intervals.[72]

If the committee's organic chemical compendium was to be modeled on a German text, the nomenclature system used to organize it would be wholly British. The committee anticipated that building the system would cause little trouble. "The order in which substances are mentioned and the whole system of nomenclature are to be decided subsequently by the Nomenclature Committee," they wrote, "but it is anticipated that the nomenclature adopted by the London Chemical Society will be closely followed." As an afterthought, the committee added, "It would be desirable to have an alphabetical index of the names of compounds; such an index could be arranged to show where derivatives of these compounds were to be found."[73] Perhaps so, but this was precisely the sort of Geneva-style nomenclature project that Jacobson

[71] Specifically, Abegg and Gmelin-Kraut, for inorganic chemistry; Landolt-Börnstein-Meyerhoff, for physico-chemical data tables; Friedländer, for patents.

[72] "Report of the General Committee," 1–2, 4–5.

[73] Ibid., 2; For the nomenclature rules applied in the society's journals at that time, see "Instructions to Abstractors, Giving the Nomenclature and System of Notation, Adopted in the Abstracts," *Journal of the Chemical Society (Abstracts)* 114 (1918): 1–5.

– the general editor in charge of the German texts that the committee proposed taking as models – had renounced. The "nomenclature adopted by the London Chemical Society" – twenty spare rules, some addressing inorganic compounds only, none addressing ring compounds other than benzene – was unlikely to provide much help along these lines. Committee members Wynne and Cain, the former and current editor of the *Journal of the Chemical Society* and members of the British organic nomenclature committee organized under IACS, were presumably aware of this. However, nomenclature rule-making was just the sort of activity that the allied chemists had characterized as intellectually trivial in their wartime discussions of reference works, even as they emphasized the practical significance of these works to the productivity and reputation of German chemistry. The development of rules of nomenclature to be used in new reference works was marginalized by the very logic that brought about the British compilation project.

This logic also helped the committee dismiss worries that the project might take chemists away from other tasks crucial for the advancement of British chemistry. "The view of the Executive Committee," they explained, "is that the type of chemist who will be best for much compilation work will not be the laboratory type; it has further been suggested that the compilation should be carried out by disabled men who are unable to carry on practical work."[74] This was not merely an improvised response to the exigencies of the postwar chemical labor force, but the logical conclusion from the chain of reasoning that led Entente scientists to call for such compilation projects in the first place. In order to be an effective means of surpassing German chemistry, compendia had to be at once absolutely crucial and the work of inferior minds and hands, sources of illusory scientific prestige that the Entente nations wished to claim for themselves.

[74] "Report of the General Committee," 5.

Alongside these national preparations, international chemical institutions oriented toward the "war after the war" were also taking form. As Daniel Kevles has shown, the American astronomer George Ellery Hale pursued both national and international scientific organization during the war.[75] In 1916, Hale convinced the leaders of the National Academy of Science to establish a National Research Council (NRC), for which he served as founding president. The NRC not only brought together American military, academic, industrial, and government experts to coordinate national scientific efforts, but also sent liaisons to Europe to manage the exchange of technical information among the British, French, and American militaries. Indeed, by 1918, even in the sensitive area of war gases, Allied organizations were collaborating and coordinating their work through a series of joint meetings, through which, Haber *fils* has argued, "the organization of production was becoming increasingly business-like and assuming a multinational dimension."[76]

Hale shared an institution-building goal with colleagues in Britain, France, and Belgium: specifically excluding scientists from Germany and the other Central Powers from postwar international scientific associations. In October of 1918, Hale and his French and British colleagues met in London to flesh out his plan for pursuing this goal. In order to circumvent statutory obstacles and scientists' objections, Hale and his collaborators sought to build an international scientific organization upon the scaffolding of inter-Allied military cooperation. While the war lasted, the organization's statutes could legitimately be written by representatives of the Allied powers alone, so that after the war ended, its membership could be expanded to include neutral countries, but not Germany and its former allies.[77]

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[75] Kevles, "'Into Hostile Political Camps'"; Daniel J. Kevles, "George Ellery Hale, the First World War, and the Advancement of Science in America," *Isis* 59, no. 4 (1968): 427–437.

[76] Haber, *The Poisonous Cloud*, 149.

[77] Kevles, "'Into Hostile Political Camps,'" 52–56.

In November 1918, a few weeks after the armistice, scientific representatives of the Allied nations met in Paris to draw up statutes for the organization, which they christened the International Research Council (IRC). To plan a new international chemical union, they formed a Committee on International Cooperation in Chemistry. This committee was chaired by Haller, who was still the nominal president of IACS (which had not met or held elections since 1913) and, as we have seen, an ardent advocate for excluding Germany from international science. However, Haller faced the same challenge as the creators of the IRC: IACS statutes provided no clear path for restructuring the association to exclude the Germans and their allies.

The challenges would be solved by similar means. As the IRC was being formed, a parallel effort to organize an Entente-controlled international chemical union was taking shape, led by industrial chemists. During the war, the British SCI became an influential outlet for that nation's chemists to weigh in on the wartime organization of chemical science and industry. In 1917, French chemical industrialist Paul Kestner, a member of the British SCI and a friend of its president, Henry Louis, proposed founding an analogous organization in France. With the support of Haller, a longtime sponsor of applied chemistry in France, and the help of Jean Gérard, a Grignard student working on war gas research under the organic chemist Charles Moureu, Kestner founded the *Société de Chimie Industrielle*. Kestner served as its president, and Gérard as its general secretary.[78]

In honor of the founding of its French sister society, the British SCI invited Kestner to deliver a plenary lecture in London at the beginning of November, 1918. During this visit, just days before the armistice, Kestner and Louis formed a plan to create a new inter-Allied chemical union. Acting along the lines proposed by the organizers of the incipient IRC, though not formally as a part of their effort, they laid down plans for an inter-Allied conference to be

[78] Danielle M. E. Fauque, "French Chemists and the International Reorganisation," 124–125.

held in Paris, under the sponsorship of the French SCI.[79] Meanwhile, in early 1919, Haller's IRC committee circulated a draft report on the formation of an "International Chemistry Council."[80] In April, delegates from Belgium, Britain, France, Italy, and the United States gathered in Paris for the "Inter-Allied Conference of Chemistry" that Kestner and Louis had organized.

The meeting took place in the midst of the Paris Peace Conference, in which diplomats from the victorious allied nations debated peace terms to be presented to the former Central Powers and the establishment of mechanisms for keeping international peace in the future. These negotiations yielded the Treaty of Versailles, a peace treaty that imposed numerous restrictions and responsibilities on Germany, most notably an onerous financial burden in reparations payments to the Allied nations. The conference also laid down the foundation for the League of Nations, an intergovernmental association that was to serve as a central body for adjudicating disputes between nations and safeguarding international peace. Both the Treaty and the League were subject to serious criticism, then and since.[81]

The chemists' negotiations involved representatives of a similar bloc of nations under similar geopolitical circumstances as the peace talks. Not surprisingly, both the organization that these chemists built and this organization's relations with Germany took on a similar cast to the League of Nations and the Treaty of Versailles. Under the presidency of Moureu, leader of the French chemical weapons program, the delegates to the conference of chemistry began to draft their own set of provisional statutes for an "Inter-Allied Federation of Chemical Societies."

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[79] Ibid., 125; Fennell, *History of IUPAC, 1919-1987*, 14–15.

[80] "Annual Meeting Division of Chemistry and Chemical Technology, National Research Council," *Journal of Industrial & Engineering Chemistry* 11, no. 5 (1919): 481–483.

[81] Manfred F. Boemeke, Gerald D. Feldman, and Elisabeth Gläser, eds., *The Treaty of Versailles: A Reassessment after 75 Years*, Publications of the German Historical Institute (New York: Cambridge University Press, 1998); Marc Trachtenberg, "Reparation at the Paris Peace Conference," *Journal of Modern History* 51, no. 1 (1979): 24–55. On the politics of reparations and the question of the economic viability of the Weimar Republic, see Detlev Peukert, *The Weimar Republic: The Crisis of Classical Modernity* (London: Allen Lane, 1991); Ian Kershaw, ed., *Weimar: Why Did German Democracy Fail?* (London: Weidenfeld and Nicolson, 1990). For a critical perspective on the Treaty of Versailles by a contemporary observer, see John Maynard Keynes, *The Economic Consequences of the Peace* (New York: Harcourt, Brace and Howe, 1920).

The delegates agreed that unlike the IACS, whose member organizations had been chemical societies, this federation should be made up of national federations of all of the various societies representing academic and industrial chemists in each nation. They formed a provisional council, chaired by Moureu, that would present a full set of statutes at a meeting in London in July 1919.

The International Chemistry Council that Haller's group had proposed and the Inter-Allied Federation of Chemical Societies proposed at the conference presided over by Moureu were more like two versions of the same project than competitors. Indeed, Haller delivered the closing address at Moureu's conference.[82] Both organizations focused on the importance of reference works and nomenclature standardization. Among the primary aims outlined in the report of Haller's committee were the promotion of cooperation in the "preparation and publication of chemical literature" and the appointment of nomenclature standardization committees.[83] And in his address at the Inter-Allied conference banquet, Moureu spoke at length of the urgent need to wrest control of chemical information from the Germans. "Among all the instruments that the chemist uses in his work," he said, "there is none more indispensable than good books and good bibliographic literature. He can no more do without these than he can do without the balance or the thermometer."[84]

At the mid-July meeting of Moureu's commission in London, after some debate, the assembled chemists agreed upon a collection of statutes for a Confederation of Associations of

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[82] The two organizations were closely coordinated by their leaders and easy to confuse. According to a news item in an American Chemical Society journal, Haller's committee met in London two days after the end of the Paris conference, appointing a provisional council made up of precisely the same set of chemists as the group chaired by Moureu, save that Haller and Henri Le Châtelier were honorary presidents. It may be that the ACS correspondent mixed up the two groups, or it may be that Haller and Moureu did indeed coordinate their efforts that closely. "Scientific Societies," *Journal of Industrial & Engineering Chemistry* 11, no. 6 (1919): 597.

[83] "Annual Meeting Division of Chemistry," 482.

[84] "La conférence interalliée de la chimie, 14-15-16 Avril," *Chimie & Industrie* 2 (1919): 569. On this point, see also Fauque, "French Chemists and the International Reorganisation," 127.

Pure and Applied Chemistry. Moureu then immediately departed to join Haller in Brussels for the constitutive conference of the IRC. Haller formally presented the confederation's statutes, then resigned his presidency of the IRC Committee on International Cooperation in Chemistry and proposed Moureu as his successor. After making some minor changes to the statutes – including changing the organization's name to the International Union of Pure and Applied Chemistry – IUPAC was formally approved as one of the three founding unions of the IRC. Haller declared IACS dissolved and returned the association's funds to Solvay.[85]

Exactly a month after the signing of the Treaty of Versailles, chemistry had its own analogous new international order. After seven months of intersecting negotiations among different organizing committees, Haller and his fellow-minded Entente chemists had achieved their goal: an international chemical union representing chemists in France, Britain, America, Italy, and Belgium, open to neutral nations to whom these founding members agreed to extend invitations, and essentially closed to others, especially the former Central Powers. The first two presidents of the Union, the Frenchman Moureu and the Briton William Pope, were one more sign of its relationship to the institutions of wartime chemistry. Each had led the chemical warfare effort of his respective nation.[86]

Meanwhile, the British compilation program got off to a promising start, but soon faltered. Pope promoted the plan for the publication of English-language compendia in his

[85] The value of the Solvay endowment had likely fallen significantly since 1913, making it easier for Haller and the other founders of IUPAC to reconcile themselves to giving it up. The Belgian franc depreciated by 62% relative to the dollar between 1913 and 1920 (per www.measuringworth.com), and wartime inflation may have further reduced the value of these funds. Solvay redirected the endowment to fund a series of Solvay Conferences on Chemistry, modeled on the Solvay Congresses on Physics begun in 1911 and resumed in 1921. On the chemistry conferences, see Mary Jo Nye, "Chemical Explanation and Physical Dynamics: Two Research Schools at the First Solvay Chemistry Conferences, 1922–1928," *Annals of Science* 46, no. 5 (1989): 461–480.

[86] Pope was knighted for his chemical warfare service. Pope remained a proponent of chemical weapons well into the 1920s, even as international opinion turned against them, culminating in the Geneva Protocol of 1925, which banned their use. Haber, *The Poisonous Cloud*, 282, 293.

address as president of the Chemical Society in March 1919.[87] The committee solicited the cooperation of the chemical societies of its wartime allies, America and France. Both offered their support, but the kinds of collaboration that they suggested did not augur well for the internationalization of the effort. The Americans warmly endorsed the plan, and proposed that the ACS take on the preparation of critical volumes on physical and chemical data, leaving the compendia of organic and inorganic chemistry to the British.[88] Having just completed the publication of the Collective Index to *Chemical Abstracts*, the Americans already had an authoritative key to the previous decade's literature on inorganic and organic compounds. This form of "collaboration," then, was primed to produce a complete set of English-language chemical reference works in America, a nation that Pope described in his March 1919 speech as a commercial competitor at least as threatening to British interests as Germany.[89] The French, on the other hand, wanted to add an international French-language abstract journal into the mix; neither the British nor the Americans were interested in collaborating on such a project.[90]

By 1920, the budget for the British project had swollen to £250,000. The Chemical Society enlisted Lord Moulton, the much-praised Director-General of the Explosives Department during the war, to solicit contributions. A year later, Moulton died. With European economies wracked by inflation and the first volumes of the new fourth edition of *Beilstein* beginning to appear, the promoters of the compendia plan admitted defeat.[91] Organic chemists

[87] William J. Pope, "Presidential Address. Chemistry in the National Service," *Journal of the Chemical Society (Transactions)* 115 (1919): 407.

[88] "Report by the Committee on Publication of Compendia of Chemical Literature, etc," *Journal of Industrial & Engineering Chemistry* 11, no. 5 (May 1, 1919): 415–17. These would become the *International Critical Tables*, a compilation of physical and chemical data published and continuously revised under NRC sponsorship beginning in the mid-1920s.

[89] Pope, "Presidential Address. Chemistry in the National Service," 404.

[90] Fauque, "French Chemists and the International Reorganisation," 128.

[91] Federal Council for Pure and Applied Chemistry circular, 1922, RSC, AR0319.

in Britain, France, and American would content themselves with continuing to consult *Beilstein*, after all.[92]

Nomenclature reform was all that was left of the grand compilation projects of 1917 and 1918. Nomenclature would, by all accounts, be a major part of IUPAC's job. However, all of the intricate machinations by which its founders had managed to exclude Germany from membership also excluded Jacobson and the editors working under him – most of the world's experts in organic chemical nomenclature. The framing of such work as intellectually trivial during the war may have contributed to the relative neglect paid to how it was to be carried out during the formation of IUPAC. Or perhaps this neglect was just due to the overwhelming attention that had to be paid to the choreography of commissions and nascent institutions; such bureaucratic confusion would continue to plague the Union. As much work as it took to keep Germany out of the Union, it would take even more to get Germany in. Here, however, nomenclature would not be pushed aside as a matter for another day, but would take a central role as a means and justification for making a place for Germany in international chemistry once again.

Nomenclature reform *sans* Germany

Nomenclature reform occupied a peculiar position in the work of the newly constituted International Union of Pure and Applied Chemistry. The founders of the Union had cited standardization of nomenclature as a central project that the Union would take on. However, as ambitious wartime plans to create new reference works petered out in the face of the economic downturn and the limits of postwar cooperation, the context for the development of this new nomenclature disappeared. In the absence of such a project, some IUPAC delegates looked to the prewar nomenclature work of IACS, orchestrated by Jacobson, as a basis for the Union's

[92] Schroeder-Gudehus, *Les scientifiques et la paix*, 138.

work on organic nomenclature. However, this was no less problematic a starting point. As a German, Jacobson was statutorily forbidden from taking part in the work of IUPAC. Further, Jacobson had tailored this work of this commission to support the compilation enterprises of the DChG – the very reference works whose hegemony IUPAC had been created in part to combat. New editions of these works, as well as the Collective Index to *Chemical Abstracts*, had begun to be published, with their own elaborate nomenclature systems in place.

The uncertainty of IUPAC's founders over what to do about organic nomenclature was reflected in its absence from the agenda at the first annual meeting of IUPAC, held in June 1920 in Rome. Czech delegate Emile Votoček, however, was determined that this omission be rectified. At the end of the fourth and final day of this meeting, Votoček moved that nomenclature reform be placed on the agenda for the following year's conference. He volunteered to contribute a report on the matter.

Votoček was primarily interested in bringing Slavic chemical publications out of the shadow of the German language. Like Friedel in the late 1880s, he sought to reform his own language and promote its use through the construction of international (and interlinguistic) nomenclature rules.[93] In his report, Votoček sketched an optimistic, progressive history of organic nomenclature reform. He began with the Geneva Congress, noting that many of the decisions made at that meeting had passed into practice. Votoček drew a straight line from the Geneva Congress to the work of the IACS nomenclature commission. He offered no organic nomenclature propositions of his own, but summarized the reports that the national nomenclature committees presented at the 1913 IACS meeting. Speaking of both inorganic and organic nomenclature, the Czech chemist concluded that the task of the members of the Union's

[93] A spirited Czech nationalist, Votoček dedicated a good deal of his report to presenting the advantages offered by the "great richness of the Czech language," as exemplified by the Czech nomenclature of inorganic compounds formulated by Jan Svatopluk Presl in 1828. Votoček granted, not without a hint of pride, that the principles of this nomenclature could only be applied in Slavic languages; Votoček, "La réforme de la nomenclature," 83–86.

commissions would be simply "to bring to a successful completion the work so happily begun before the war, adding, if necessary, improvements that they judge useful, and above all to develop the necessary procedures to put into practice the reforms that should be agreed upon."[94]

Writing on behalf of the French delegation, Marcel Delépine was less optimistic in his judgment of the progress of organic nomenclature reform since Geneva. He pointed out that despite the initial enthusiasm for the Geneva nomenclature, even in Germany, "everyone knows that one would search in vain for these names in the majority of reference works."[95] Delépine laid blame for the paltry enactment of the Geneva rules at the feet of the insufficiently demanding Geneva delegates, who, "once they returned home, neglected to impose the decisions taken by the international committee on their compatriots."[96] As Delépine saw it, IACS had not taken any action at all in the area of organic nomenclature; he was doubtful that any international body could, given the differences in the basic chemical vocabulary of each language. Like Votoček, he emphasized that any decision on a national or international level would have to be promoted and enforced by national chemical societies. The two reports reflected the varieties of sentiment among Allied scientists toward German reference works during the war: Delépine, the disparagement of the purported accomplishments of the Germans, Votoček, the determination to beat the Germans at their own game.

At IUPAC's following meeting in 1921, Moureu convened an ad hoc committee to determine the form that the Union's nomenclature activities would take. Béhal presided over the group, which included Votoček, Delépine and nineteen other delegates chosen as those

[94] Ibid., 86.

[95] IUPAC Comptes Rendus 2 (1921): 76.

[96] Ibid. The argument seems to have been shaped by the post-Versailles politics of the enforcement of international agreements.

"who have considered the question somewhat carefully."[97] The group resolved that the Union would "resume the work begun in 1913 by the International Association of Chemical Societies," adding the third prong of biochemical nomenclature to the previous association's work on the nomenclature of organic and inorganic chemistry.[98] Accordingly, Béhal's committee constituted three separate nomenclature commissions, each made up of one delegate from each nation represented in the Union (twenty-one, at that time). Each commission was in turn to choose a working group (*Comité de Travail*) made up of six members. Each working group member was charged with establishing a national commission to discuss nomenclature questions and with relaying its conclusions to his fellow working group members. The working group would convene annually to discuss these various reports and submit definitive proposals to the larger Commission.[99]

Béhal's committee provided no specific criteria for membership in the working group, but its selections fit the aim of resuming the work of IACS, filtered through the geopolitics of the war. Three members, Biilmann (Denmark), Blaise (France), and Ciamician (Italy, appointed in absentia) had also been members of Jacobson's prewar working group on organic nomenclature. Having replaced Cain as editor of the *Journal of the Chemical Society*, Clarence Smith was also assigned a seat on this commission, in absentia. Votoček was added as a representative of a Slavic-speaking nation.[100] MIT chemist James Norris was added in absentia as an American representative, likely on the recommendation of Harvard organic chemist

[97] IUPAC Comptes Rendus 2 (1921): 24–25.

[98] On the emergence of the discipline of biochemistry during this period, see Robert E. Kohler, *From Medical Chemistry to Biochemistry: The Making of a Biomedical Discipline* (New York: Cambridge University Press, 1982).

[99] IUPAC Comptes Rendus 2 (1921): 5–6, 53–54.

[100] The inclusion of a Slavic-speaking representative was clearly important to the organizers of the working groups. Unable to name such a representative immediately, the working groups of the inorganic and biochemical nomenclature commissions kept their sixth slots open for "a representative of the Slavic languages." Ibid., 54.

James B. Conant, chosen as the American member of the larger commission.[101] The ever-willing volunteer Amé Pictet rounded out the group; Jacobson, needless to say, was left off the list.[102]

This initial form of the IUPAC commission on organic chemical nomenclature encountered some challenges. The organic nomenclature commission was to hold its first meeting at the 1922 IUPAC conference; however, only seven of seventeen members of the commission were present in Lyon. An additional five countries had not yet appointed their commission members. Six other conference attendees joined these seven commission members at the commission meeting, per a Union policy permitting the replacement of commission members for the purpose of conducting conference business.[103] The meager attendance among commission members could perhaps be explained by the working plan laid out the previous year: until the working group submitted a definitive proposal, the official Commission had nothing to do. However, only two of the six members of the working group itself – Biilmann and Pictet – showed up in Lyon. Ciamician had died in the intervening year, and his replacement did not attend. In lieu of Norris, W. A. Noyes represented the U.S. at the meeting. Smith did not attend the conference, nor did he establish the national committee that members of the working group had been instructed to organize.[104]

Although Blaise did not attend the meeting, he was the one working group member who carried out his assigned duty of preparing a report of his national commission. It was an impressive document, clearly the fruit of careful thought, positioned as a direct continuation of the work of Jacobson's committee. The report was filled with precise details of the work of the

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[101] Norris had recently begun a five-year term as one of nine associate editors of the *Journal of the American Chemical Society*; *Proceedings of the American Chemical Society* (1920): 145.

[102] IUPAC Comptes Rendus 2 (1921): 66–67.

[103] IUPAC Comptes Rendus 3 (1922): 51.

[104] Smith did not find out that he'd been chosen as a member of the inorganic nomenclature working group at the 1921 conference until he received the proceedings of the 1922 conference the following winter, and it is possible that Smith *never* learned that he was part of the organic nomenclature working group. Smith to Gérard, 23 Apr 1923, IUPAC papers, Box 51 (filed in folder 1924).

earlier committee, insights regarding the relationship between the names used in indexes and in other genres of chemical writing, and astute judgments of the shortcomings of the Geneva nomenclature. It clearly laid out questions that the working group would have to answer regarding the purpose and form of the reform it was to undertake and fundamental decisions about the arrangement of parts of a chemical name relevant for nearly all organic compounds. It concluded with a collection of detailed recommendations regarding the acyclic hydrocarbons, the first order of business on the working plan agreed upon in 1914. Blaise was an unimpeachable authority in French organic chemistry. He was a student of Béhal and Friedel, and had recently succeeded Haller (who succeeded Friedel, who succeeded Wurtz) as Sorbonne chair in organic chemistry. His report was cosigned by a French nomenclature commission whose membership remained nearly unchanged since 1912 and included three veterans of the Geneva Congress.[105] It provided an authoritative, thorough basis for the orderly undertaking of a program of organic nomenclature reform.

Before this report could be discussed, however, British commission member and meeting co-chair William Pope presented a plan to entirely reorganize the work of the commission. Apparently unimpressed with the scope of Blaise's contribution, and perhaps still under the influence of wartime assumptions of the meager intellectual challenge presented by systematic nomenclature, Pope proposed that the commission aim to "furnish a report, in 1923, for formulating a complete system of nomenclature, which could be adopted in the major scientific and technical journals of chemistry and by the authors of reference and instructional works."[106] Pope believed that the duty of preparing this report should not be entrusted to the

[105] IUPAC Comptes Rendus 3 (1922): 77–83.

[106] Pope's resolution did not make it clear whether he was truly calling for the establishment of a complete set of rules of nomenclature applicable across chemical journals, reference works, and textbooks in a year's time, or, more plausibly, for a more concrete plan of action for building such a system than the previous year's meeting or Blaise's report had provided. The ambiguity turns on the clause "*Que la Commission fournisse un rapport en 1923, en*

existing commission or its working group, but to "the editors of the most significant chemical journals," and that "the functions of the commission be limited to examining and critiquing the reports of the experts, the editors."[107]

As co-chair (with Béhal) of the meeting of the nomenclature commission in Lyon and the incoming IUPAC president, Pope wielded considerable influence. This influence was magnified in the absence of most of the working group, including Blaise, the only working group member who had expended any effort in its service. With the assent of a group of chemists who, for the most part, had little particular expertise or stake in matters of nomenclature, Pope redefined who counted as an expert in organic nomenclature: not literary workers, as Jacobson had it, but specifically editors-in-chief of major journals. The commission members put the preparation of this report in the hands of three editors in particular – E. J. Crane of *Chemical Abstracts*, A. J. Greenaway of *Journal of the Chemical Society*, and Raymond Marquis of *Bulletin de la Société Chimique*.[108]

Marquis had at least been present in Lyon; Crane and Greenaway were blindsided by a laughably prodigious task. It had taken *Chemical Abstracts* editors Patterson and Curran the better part of a year to form a complete system of nomenclature just for the collective index to that publication; the three editors were now asked to form such a system that would be applicable across different languages and genres of chemical text. They would have to do so in a matter of a few months. The commission had instructed that this report be sent to the six members of the existing working group early enough to allow them to examine it for two

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vue de formuler un système complet de Nomenclature," which might be taken as calling a) for the commission to present a report on how to go about formulating a complete system of nomenclature or b) for a report constituting such a system. The meaning that Pope and the commission attributed to the resolution might have been more clear in the context of the discussion at Lyon. However, since neither the IUPAC secretary-general who edited the minutes of the meeting nor two of the three editors called upon to carry out this task were present at the meeting, the ambiguity in the text was likely significant.

[107] Ibid., 24, 51.

[108] Ibid., 51.

months, send comments to the report's authors, and for the authors of the report to incorporate these comments into a collective report to be presented at the following summer's meeting in Cambridge.[109] Furthermore, alongside the demands of Pope and the Commission for concrete proposals, the editors had their own constituencies at home to satisfy – indexers and abstractors, editorial boards, society councils, authors of articles.

Crane, Greenaway, and Marquis began their report with a gentle rebuke to those who had assigned them their impossible task: "The elaboration of a complete system of nomenclature of organic chemistry demands much time, each detail requiring an attentive examination."[110] Fortunately, there was an ongoing collaboration on nomenclature that Crane and Greenaway could call upon to satisfy the Commission's demand for results. In late 1919, just as the initial plans for the formation of the IRC and IUPAC were being put into effect, Crane launched an effort to work with his British colleagues on matters of nomenclature.[111] At Crane's request, relayed by ACS President William Nichols, the Chemical Society of London and the Society for Chemical Industry formed nomenclature committees. Crane's aim, endorsed by the ACS leadership, was not to standardize or unify English-language chemical nomenclature, spelling, and pronunciation, but "to secure as large a measure of agreement in these fields as is practical."[112]

To this end, the committees exchanged the nomenclature guidelines that each society set forth for its abstractors and editors. By late spring 1922, a month or two before the Lyon meeting, the societies jointly published the results of their collaboration. It was emphatically not a system, but rather ten rules "covering the more important points concerning which there

[109] Ibid.

[110] IUPAC Comptes Rendus 4 (1923): 81.

[111] This may have been connected to American collaboration in the British compendia plans, discussed above.

[112] *Proceedings of the American Chemical Society* (1919): 142; Chemical Society Council Minutes, 30 Oct 1919, RSC, AR0148. As president of the Chemical Society in 1919 and the Society for Chemical Industry in 1920, Pope must have been aware of this collaboration, knowledge of which might have inspired his intervention in Lyon.

are more or less common differences in practice." As a set of answers to frequently asked questions on nomenclature, the rules resembled the eight meager rules established at the Paris Congress back in 1889. Nonetheless, as a binding international agreement between chemical societies upon the nomenclature to be used in their publications, it was unprecedented.[113]

Of these ten rules, Marquis judged seven acceptable to French chemists.[114] As the rules had meanwhile become permanent and binding in the US and the UK, they were rather a dead end when it came to negotiating international standards. Still, building their report around these rules gave Crane, Greenaway, and Marquis a foundation of concrete accomplishment on which to base their promise that "an important preliminary work has been carried out, but cannot yet be presented."

Once again in 1923, however, the commission did not use the annual meeting to build on the proposals presented in the report submitted to it. Neither Crane nor an ailing Greenaway attended the Cambridge meeting.[115] Notwithstanding Pope's injunction that all proposals pass through the committee of editors, the Dutch commission submitted a report containing many specific recommendations and yet another plan for organizing nomenclature work. The Dutch chemists recommended dividing the field of organic nomenclature into eighteen subsections to be studied by separate countries whose proposals the working group would be charged with harmonizing.[116]

Instead, the mercurial Commission on Organic Nomenclature reconfigured itself once more. In Cambridge, even fewer of its titular members were present. They were outnumbered

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[113] Verkade, *A History*, 99.

[114] Or "all but three," as the report optimistically presented it; IUPAC Comptes Rendus 4 (1923): 81.

[115] Greenaway reported to Gérard in early 1924 that an illness had necessitated a period of "complete rest"; Greenaway to Gérard, 10 Feb 1924, IUPAC papers, Box 51.

[116] IUPAC Comptes Rendus 4 (1923): 82–86.

by replacements, one of whom, Béhal, presided.[117] This group nonetheless took action. It resolved to add representatives of the leading Italian, Dutch, and Swiss chemical journals to the commission of three editors selected the previous year. The members of this new working group would not necessarily be editors-in-chief, but rather suitable delegates selected by the editorial board of each journal. Perhaps in response to the unsolicited Dutch report presented at the conference, the commission reiterated that all questions or suggestions pertaining to organic nomenclature were to be submitted to this working group of six editors (no mention was made of the group that had been referred to as the "working group" for the previous two years). The six editors were authorized to schedule separate meetings apart from the Union's annual conference, in order to carry on their work.[118] The six editors were to arrive at all decisions unanimously, and these decisions would be subject to the approval of the conference. To these procedural resolutions, the Commission added one more: "The Geneva Nomenclature will be taken as the foundation of further discussions." The Commission added emphatically (and, given its track record, prudently), "this resolution will be passed along to the committee of six editors."[119]

These decisions were remarkable for a few reasons. First, the Commission once again redefined the expert in organic nomenclature. Nomenclature expertise was no longer to be treated as something that inhered in a journal editor *ex officio*, but rather a quality that a

[117] The minutes show that only three of the commission's seventeen titular members were among the fourteen in attendance at the meeting. It is possible that Pope, the Union president, and Biilmann, a vice-president, were present but not counted in this list.

[118] The Commission requested special funds from the Union to support these meetings, but no such appropriation was made. In 1922, the IUPAC finance committee had allocated 9,000 francs to be divided among the three nomenclature commissions. However, the perpetually cash-strapped Union allocated no further funds for nomenclature work until 1927, when it began partially reimbursing certain working group members for their travel. IUPAC Comptes Rendus 3 (1922): 41; Holleman to Gérard, 22 Mar 1927, Gérard to Norris and Gérard to Pictet, 18 Aug 1927, Gérard to Holleman, 14 Sept 1927, IUPAC papers, Box 52. As Verkade, who had a vested interest in the matter, put it, "Many years would pass before the Union understood that it was necessary and natural that it should look after the expenses of the members of the commissions." Verkade, *A History*, 194.

[119] IUPAC Comptes Rendus 4 (1923): 25.

journal editor was best positioned to recognize – or perhaps a quality that could only be realized in someone who had the full and explicit confidence of a journal editor. The editors took advantage of this leeway. Though the seventy-two year old Greenaway had retired as editor of the *Journal of the Chemical Society* after forty years as a salaried sub-editor and editor, he agreed to continue to advise the editorial board on matters of nomenclature and to serve on the IUPAC working group.[120] E. J. Crane selected his friend and mentor Austin Patterson as the representative of *Chemical Abstracts*.

Second, the decisions rewrote the history of organic nomenclature in a manner that would strongly affect its subsequent development. The report of the three editors had already begun to do so. Whereas Blaise's report of the previous year presented a program for reform picking up precisely where Jacobson's working group had left off in 1914, the editors' report, strenuously aiming at practicality and consensus, presented only "certain rules" agreed upon by its American, British, and (in some cases) French authors, strategically avoiding relating these rules to prewar proposals. The commission took the space that Crane and his fellow editors had cleared out and filled it with the Geneva rules, stipulating these as the sole and mandatory basis of all nomenclature reform to follow.

Third, and perhaps most importantly, with these resolutions the commission legislated itself out of significance, at least for a little while. It did not impose a timetable on the working group, and encouraged its members to meet away from the annual conferences. Indeed, the wording of the resolutions regarding oversight of the working group made it unclear whether the commission had any role to play at all – was the "Conference" to which the editors were to

[120] Chemical Society Council Minutes, 18 Dec 1884, RSC, AR0143; Greenaway to Gérard, 10 Feb 1924 and 10 Mar 1924, IUPAC papers, Box 51. Greenaway was first hired as sub-editor after the death of Henry Watts in 1884. As sub-editor, Greenaway had been responsible for editing abstracts and preparing the annual index, and thus for enforcing the nomenclature rules developed by Watts to govern Chemical Society publications, as described in Chapter 3. Henry Armstrong, report appended to Chemical Society Council Minutes, 27 Mar 1895, RSC, AR0144.

submit their unanimous decisions the Commission on Organic Nomenclature, the IUPAC Council, or the General Assembly? That would have to be worked out later. For now, the commission had laid the groundwork for the formation of a working group made up of a stable collection of members, who were volunteers rather than conscripts, self-selected or chosen for their experience in matters of organic nomenclature, empowered to construct a system of nomenclature for the purposes and in the order that they pleased – as long as it built upon the Geneva rules.

Given the precedent of the previous years, it was far from certain that this committee of editors would come together as the commission planned. This time, however, one chemist was determined that it would.

Names as swords, names as plowshares

William A. Noyes had attended the Lyon and Cambridge meetings of IUPAC, participating in the organic nomenclature commission sessions on behalf of Crane and Conant, the titular commission member. After the Union meeting in Cambridge in June, 1923, Noyes remained in England, and in September, he decamped for Paris, where he remained through the end of the following summer.[121] While Noyes was in Europe, his longtime friend Austin Patterson was completing a draft of a system for numbering ring compounds, a project that Patterson been working on for two years under the auspices of a joint NRC-ACS committee of which Noyes was also a member. Patterson, the architect of the index nomenclature used in *Chemical Abstracts*, was known as America's foremost authority on chemical nomenclature and one of the world's leading experts in the field, and the numbering of rings and ring systems was a pressing problem whose complexity demanded the attention of such an expert. In their 1923

[121] On Noyes' itineraries, see William A. Noyes, *Building for Peace: A Chemist's Summer in Europe,* (New York: Chemical Catalog Co., 1923), 4–6; William A. Noyes, *Building for Peace II: International Letters,* (New York: Chemical Catalog Co., 1924); Noyes to Gérard, 13 Aug 1924, IUPAC papers, Box 51.

report to the IUPAC commission, Cain, Greenaway, and Marquis had mentioned Patterson's work on the subject; however, due to illness, Patterson had been unable to submit the latest version of his system for their review.[122] By November, Patterson had completed these revisions, and Noyes wished to pass them along to the members of the new committee of editors.

Noyes wrote to Jean Gérard, secretary-general of the Union, asking whether the members of the organic nomenclature committee had selected a president to whom he could pass along Patterson's work.[123] Gérard responded by describing the composition of the working group – a necessary preface, given how frequently it had been changing – and instructing Noyes to pass along all suggestions to Crane, his nation's representative.[124] Instead, Noyes wrote independently to all of the members of the group, "in an endeavor to get the Committee started." When Gérard wrote to the working group members in mid-December to suggest that they select the Dutch member Arnold Holleman as their president, he discovered that Noyes had already made the same suggestion. Explaining that he had obtained the approval of Greenaway, Marquis, Pictet, and Crane for the nomination of Holleman as president of the working group, Noyes requested that Gérard officially appoint the Dutchman. "With your notification of Professor Holleman," Noyes promised, "my own connection with the matter, which has been entirely informal and unofficial, will cease."[125] Gérard did so, likely hoping that Noyes would hold to his promise.[126]

Three weeks later, Noyes wrote to Marquis and Gérard to set up a meeting between Holleman, Marquis, Gérard and himself. Holleman, Noyes related, had numerous questions

[122] IUPAC Comptes Rendus 4 (1923): 81
[123] Noyes to Gérard, 15 Nov 1923, IUPAC papers, Box 51.
[124] Gérard to Noyes, 17 Nov 1921, IUPAC papers, Box 51.
[125] Noyes to Gérard, 2 Jan 1924, IUPAC papers, Box 51.
[126] Gérard to Holleman, 5 Jan 1924, IUPAC papers, Box 51.

about the commission's authority and the logistics of its work – some suspiciously close to questions that Noyes had previously posed, and Gérard had not answered.[127] The day after this meeting, Noyes wrote Patterson to describe the workings of the committee and, in particular, the favorable attitude of the Dutch and French chemists toward the publication of Patterson's rules for ring-numbering. He encouraged Patterson to attend the upcoming meeting of the committee of editors in April and the June meeting of the Union in Copenhagen, but volunteered to serve in his stead if Patterson could not attend.[128] Gérard was not rid of Noyes yet.

Noyes' intervention in the work of the organic nomenclature commission was part of a broader "informal and unofficial" agenda that he pursued during his sojourn in Europe, dedicated to promoting international understanding, disarmament, the settlement of reparations, and the construction of a more robust international order fully incorporating the former Central Powers – especially Germany – on an equal basis. Noyes spent much of his time in Europe meeting and corresponding with his chemist colleagues on all sides of on the major political controversies fracturing postwar Europe – the intricacies of reparations negotiations, the French occupation of the Ruhr, the responsibility for the burning of the library at Louvain, the production and use of war gases. He published numerous articles and two books, both in America and Europe, in which he reproduced his exchanges and attempted to reconcile the differences of opinion expressed by his correspondents, always taking pains to call attention to the role of misunderstandings and military-industrial interests in perpetuating antagonism

[127] Noyes to Marquis (copy to Gérard), 25 Jan 1924, IUPAC papers, Box 51. For example, Noyes passed along a question from "Holleman" that Noyes had asked in the beginning of January regarding the relationship between working groups for inorganic and organic nomenclature. Other correspondents seem to have understood that the committees would be separate.

[128] Noyes to Patterson, two letters of 1 Feb 1924 (copies to Marquis, Gérard, and Holleman), IUPAC papers, Box 51.

among the former adversaries.[129] He wrote to Secretary of Commerce Herbert Hoover proposing a plan for revising reparations and resolving the Ruhr crisis, and to Senator Henry Cabot Lodge, Senator William McKinley, and President Calvin Coolidge urging the US to join the World Court.[130] Most of his colleagues saw Noyes' political efforts as well-intentioned but naïve. In a 1923 letter, ACS secretary Charles Parsons pithily expressed the general response to Noyes' mission to untie the knots of postwar resentment: "I hope you will be able to solve it, but I do not expect you will."[131]

Noyes worked more directly to effect reconciliation within the international chemical community, a step that he saw as crucial for the advancement of both peace and science.[132] Already in 1922, there was a growing movement among chemists from many nations to begin this process of reintegration. During the week prior to the meeting of IUPAC in Lyon at the end of June, Noyes attended a gathering in Utrecht organized and hosted by Dutch chemist Ernst Cohen, bringing together forty scientists from ten nations including Germany (the French and Belgian invitees pointedly declined to attend).[133] A month later, Noyes attended a meeting of the IRC in Brussels, where the Swedish delegation introduced a resolution to open the council

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[129] Noyes, *Building for Peace*; Noyes, *Building for Peace II*; William A. Noyes, "'America First,'" *Journal of Industrial & Engineering Chemistry* 14, no. 12 (December 1, 1922): 1170–1171.

[130] Noyes to Hoover, 18 Oct 1923, W. A. Noyes papers, Box 5, Folder H-J; Noyes to Lodge, 19 Apr 1924 (copies to Coolidge and McKinley), W. A. Noyes papers, Box 5, Folder K-L. Hoover responded, "I think your suggestion is a good one but whether it would be practical to have it carried out or not is, of course, another question." Hoover to Noyes, 29 Oct 1923, W. A. Noyes papers, Box 5, Folder H-J.

[131] Parsons to Noyes, 16 Aug 1923, W. A. Noyes papers, Box 5, Folder M-P.

[132] In this connection, Noyes wrote of both "the desperate need for the renewal of acquaintance between men in different countries who have a sincere desire to find the way to better world-conditions" and the "great need that men should once more unite their forces and work together for the advancement of knowledge." Noyes, *Building for Peace,* 22–23.

[133] Ibid., 4. On this meeting, see Jorrit Smit, "Nuclei in a Supersaturated Solution: Utrecht Chemists and the Crystallization of International Relations after the First World War," *Studium* 7, no. 3 (2014): 190–208.

The choice to join the IRC in the first place had been controversial in Dutch scientific circles. Throughout the 1920s, Cohen and other Dutch scientists worked within and around the IRC and its Unions toward the goal of securing German and Austrian membership in these organizations. See Geert Somsen, "'Holland's Calling': Dutch Scientists' Self-Fashioning as International Mediators," in *Neutrality in Twentieth-Century Europe: Intersections of Science, Culture, and Politics after the First World War*, ed. Rebecka Lettevall, Geert Somsen, and Sven Widmalm (New York: Routledge, 2012), 45–64.

and its affiliated unions to all nations. Noyes believed that the motion could have passed, but under the intense opposition of French and Belgian delegates, it was tabled. The Belgian chemist Frédéric Swarts was particularly opposed to admitting German scientists until the living signatories of the Manifesto of the Ninety-Three had publicly repudiated it.[134] Writing to Noyes the following summer, Grignard expanded upon the reasons for French resistance to the readmission of Germany into the scientific and political international order. On the one hand, Grignard suspected the Germans of a persistent desire to impose hegemony, expressed in the overweening ambition of such undeniably valuable work as *Beilstein*. On the other, he accused the US and England of seeking to further their own interests by withdrawing from conflict with Germany and even "cultivating the animosity between France and Germany."[135]

For Noyes, who had long cared deeply about both chemical publications and international peace, nomenclature reform had a double significance. First, German expertise would provide an invaluable contribution to nomenclature reform. Indeed, Noyes and Patterson had solicited the advice of Jacobson and Stelzner regarding Patterson's ring-numbering scheme.[136] Further, Stelzner and his collaborator Hedwig Kuh had published a hundred-page treatise on nomenclature at the beginning of the 1921 volume of Stelzner's *Literatur-Register*.[137] Second, a system of nomenclature developed by IUPAC without German input would have to be entirely renegotiated once Germans finally were incorporated into the Union. Flipping this argument around, these two factors made the exigencies of nomenclature

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[134] Noyes, *Building for Peace,* 21–22.

[135] Grignard to Noyes, 4 Aug 1923 and 7 Sept 1923, in Noyes, *Building for Peace II,* 9, 14.

[136] Austin M. Patterson, "Proposed International Rules for Numbering Organic Ring Systems," *Journal of the American Chemical Society* 47, no. 2 (1925): 543.

[137] Robert Stelzner and Hedwig Kuh, "Nomenklatur-Fragen: I. Über eine vereinfachte Bezeichnungsweise von Kohlenstoff-Resten. — II. Materialien zur Nomenklatur und Bezifferung cyclischer Systeme," in *Literatur-Register der organischen Chemie*, vol. 3 (1914–1915) (Berlin: Springer, 1921), 1–100. The *Literatur-Register* was the continuation of Richter's *Lexikon*, an index of organic compounds arranged by empirical formula with select literature references. For more on these publications, see Chapter 3.

reform a useful means of breaking down the social barriers isolating German scientists and facilitating the frequent, friendly discourse that Noyes believed necessary for achieving his political goal of mutual understanding among the former enemy nations.

Having inserted himself into the business of the IUPAC organic nomenclature working group during the winter of 1923-24, Noyes used his position to try to make room for German involvement. He began modestly, asking Patterson whether he might publish his list of rules and examples of ring numbering in a trilingual English, French and German edition, since "we aim at something so permanent that the Germans should certainly be included."[138] This was a linguistic internationalism as insurance against the future obsolescence of hard work on nomenclature. In a letter to the members of the working group, Noyes wrote, "it is very clear that no genuine international and permanent conclusions can be reached and put into effect without the cooperation of German chemists."[139] Accordingly, he asked the editors to indicate whether or not they approved of adding informal members appointed by the journals of the German and Austrian chemical societies to the working groups, to confer with the official members by correspondence.[140] A month later, Noyes wrote to Gérard to inform him that all of the committee members other than Greenaway had agreed to pursue this collaboration. Greenaway had resisted, Noyes reported, on consultation with Union president Pope, who had considered the exclusion of German chemists from the Union to bar formal or informal collaboration by the Union's committees. An indignant Noyes asked, "Do you think that our Committees must be bound by the opinion of Sir William Pope in a matter which is of such vital

[138] Noyes to Patterson, 1 Feb 1924, IUPAC papers, Box 51.

[139] A similar committee of editors had been appointed to address inorganic nomenclature; Noyes sent copies of his letter to its members as well.

[140] Noyes to Members of the Committees on Nomenclature, 17 Feb 1924, IUPAC papers, Box 51.

importance?"[141] Gérard, taken aback by Noyes' open invitation to undermine the authority of the Union president, suggested that Noyes present his request directly to Pope.[142]

Noyes did so, buttressing his argument by quoting the estimate of Paul Kestner, President of the Société de Chimie Industrielle and co-originator of the idea behind IUPAC, that three-fourths of French chemists were ready for the admission of Germans to the Union. Noyes also appealed to the cause of order at the annual conference, suggesting that informal cooperation would be preferable to an acrimonious discussion of the matter at the upcoming IUPAC meeting in Copenhagen.[143] Pope replied that any such rapprochement should take place not merely with the assent of the French but under French leadership.[144] Noyes agreed, in principle. However, he argued that scientists should be proactive about mending fences even under politically-challenging circumstances, so as to "lead in ways of peace and not merely follow in the wake of financiers and politicians." Further, he reminded Pope of the risk that failing to consult Germans posed to the work of the Union's nomenclature commissions:

> I should be little interested in the work of these Committees if I did not hope that we may reach conclusions which will result in the unification of usage throughout the world. Probably one-half of the chemists of Europe live in Germany and Austria. It seems to me preposterous that we should expect to establish rules which they will accept if we do not give them some opportunity to consult with us in the preparation of these rules.[145]

Meanwhile, Noyes opened another front in the battle, writing Gérard and the IRC secretary, the Briton Arthur Schuster, to inquire whether Austria's entrance into the League of Nations entitled Austrian chemists to membership in IUPAC.[146] Gérard replied that Austria had

[141] Noyes to Gérard, 17 Mar 1924, IUPAC papers, Box 51.
[142] Gérard to Noyes, n.d. [Mar 1924], IUPAC papers, Box 51.
[143] Noyes to Pope, 20 Mar 1924, IUPAC papers, Box 51.
[144] Noyes to Pope, 29 Mar 1924, IUPAC papers, Box 51.
[145] Ibid.
[146] Noyes to Gérard, 13 Mar 1924, IUPAC papers, Box 51.

to be admitted to the IRC first.[147] Schuster explained that the statutes of IUPAC and its sister unions made a country's membership contingent on membership in the IRC, and that IRC statutes effectively forbid the former Central Powers from joining.[148] Noyes followed up with Schuster, asking whether informal collaboration with Austrian and German editors on nomenclature matters might be permissible and whether, with French assent, a German chemist might be invited to deliver a scientific lecture in Copenhagen.[149] Noyes also requested Gérard's "personal, not official, opinion" regarding the acceptability of the latter plan to the French.[150] Gérard once again parried Noyes' rather awkward inquiry, claiming that in his own "role as Secretary General of the Union, I am in some sense internationalized" and referring Noyes to the presidents of French chemical associations.[151]

A couple of weeks later, Béhal put an unequivocal stop to Noyes' machinations:

> I have consulted informally the majority of my French colleagues from the Committees for the Reform of the nomenclature of mineral chemistry, of organic chemistry and of biochemistry on the subject of the renewal of relations between the chemists of our country and the German chemists on the subject of studies of nomenclature reform. They consider that there is no reason to resume relations, even unofficial, with the German chemists as long as they are not members of the International Union of Chemistry.[152]

[147] Gérard to Noyes, 14 Mar 1924, IUPAC papers, Box 51.

[148] Schuster to Noyes, 21 Mar 1924, IUPAC papers, Box 51.

[149] Noyes to Schuster, 22 Mar 1924, IUPAC papers, Box 51. Noyes argued that such a German address might be particularly useful in Copenhagen given the greater familiarity of most Danes with German than French. Danes were not the only ones whose participation in IUPAC was limited by the predominance of French at its meetings. Sending thanks (in English) to Gérard after attending the Copenhagen meeting, Japanese chemist Riko Majima admitted that he "couldn't well understand the official language spoken at the meeting." Riko Majima to Gérard, 31 Jul 1924, IUPAC papers, Box 51.

[150] Noyes to Gérard, 22 Mar 1924, IUPAC papers, Box 51.

[151] Gérard to Noyes, 24 Mar 1924, IUPAC papers, Box 51.

[152] Béhal to Noyes, 10 April 1924, IUPAC papers, Box 51.

Conclusion

Discussing the Utrecht meeting that Noyes attended in 1922, the historian Elisabeth Crawford writes, "One is struck by the fact that those initiating the contacts were motivated less by a strong need for practical international cooperation in the area of communication of research results, for example, than by a sincere desire to show the way in an effort to reconstruct not just international science but other international cooperative efforts as well."[153] For Noyes, however, communication and cooperation went hand in hand. To the American chemist, the standardization of organic nomenclature was inseparable from the reintegration of Germany into international chemistry.

On one front, his efforts paid immediate dividends. Aided by Noyes' prodding, the IUPAC organic nomenclature working group had come together as planned. The stage was set for the resumption of work on an international system of organic chemical nomenclature, planned since 1911 but delayed for thirteen years by organizational challenges, editorial interests, war, and economic depression. On the front of international scientific diplomacy, he encountered resistance.

Over the following years, however, this resistance slowly thawed. In April 1924, Noyes managed to secure the agreement of Holleman and Gérard that the members of the organic nomenclature working group would not be explicitly forbidden from personally consulting their German colleagues.[154] A heated discussion at the 1925 IUPAC conference resulted in a resolution requesting that the IRC modify its statutes to admit any member of the League of Nations to the Council as well. To secure its unanimous approval, Union leaders had added the qualifier "eventually," rendering the resolution practically meaningless; still, it was a

[153] Crawford, *Nationalism and Internationalism in Science, 1880-1939*, 68–69.

[154] Verkade, *A History*, 128. Marquis and Gérard agreed that if the minutes of this meeting were printed, all discussion of even the possibility of personal collaboration with the Germans would be suppressed; Marquis to Gérard, 1 May 1924, IUPAC papers, Box 51.

symbolically important step.[155] At the following year's meeting in Washington D.C., the new Union president Ernst Cohen – the host of the 1922 Utrecht meeting – read a cable sent from the IRC meeting announcing that the council would be opened to the former Central Powers.[156] The path for German entry into IUPAC was finally clear. Getting them to join, however, was another matter – one that would depend in part, as Noyes had foreseen, on the system of organic chemical nomenclature that Holleman's committee was engaged in developing.[157] We will turn now to the work of this working group.

[155] IUPAC Comptes Rendus 6 (1925): 64–65.

[156] As reported in IUPAC Comptes Rendus 9 (1928): 15.

[157] On the German response to the IRC boycott, see Schroeder-Gudehus, *Deutsche Wissenschaft und internationale Zusammenarbeit*; Schroeder-Gudehus, "Challenge to Transnational Loyalties," 104–114; Gordin, *Scientific Babel*, 176–178, 183–185.

Chapter 6

Negotiating Nomenclature in the International Union of Pure and Applied Chemistry, 1924-1940

On the morning of July 19th, 1928, the commission on organic nomenclature of the International Union of Pure and Applied Chemistry (IUPAC) assembled in The Hague to establish a set of international rules for naming organic chemical compounds. It had been thirty-six years and three months since the Geneva Congress had convened for the same purpose. Linking the fifty chemists in the room to the 1892 congress were three veterans of Geneva – Auguste Béhal, Henry Armstrong, and Amé Pictet – and the commission's self-imposed directive to "base its discussions on the Geneva nomenclature."[1]

The nomenclature debates of the Geneva Congress had been deliberate and collegial. However, the intellectual, institutional, and geopolitical conditions of international chemistry had changed considerably since 1892. Arnold Holleman, chair of both the commission and the working group that had drawn up a set of proposed nomenclature rules, called the meeting to order. The organic chemist Victor Grignard immediately took the floor.[2] The French Nobel laureate summarized his objections to the working group's rules, which Grignard had arranged to be circulated to the commission members along with the report of the working group. In Grignard's opinion, Holleman and his colleagues had yielded to "Anglo-Saxon illogic" and

[1] Amé Pictet, Agenda entries of 19–21 April 1892.

[2] Arnold F. Holleman, "Letter of Greeting," in *Chemical Nomenclature*, Advances in Chemistry 8 (Washington, DC: American Chemical Society, 1953), 3.

betrayed the spirit of the Geneva nomenclature.[3] Béhal, one of the primary architects of the Geneva rules, seconded Grignard's critiques, "with a great vehemence."[4]

Other chemists arose to extemporize in three languages upon further critiques and alternative procedures. Some seemed not to have read the report supposedly under discussion. Curious onlookers came and went as they pleased.[5] At one point, Armstrong presented an entirely different system of nomenclature that he had prepared on his own. Holleman cut him off, informing Armstrong that only the pre-circulated reports were to be discussed, whereupon Armstrong "shut his portfolio with a bang and disappeared."[6] The noisy departure threw the assembly into even greater confusion, and Holleman abruptly called an end to the meeting.[7]

That evening, Pictet wrote in his diary, "I was almost alone in keeping my head. We don't know how to move on and we decide nothing. The working group – Holleman, Marquis, and myself – is very discouraged."[8] To Pictet, the events of nomenclature committee meeting were typical of the "confused and degrading" state into which the Union as a whole had fallen. Proposed as a possible candidate for IUPAC's presidency, Pictet declined the nomination. "For me," he wrote, "this is the end of the Union. I am not sorry, for this meeting at The Hague left me a little disgusted."[9]

Two years later, the IUPAC nomenclature commission – Grignard included – unanimously approved a collection of nomenclature rules virtually identical to those so hotly contested at The Hague. The rules of the "Liège nomenclature" and the procedure by which

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[3] Grignard to Gérard, 20 May 1928, IUPAC papers, Box 52.

[4] Pictet, Agenda entry of 19 Jul 1928.

[5] Pieter Eduard Verkade, *A History of the Nomenclature of Organic Chemistry*, trans. S. G. Davies (Boston: Reidel, 1985), 131–32.

[6] Holleman, "Letter of Greeting," 3.

[7] Verkade, *A History*, 131.

[8] Pictet, Agenda entry of 19 Jul 1928.

[9] Pictet, Agenda entry of 20 Jul 1928.

they were written and approved became the basis for the incremental development of IUPAC nomenclature through the present day. How was it that organic nomenclature aroused such passionate disagreement during the 1920s? How were the disputes that dominated the meeting at The Hague so rapidly and decisively resolved?

During the 1920s, the IUPAC commission on organic chemical nomenclature became a site for negotiating the uncertain future of international chemistry. In order to justify the continued existence of IUPAC, Union leaders and outside observers alike felt that IUPAC had to, first, have something to show for its efforts in the international organization of chemistry, and second, find a way to bring German chemists into the fold. Organic nomenclature reform both required rapprochement with Germany and presented an opportunity to establish concrete standards in an area of well-known disorder. However, various delegates had very different ideas about the proper form of nomenclature standards and of the Union's relationship with Germany. Attempts to take definitive action on either front led to escalating confrontations that threatened to tear the Union apart.

Between 1924 and 1927, Holleman and his working group built up the set of nomenclature proposals that were debated at The Hague and approved at Liège. The process by which they did so, as well as the content and framing of the group's proposed rules, constituted a multilayered interpretation of the group's mandate to "base its discussions on the Geneva nomenclature." So did the grounds on which Grignard and others challenged the proposals of the working group. The debate over nomenclature rules was a debate over the legacy of the Geneva Congress, and what it would mean to advance or undermine it.

Holleman, the working group, and Grignard were wrestling with a question central to an effort to reform a system of scientific nomenclature, or indeed to any effort to replace a set of standards: what does it mean to create one set of rules "based on" another? How can a new

system neither extend an existing one, nor replace it wholesale, but supercede the current scheme? How can reformers preserve (at least the impression of) continuity with the standards to be reformed without (at least appearing to) succumb to the same drawbacks that motivated the reform effort in the first place? In response to the 1928 impasse, Holleman marshaled support for his rules by articulating their capacity to put chemists as well as chemicals in order. With the future of chemical nomenclature, chemical reference works, and international chemistry in the balance, Holleman secured the approval of his nomenclature rules by negotiating a new configuration of authority over nomenclature rule-making.

Rewriting the Geneva Nomenclature

On a summer evening four years before the chaotic meeting at The Hague, Amé Pictet joined a cheerful party of aging chemists at a café in Copenhagen. The fifth annual meeting of IUPAC was to begin the next day; this was the reunion before the *réunion*, and liqueurs were flowing liberally. Pictet saw William Pope, hero of the British chemical warfare effort during World War I and current President of the Union, and Auguste Béhal, head of the French delegation to the meeting, both getting "a little tipsy."[10]

The previous year, with Pope and Béhal presiding, the Union's commission on the nomenclature of organic chemistry – an ad hoc gathering, as usual – had formally conferred responsibility for organic nomenclature reform upon a six-member working group.[11] As ever in the early years of IUPAC, despite the paramount importance of German chemistry and chemical literature, there were no Germans in the working group, or at the café that evening. Instead, the committee, which would meet for the first time the following day, was composed of representatives of the leading American, British, Dutch, French, Italian, and Swiss chemical

[10] Pictet, Agenda entry of 26 Jun 1924.

[11] The Commission had requested funding to support the meetings of the working group in 1923; the Union began providing it only in 1927. See Chapter 5, note 118.

journals.[12] As one of these representatives, Pictet was about to embark upon three years of meetings, drafts, and proposals revisiting the work of the Geneva Congress – or, more specifically, revisiting the account of that work that Pictet himself had written in May of 1892.

Pictet represented the Swiss journal *Helvetica Chimica Acta*, which he had co-founded as a politically and linguistically inclusive outlet for publication during the First World War.[13] Pictet had enjoyed a productive career at the University of Geneva, where he had held the chair in organic chemistry since 1907. At the cusp of sixty-seven he remained active in research and teaching; in 1926, his name was even mentioned in a list of the world's leading synthetic chemists (though not at the top of the list).[14] He had been one of the members of the Swiss subcommittee to the Committee on Organic Nomenclature organized by the short-lived International Association of Chemical Societies in 1912, and had participated in the organic nomenclature meetings at the preceding three IUPAC conferences. None of these groups had advanced beyond organizational stages, but it was as much experience in matters of nomenclature as anyone in Switzerland could boast.

The president of the working group was its Dutch representative, Arnold Holleman.[15] Holleman was professor of chemistry at the University of Amsterdam and Vice-President of the Dutch Royal Academy of Sciences. Holleman was fluent in English, German, and French, as well as his native Dutch; he was also an Esperanto enthusiast.[16] He had written a popular textbook

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[12] IUPAC Comptes Rendus 4 (1923): 25.

[13] "Résolutions sur le journal de Chimie prises à Berne le 20 Oct 17," BGE, Ms. Fr. 3459, 52; Philippe A. Guye, "Avant-Propos," *Helvetica Chimica Acta* 1 (1918): 3–4.

[14] F.C. Whitmore to Noyes, 5 Nov 1926, W. A. Noyes papers, Box 6, Folder 1926 [I]. The leading vote-getting in the survey was the German chemist Richard Willstätter.

[15] On Holleman's election as working group president, see Chapter 5.

[16] Verkade speculated that Holleman's admiration for the standardized word inflections of Esperanto inspired him to ask Pictet to compile a list of commonly misused chemical suffixes for the working group's report; Verkade, *A History*, 168. Indeed, even three decades later, Holleman had not given up on Esperanto as an international scientific language, though he acknowledged that "it will certainly be several decades, perhaps even a century, before Esperanto is everywhere used by them [scientists]." Holleman, "Letter of Greeting," 3.

of organic chemistry, then in its seventeenth German and sixth English editions. His linguistic abilities and textbook-writing experience had likely inspired his interest in nomenclature. Holleman had been the principal author of a report on organic nomenclature submitted to the Union by a Dutch commission the year before. The report had taken a conservative stance with regard to the introduction of systematic names, arguing for the value of retaining trivial names wherever possible. The report also divided organic compounds into nineteen different groups, and proposed that each be assigned to one of the Union's nineteen member countries.[17] When it came to developing a system of nomenclature, Holleman was no stickler for top-down logic or internal consistency.

The American representative was Austin Patterson. As discussed in Chapter 3, Patterson had succeeded William A. Noyes as editor of *Chemical Abstracts* in 1910. He soon passed on the editorship to his deputy E.J. Crane, but he remained closely involved with the work of the abstract journal, developing the system of nomenclature used to organize the publication's Collective Index. Though he also taught at Antioch College, a small school near his even smaller hometown of Xenia, Ohio, he continued to spend much time working on matters of nomenclature and notation for *Chemical Abstracts* and the American Chemical Society. He was regarded by many, including Holleman, as the world's leading (non-German) expert in organic nomenclature.[18]

Raymond Marquis, the longtime editor-in-chief of the *Bulletin de la Société Chimique de France*, represented his journal on the working group. He also served as the committee's

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On the efforts of Dutch scientists to reincorporate Germany and the former Habsburg nations into the institutions of interwar international science, see Geert Somsen, "'Holland's calling': Dutch scientists' self-fashioning as international mediators," in *Neutrality in Twentieth-Century Europe: Intersections of Science, Culture, and Politics after the First World War*, ed. Rebecka Lettevall, Geert J. Somsen, and Sven Widmalm (New York: Routledge, 2012), 45–64.

[17] IUPAC Comptes Rendus 4 (1923): 82–86.

[18] Holleman to Patterson, 20 Nov 1924, quoted in Patterson to Rose, 6 Jan 1925, Patterson IEB file.

secretary, an office that he was able to carry out particularly effectively thanks to his proximity to IUPAC general secretary Jean Gérard in Paris.

The representative of the British *Journal of the Chemical Society*, John Greenaway, had been a member of the journal's editorial staff for forty years. However, in late 1923, Greenaway retired suddenly from editorship of the journal after what a close colleague recalled as a "serious breakdown."[19] This did not prevent the Chemical Society from naming him as its representative to the working group, but it likely limited Greenaway's activity within the committee. "An English greybeard who does not know a word of French and does not seem particularly knowledgeable" was Pictet's private impression of Greenaway. As working group meetings were conducted mainly in French, Holleman had to translate for Greenaway when he attended. [20]

The final working group member was to be appointed by the *Gazzetta Chimica Italiana*. However, the Gazzetta's representative, Alberto Peratoner, never attended a meeting. His replacement was appointed only after the working group assembled for what would be the last time, in January 1927.[21]

On the opening afternoon of the Copenhagen conference, the working group held its first substantive meeting. Pictet, Holleman, and Marquis were joined by Patterson's friend and mentor Noyes, in Europe for the year, who attended on Patterson's behalf.[22] The Union resolutions that had created this committee and set it to work on the reform of organic

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[19] Charles S. Gibson, "Arthur John Greenaway," *Journal of the Chemical Society* (1939): 207–209 (quotation on 208).

[20] Pictet, Agenda entry of 5 Jan 1925. Greenaway's colleague Charles Gibson joined the working group for one of the meetings; he offered his opinion on a few matters but was little more active than his countryman. Pictet, Agenda entry of 3 Oct 1925; "Procès verbal des séances tenues à Paris du 3 au 6 Octobre 1925 par le Comité de Travail de la Nomenclature de Chimie Organique," typescript, n.d. [Dec 1925], BGE, Ms. Fr. 3423, 219–226.

[21] *Rapport du comité de travail de réforme de la nomenclature de chimie organique* (Paris: Union Internationale de la Chimie Pure et Appliquée, 1927), 4. Cited hereafter as *1927 Report*.

[22] Pictet, Agenda entry of 27 Jun 1924. As we saw in Chapter 5, Noyes took a keen interest in the organization of the working group, and was never shy about inserting himself into Union work in unofficial (or extralegal) fashion when he thought he could speed things along.

nomenclature gave no guidance as to what that work would entail – only that any decisions that the working group made had to be based on the Geneva nomenclature. Over the course of the spring, Noyes had by correspondence urged the working group to focus on *extending* the Geneva nomenclature. He especially wanted Holleman's committee to consider a set of rules that Patterson had been developing for numbering the atoms of ring systems, an important problem that Patterson and a few others were just beginning to try to address systematically.[23]

Holleman interpreted his mandate more literally. He took the agenda for the working group from the Geneva Rules themselves – specifically, from the original text of and commentary on the Geneva rules that Pictet had composed three decades before. The working group would consider each of the rules enumerated in Pictet's proceedings, retaining it or making any adjustments they deemed necessary, and then move on to the next one.[24]

Pictet jotted notes in his diary describing the three meetings in Copenhagen during which Holleman and his colleagues began this work. Undeterred by either the rejection of his suggested agenda or the fact that he wasn't actually a member of the working group, Noyes objected to everything. However, by making all of their decisions provisional, the group managed to allay his concerns enough to proceed on their march through the Geneva rules. By the end of the third meeting, the group had completed an updated collection of rules for the nomenclature of the hydrocarbons, the first section of Pictet's proceedings of the Geneva Congress and the cornerstone of the Geneva nomenclature.[25]

The group took up its work again in Paris the following January. Once again, Patterson was unable to attend, though this time, Greenaway was. Notwithstanding his disparaging

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[23] A pair of German chemists, Richard Stelzner and Hedwig Kuh, were working on this problem as well. Stelzner and Kuh were respectively the editor and indexer of the *Literatur-Register*, one of the prominent German reference works published by the Deutsche Chemische Gesellschaft; Verkade, *A History*, 101–115.

[24] Pictet, Agenda entry of 27 Jun 1924.

[25] Pictet, Agenda entries of 27, 28, 30 Jun 1924. Votoček joined in the meeting of 30 Jun. On the foundational role of hydrocarbon nomenclature in the Geneva rules, see Chapter 2.

remarks about the British chemist, Pictet found him a more congenial collaborator than the importunate Noyes – "Greenaway agreed with everything," he noted approvingly – and away from the distractions of the annual conference, the group made rapid progress. By the end of the three days of meetings, they had extended their updated rules beyond the scope of the Geneva nomenclature, covering not only functionally simple compounds but the compounds with multiple different functional groups that the Geneva Congress had set aside.[26] The working group members, Pictet wrote, "took our leave very satisfied with the work accomplished."[27]

Patterson was less satisfied. Since 1921, he had been working on developing a set of rules for determining a canonical numbering for ring systems – systems of one or more directly-connected closed chains of carbon and other atoms. Such ring systems, often highly resistant to chemical change, made up the core of many of the naturally-occurring and synthetic compounds of greatest interest to organic chemists, including some of great biological and industrial importance. In order to form rational names for these compounds that indicated the chemical relationships among them, it was crucial to have a means of indicating particular ring atoms to which a substituent (an atom or group of atoms attached to a "parent" ring or chain) was attached. In response to Pictet's skepticism about tackling such a complex problem, Patterson noted that since his numberings were independent of the names assigned to these ring systems, it was not "necessary or even desirable to wait until the nomenclature has been fully worked out before deciding on the numbering."[28]

Patterson submitted a new draft of his rules to the working group members in March 1924, but the working group did not discuss it. Instead, at Noyes' request, they gave their

[26] Pictet, Agenda entry of 6 Jan 1925.
[27] Pictet, Agenda entries of 6-7 Jan 1925.
[28] Patterson to Pictet, 9 Jan 1924, BGE, Ms. Fr. 3447, 72-73.

assent to the publication of the rules as provisional standards.[29] Holleman wrote to Pictet that the committee should set the numbering rules aside and continue working through the territory covered by the Geneva rules.[30] For Holleman, when it came to the setting the agenda for the group that he led, "basing discussions on the Geneva rules" meant using the printed text of these rules to establish what was in and out of bounds for the discussions of the working group. In working its way through Pictet's 1892 report, the group established the momentum that previous IUPAC nomenclature efforts had lacked, at the cost of ignoring issues that one of its members felt were most crucial to address.

Recognizing that Patterson was unlikely to be able to exercise much influence on the work of the commission by correspondence, Noyes wrote to the Rockefeller-funded International Education Board in late 1924 to request a grant of $1,500 to help enable Patterson to spend six months in Europe, so that he could collaborate in person with both the International Commission and German editor Richard Stelzner. The Board president passed along the proposal to Harvard organic chemist E.P. Kohler, who recommended honoring the request, as "the problem is urgent, [and] Stelzner and Patterson undoubtedly are more competent to handle it that anybody else."[31] The Board approved the grant.

With his family in tow, Patterson sailed for Europe in April 1925. He set his itinerary to allow him to meet with members of the working group one on one. In May and June, he traveled to England, Holland and Paris, meeting with Greenaway, Holleman and Marquis to discuss the list of nomenclature rules that Holleman had drawn up based on the minutes of the

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[29] Patterson to Pictet, 31 Mar 1924, BGE, Ms. Fr. 3447, 74. Austin M. Patterson, "Proposed International Rules for Numbering Organic Ring Systems," *Journal of the American Chemical Society* 47, no. 2 (1925): 543.

[30] Holleman to Pictet, 10 Nov 1924, BGE, Ms. Fr. 3441, 195.

[31] Kohler to Richards, n.d. [Dec 1924], Patterson IEB file. IEB president Wickliffe Rose wrote to both Simon Flexner of the Rockefeller Institute for Medical Research and the Harvard physical chemist T.W. Richards for advice on Noyes' request. Each independently passed along the inquiry to Kohler. Rose to Flexner, 18 Dec 1924, Flexner to Rose, 19 Dec 1924, Richards to Rose, 20 Dec 1924, Patterson IEB file.

first two working group meetings.[32] In writing to Pictet in the hopes of arranging another such meeting in Geneva, Patterson noted that "These personal discussions with the other members of the Committee have been very valuable to me and I believe they are also profitable for our Committee work." He noted that he and Marquis "find that we seem to be in accord on the majority of points and that our differences are not serious," further evidence that "any apparent differences of opinion between the English-speaking and French-speaking chemists of the Committee may spring at least partly from the difference in language, and if there are any possibilities of misunderstanding of this kind, … free, informal conversation between the members will do much to eliminate it."[33]

In August, Patterson circulated a revised draft of Holleman's list of rules, taking the liberty of rearranging, omitting, and adding rules in the interest of discussion, toward the end of making the rules "as good as we can make them."[34] Shortly thereafter, he visited Pictet in Geneva. Pictet found Patterson "very cheerful and cordial, speaking French fairly badly but comprehensibly." He invited the Patterson family to the Pictet country house in Dully, on the northern shore of Lake Geneva twenty miles from the city. After lunch, while Patterson's children and wife visited a friend at a Quaker school the next town over, Patterson and Pictet compared notes on the working group's rules. Pictet noted that the two were in agreement on many points, though he felt that Patterson had a tendency to lose himself in superfluous details.[35]

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[32] Patterson also attended the IUPAC annual meeting in Bucharest in June, taking Noyes' place as one of the six American members of the IUPAC council. However, none of the other members of the working group attended, and there was no discussion of organic nomenclature at the conference; IUPAC Comptes Rendus 6 (1925): 56.

[33] Patterson to Pictet, 22 May 1925 and n.d. [early Jun 1925], BGE, Ms. Fr. 3447, 76–78.

[34] Patterson to Pictet, 6 Aug and 22 Aug 1925, BGE, Ms. Fr. 3447, 79–80, 82.

[35] Pictet, Agenda entries of 2-3 Sept 1925. On the Quaker school in Gland, Switzerland, see Jennifer Taylor, "'Work of modest proportion': Ayton school: One Example of the Contribution of the Society of Friends to Saving the Refugees from Hitler," in *I Didn't Want to Float, I Wanted to Belong to Something: Refugee Organizations in Britain 1933-1945,* ed. Anthony Grenville and Andrea Reiter (New York: Rodopi, 2008), 154–155.

At the beginning of October, the working group met for four days in Paris (*Fig. 1*). In Pictet's reckoning, Patterson quickly exhausted the goodwill and the sense of accord that he had developed in his personal meetings with his fellow delegates over the summer. In his diary, Pictet wrote that Patterson presented "a heap of propositions, often strange, but some of which he was adamant about, and there was no peace to be had until they were accepted."[36] The second day of the meeting, Patterson proposed a nomenclature of aldehydes that Pictet felt cast doubt on the entire framework that the working group had already established. The following day, Pictet complained that the committee had set aside his own proposals for extending the rules to aromatic compounds in favor of wasting time on "endless details" and "inconsequential matters." "I am more and more disgusted," Pictet wrote that evening.[37]

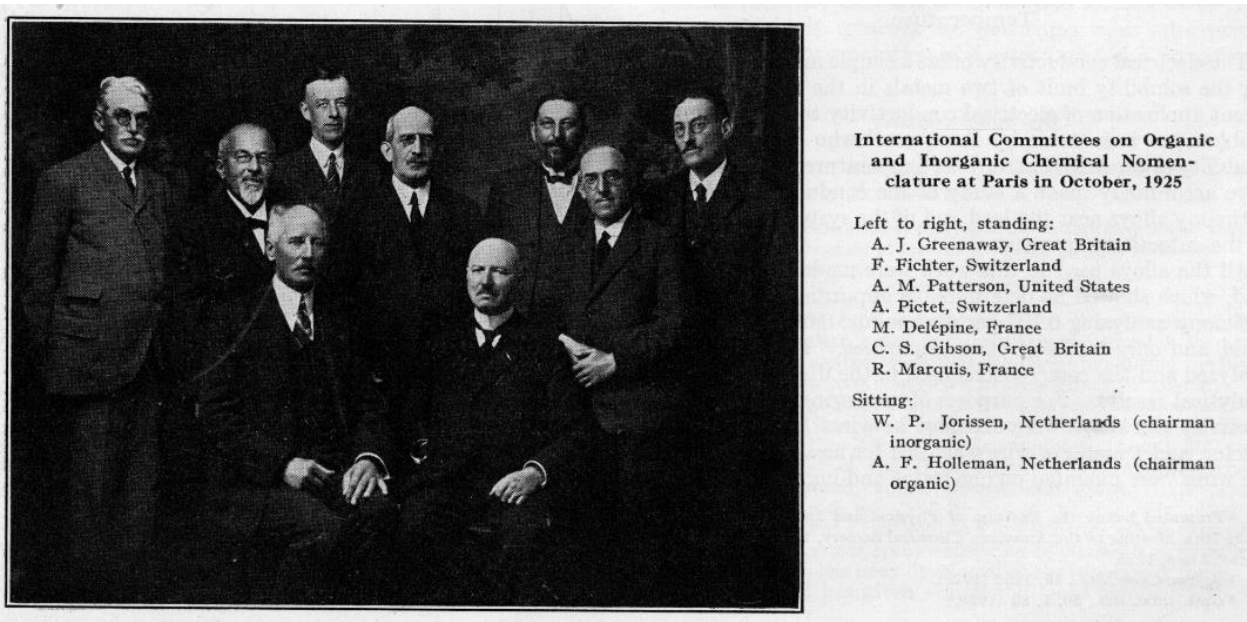
Nevertheless, the group carried on. Aside from ten rules for naming specific functional groups, set aside for later discussion, the working group completed its review of its working list of rules, modifying the text in numerous places and adopting several new rules proposed by Patterson.[38] In January, Holleman added to these a sketch of about a dozen more rules "supplementing those that we have already adopted," and sent the resulting eight-page typescript of sixty-eight rules to the members of the committee.[39]

[36] Pictet, Agenda entry of 3 Oct 1925. This might have been sour grapes, at least in part. During the January 1925 meeting, the working group members had agreed to Pictet's proposal to name compounds with multiple functional groups using multiple suffixes. At the October meeting, however, Patterson won the group over to a scheme using one suffix only, as in the system used in *Chemical Abstracts*. Verkade, *A History*, 160.

[37] Pictet, Agenda entries of 4-5 Oct 1925.

[38] "Procès verbal des séances tenues à Paris du 3 au 6 Octobre 1925."

[39] Holleman to members of the Comité de Travail, Jan 1926, BGE, Ms. Fr. 3423, 227–234.



International Committees on Organic and Inorganic Chemical Nomenclature at Paris in October, 1925

Left to right, standing:
A. J. Greenaway, Great Britain
F. Fichter, Switzerland
A. M. Patterson, United States
A. Pictet, Switzerland
M. Delépine, France
C. S. Gibson, Great Britain
R. Marquis, France

Sitting:
W. P. Jorissen, Netherlands (chairman inorganic)
A. F. Holleman, Netherlands (chairman organic)

Figure 1. The IUPAC working group on organic chemical nomenclature, along with several members of another Union working group charged with the reform of inorganic chemical nomenclature, in October 1925.[40]

By then, Holleman was eager to complete his work and pass on responsibility for IUPAC's work on organic nomenclature to someone else. Holleman had resisted previous requests by the Union general secretary Gérard to publish the findings of the working group, arguing that it was counterproductive to publish resolutions that the committee itself had not yet firmly agreed upon.[41] By early 1926, however, Holleman felt that the end was in sight. He sent the rules to Stelzner and Prager in Berlin for unofficial comment,[42] and scheduled a working group meeting for April. When Marquis became ill, Holleman postponed the meeting until July; Holleman hoped that the working group could bring its work to a conclusion at that meeting. He would then meet with Patterson and the American nomenclature commission before the IUPAC Conference in Washington in September, and having secured their assent,

[40] Reprinted with permission from *Industrial & Engineering Chemistry* 17, no. 12 (1925): 1245. Copyright 1925 American Chemical Society.

[41] Holleman to Gérard, 12 Jul 1924 and 14 Feb 1925, IUPAC papers, Box 51.

[42] Verkade, *A History*, 127–28.

publish the resolutions.[43] The July meeting was also scuttled, this time due to the British General Strike of May 1926.[44] However, Holleman met with Patterson and the American commission as planned; based on these discussions and Patterson's comments on the January 1926 draft, Holleman made numerous adjustments to the rules.[45] In January 1927, he chaired the final meeting of the working group, at which Holleman, Marquis, Pictet, and George Barger (the new, polyglot British representative) "dispatched much work and were all in agreement."[46] This agreement was timely; the Washington Conference had been seen as a great disappointment, and the IUPAC Bureau began to put pressure on its commission presidents to bring concrete questions to the agenda of the meeting to be held in September 1927 in Warsaw.[47] Three days after the conclusion of the working group meeting, Holleman wrote to a Union Vice-President to announce that the organic nomenclature commission could discuss the working group's nomenclature recommendations.[48]

A foundation for "as little change as possible"

Holleman had used the text of Pictet's proceedings of the Geneva Congress to organize the process by which the working group developed its proposed system of nomenclature. The

[43] Holleman acknowledged in his letter to Pictet that the working group had spoken of the necessity of securing the agreement of the Germans before publishing its rules. However, given that the editors of *Beilstein* and the *Literatur-Register* could not agree on a common system of nomenclature, even though both publications were compiled under the same roof in Berlin, Holleman hardly thought it likely that the Germans would agree to mold their systems to fit the IUPAC rules. Furthermore, since Stelzner had not submitted any serious criticism in response to the January list of rules, Holleman suspected that there was no active German commission on nomenclature. Holleman to Pictet, 6 May 1926, Ms. Fr. 3441, 75–76.

[44] Student participation in the strike led to the postponement of British university examinations to July and August, and Holleman had not wished to hold a meeting that neither the British nor American delegates could attend. The biochemical nomenclature committee had already run into problems when its British and American delegates felt that their opinions had not been adequately sought or heeded. See, e.g., correspondence between Levene and Gérard, Sept 1923 to Feb 1924, IUPAC papers, Box 19.

[45] Minutes of the Fourth Meeting of the American Committee on Organic Nomenclature, 9 Sept 1926, W. A. Noyes papers, Box 6, Folder 1926 [II]; "Communication from Dr. Holleman [translation by Patterson]," 8 Nov 1926, Patterson to Members of the American Comité de Travail on Organic Nomenclature, 27 Nov 1926, Patterson, "Comments on the Holleman Draft of Nov. 8, 1926," n.d. [Dec 1926], W. A. Noyes papers, Box 6, Folder 1926 [I].

[46] Pictet, Agenda entries of 3–4 Jan 1927.

[47] Nasini to Holleman, 10 Nov 1926, IUPAC papers, Box 52.

[48] Holleman to Nasini, 7 Jan 1927, IUPAC papers, Box 52.

content of these rules, and the historical framing that Holleman provided in his introduction, express two further senses in which the working group "based its discussions on the Geneva nomenclature." To develop a system "based on" the 1892 rules meant both retaining a connection to the Geneva nomenclature and departing from it. In the report that Holleman submitted to the larger commission in 1927, the working group hewed closely to the form and content of the Geneva rules, but departed from Geneva in the manner in which the rules were to be applied. The report also recast the history of the Geneva nomenclature in a manner that emphasized these connections and downplayed the departures.

The working group's proposals clearly bear the mark of the Geneva nomenclature.[49] The Geneva nomenclature was made up of sixty-two rules; the 1927 report contained sixty-eight. Not only did the bulk of the 1927 report cover the same ground as the Geneva rules – hydrocarbons, compounds containing a single variety of functional group, and radicals – but it also grouped these categories of rules into three separate numbered sections, as Pictet had done in his 1892 report. [50] Apart from regrouping rules addressing the numbering of atoms into a separate section, the rules in each of these sections were listed in substantially the same order as Pictet's Geneva rules. While some of these rules were replaced or modified, in other cases, the report reproduced the text of Geneva rules verbatim.[51]

Even where the working group built upon acknowledged gaps in the Geneva nomenclature, it did not venture far beyond the decisions of 1892. Two of the Geneva rules had called for further study of the nomenclature of cyclic compounds and compounds of complex function. The 1927 report answered with two additional sections of three rules each. However,

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[49] *1927 Report*. Compare to Amé Pictet, "Le congrès international de Genève pour la réforme de la nomenclature chimique," *Les archives des sciences physiques et naturelles* 27 (May 1892): 485–520.

[50] The committee structured its report in this way on Pictet's recommendation; "Procès verbal des séances tenues à Paris du 3 au 6 Octobre 1925", 220.

[51] On the 1927 report, see Verkade, *A History*, 123–127. Verkade presents a rule-by-rule comparison of the Liège nomenclature and the Geneva nomenclature on 141–175.

the rules for naming complex functions were substantially equivalent to the procedure that Béhal had proposed in the Subcommission's report to the Geneva Congress, and the rules for naming cyclics barely scratched the surface of the problem of assigning systematic names and canonical numberings to ring systems.[52]

The working group had produced this homology between the Geneva rules and its own decisions by the manner in which it conducted its deliberations: working its way through Pictet's report and putting off discussion of Patterson's work on the numbering and nomenclature of cyclic systems. The working group solidified this connection in the introduction to its report. Probably penned by Holleman in the spring of 1927, the introduction opens with a paragraph describing the current "state of anarchy" in the nomenclature of organic chemistry, the second half of which was copied verbatim from the opening of Pictet's proceedings of the Geneva Congress.[53] The problems of 1892, then, were the problems of the present.

Holleman painted the Geneva nomenclature as a response to this challenge, built around the general principles of forming names based on roots indicating the number of carbon atoms in a chain supplemented by prefixes and suffixes indicating chemical function. Although the Geneva rules were never generally adopted, Holleman wrote, they had been increasingly influential, especially for the naming of new compounds. Furthermore, the

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[52] More specifically, the 1927 report contained a section of three rules addressing "Fundamental Heterocyclic Compounds" and two rules addressing aromatic and unsaturated cyclic hydrocarbons. Notwithstanding these additions, Verkade judged that the gap in the Geneva nomenclature regarding cyclic compounds and their derivatives was "in no way filled in the Report." Ibid., 124. Patterson's numbering rules were mentioned only in a concluding note, stating that the period for comment on the rules had expired without anyone contacting Patterson about them, but that some committee members nonetheless felt that the rules were impractical; *1927 Report*, 11. Patterson had circulated a scheme for naming such compounds to his fellow committee members in 1926. However, these compounds were not mentioned in the report and according to Verkade were never officially discussed by the working group; Verkade, *A History,* 125. Patterson published these rules in 1928 as Austin M. Patterson, "The Nomenclature of Parent Ring Systems," *Journal of the American Chemical Society* 50, no. 11 (1928): 3074–3087.

[53] Compare *1927 Report*, 3 and Pictet, "Congrès international de Genève," 485.

application of names consistent with the Geneva rules in the fourth edition of *Beilstein* demonstrated the general applicability of the system's core principles, even in complicated cases. Concluding his historical framing, Holleman wrote that at present, "a revision of these rules becomes more and more urgent, because many are now obsolete and others were never applied; moreover, the system was incomplete from the beginning and *a fortiori* is so now."[54] From this urgent need was sprung the working group.

This introduction, then, cast the Geneva nomenclature in a favorable light, effaced thirty subsequent years of work on the nomenclature of organic chemistry, and suggested that the working group had picked up just where the Geneva Congress had left off. All three points served important rhetorical purposes. If the Geneva nomenclature was presented as ill-founded or little-adopted, the IUPAC commission on organic nomenclature might well question whether a system of nomenclature based on the Geneva rules was worthwhile after all. References to other work on nomenclature – which the committee had by and large ignored in its own labors – might similarly call the foundations of the decisions in the report into question. Presenting the Geneva nomenclature as influential and the report as its much-needed successor, on the other hand, was a means of appealing to chemists with both conservative and progressive attitudes towards matters of naming.

Most remarkably, the introduction was entirely silent on what was elsewhere taken to be the most significant feature of the Geneva rules: the decision of the Geneva Congress to establish rules for generating unique, official names. Nor did the 1927 report distinguish between index names and nomenclature used in other settings, as the Geneva Congress had. Indeed, the report made no mention of any particular settings in which its rules might or might not be applied. The decision to focus exclusively on creating an official nomenclature for

[54] *1927 Report*, 3.

indexes was taken by many as the signal error of the Geneva Congress from which all of the other shortcomings of the Geneva nomenclature followed.[55] By ignoring the matter entirely in its report, the working group avoided either criticizing this central feature of the Geneva rules or appearing to endorse it.

The first rule listed in the 1927 report stated that "as little change as possible will be made to universally adopted terminology."[56] To those familiar with the Geneva rules, this was a clear statement that the working group was operating in a different spirit than that of the Geneva Congress. Whether the working group knew it or not, this was a shift back toward the conservative approach to nomenclature reform that Friedel's Subcommission had advocated.[57] This was also true of the working group's most significant change to the process of forming a name. As discussed in Chapter 2, the Geneva rules codified an algorithmic process for naming compounds based on *connectivity*: stripping a structural formula of all substituents, finding the longest chain of carbon atoms in the resulting formula and taking the name of this principal chain as a root, naming and numbering the remainder of the carbon skeleton, and, as a final step, adding in the prefixes and suffixes corresponding to the functional substituents. The working group, in contrast, decided that the principal chain should always be determined such that it contained the maximum number of functional groups. This was the approach that the Béhal had advocated at Geneva as a means of forming names that better expressed the *functional behavior* of the substance. The working group justified its decision on much the same grounds, noting that selecting a principal group in this manner rather than that of the Geneva

[55] See Chapters 3 and 5.

[56] "Terminology" changed over several drafts, from "dénomination," (Oct 1925, "Procès verbal des séances tenues à Paris du 3 au 6 Octobre 1925," 219) to "terminaison" (1926-27; Holleman to working group, Jan 1926, BGE, Ms. Fr. 3423, 227; *1927 Report*, 4) to "terminologie" (1928; IUPAC Comptes Rendus 9 (1928): 64). I take "terminaison" ("ending") as a mistake, though a telling one, given other evidence of Holleman's Esperantist preoccupation with word-endings. (See note 16 above.)

[57] See Chapter 2.

rules gave precedence within the name to the structural elements that "have a far greater influence upon the properties of the compound."[58]

The working group's other significant modification of the Geneva approach followed the same reasoning. After a long debate over the naming of carboxylic acids, the majority of the Geneva delegates had voted to consider the carboxylic carbon atom part of the carbon skeleton rather than the substituent group. This conserved the consistency of the algorithmic approach to naming compounds based on their carbon skeleton at the cost of approving a rule that generated awkward names and could not be extended to cyclic substances. The working group reversed this decision, on the grounds that the Geneva rule had never been widely applied, and that the opposing approach led to less ambiguous names for branched chain acids and could be applied without modification to cyclic acids. The latter two arguments were the same ones advanced by Louis Bouveault at Geneva and reported in Pictet's extended discussion of this debate in his account.[59]

The 1927 report departed from the rigor of the Geneva rules in one more way. In the working group's meeting in October 1925, nine rules into their discussion, the committee members reached an impasse on the question of how to order the prefixes corresponding to hydrocarbon side chains. Pictet and Marquis favored ordering them according to some chemical criterion, such as molecular weight; Holleman, Gibson, and Patterson preferred alphabetical order. (Patterson had already adopted this approach in the Collective Index to *Chemical Abstracts*.) Yet the terms under which the working group had been formed required its members had to reach their decisions unanimously.

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[58] Austin M. Patterson, "Definitive Report of the Commission on the Reform of the Nomenclature of Organic Chemistry Translation with Comments," *Journal of the American Chemical Society* 55, no. 10 (1933): 3913.

[59] *1927 Report*, 6–7; Pictet, "Congrès international de Genève," 502–504. Pictet had been among the minority of Geneva delegates who sided with Bouveault.

Under pressure both to achieve unanimity and to move on with their work, the delegates elected not to make a choice; the 1927 report sanctioned the arrangement of prefixes in either a "conventional" or alphabetical order.[60] This was a step beyond the exceptions that Patterson introduced in the process of assembling the Collective Index, or the flexibility that Friedel and his Subcommission had built into their own report. In the latter cases, the chemist or indexer had license to choose which of several rules to apply, according to the particular features of a compound and circumstances in which it was being named. Here, chemists were given license to assemble the name for the same compound in the same situation in different ways, according to their personal preferences.

The Union printed the report in 1927; Gérard distributed copies to the nominal representative of each member nation on the organic nomenclature commission as well as to other interested parties.[61] Holleman placed the report on the agenda for the meeting of the full organic nomenclature commission (its first since he had been elected commission president three and a half years before) at the Union conference in Warsaw in September. In the final prefatory paragraph of the report, just before beginning the list of rules, Holleman added a note of humility: "The working group is not at all under the illusion that its propositions will be accepted in total and in a short period of time."[62] Whether offered as false modesty or in earnest, the judgment was correct. The working group's departure from the precedent of Geneva in the scope and rigor of its rules set the stage for a showdown with a chemist who had a different idea of what it meant to remain loyal to the spirit of the Geneva Congress.

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[60] "Procès verbal des séances tenues à Paris du 3 au 6 Octobre 1925," 220, 224–225 (rules numbered 9 and 45A); *1927 Report*, 5, 10 (rules numbered 7 and 64). In the first of these rules, a particular "conventional" order based on the number and configuration of carbon atoms in the side chains was specified; in the latter, the meaning of "conventional" was itself left open.

[61] Verkade, *A History*, 127.

[62] *1927 Report*, 3.

The last charismatic nomenclature reformer

Victor Grignard, the French member of the IUPAC Commission on the Reform of Organic Nomenclature, was a world-renowned synthetic organic chemist and one of his nation's leading public scientific figures.[63] Grignard had a longstanding interest in matters of organic nomenclature, and he held strong opinions about the nomenclature reform project about to be presented to his commission. As he arrived at the IUPAC conference in Warsaw in 1927, the fifty-six-year-old Grignard was thus in a strikingly similar position to the fifty-six-year-old Adolf Baeyer upon his arrival at the Geneva Congress in 1892.

Like Baeyer, Grignard sought to single-handedly shift the course of nomenclature reform. By dint of eloquent, well-reasoned argument, Baeyer had persuaded the assembly to turn away from a nomenclature that ratified existing usage and flexibility to the cause of logic and rigor. Though the patriotic Frenchman would have cringed at the parallel with a signatory of the hated Manifesto of the Ninety-Three, Grignard aimed to do much the same.[64] For two consecutive meetings of the Union, Grignard rallied the organic nomenclature commission against portions of the working group's report that he felt needlessly sacrificed the logic of the Geneva rules.

However, Grignard's intervention was ultimately unsuccessful – doubly so. First, he was unable to bring the method of nomenclature embodied in the working group's rules more in line with that of the Geneva nomenclature. Second, in finding a way to get the nomenclature approved despite Grignard's objections, Holleman foreclosed Grignard's means of asserting

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[63] For example, Noyes chose Grignard as his primary French correspondent in his edited collection of letters to and from European chemists regarding European international relations; William A. Noyes, *Building for Peace II: International Letters,* (New York: Chemical Catalog Co., 1924).

[64] Ironically, Grignard owed his Nobel to the support of Baeyer and other German nominators. Oscar Widman, the Swedish bellwether of organic nomenclature reform discussed in Chapter 1, came up with the pairing of Grignard with his countryman Sabatier that secured the majority vote. Elisabeth T. Crawford, *Nationalism and Internationalism in Science, 1880-1939: Four Studies of the Nobel Population* (New York: Cambridge University Press, 1992), 63; Mary Jo Nye, *Science in the Provinces: Scientific Communities and Provincial Leadership in France, 1860-1930* (Berkeley: University of California Press, 1986), 185.

authority over matters of nomenclature. What had been the business of "the most authoritative scholars" at Geneva became, at Liège, the business of negotiations among experts in the obscure new field of chemical reference literature.[65]

Grignard took an interest in matters of nomenclature as his career was reaching its zenith.[66] In 1910, he was promoted to a full professorship at Nancy; in the autumn of 1912, he was awarded a Nobel Prize for his eponymous reaction, an invaluable tool for synthetic chemists. In between, he published an article on the nomenclature of polycyclic hydrocarbons and was named to the French commission on organic nomenclature, organized as part of the establishment of the IACS committee on organic nomenclature. [67] After the war, Grignard was named French representative to the IUPAC nomenclature commission.[68] He left Nancy but remained in the provinces, taking up a professorship in Lyon.[69]

The organic nomenclature commission met during the afternoon of the first day of the 1927 Congress in Warsaw. For Pictet, a Vice-President of the Union, the session conflicted with

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[65] "Savants les plus autorisées": Pictet, " Congrès international de Genève," 486.

[66] Grignard's initial interest in nomenclature likely traced to Bouveault, one of the most active members of Friedel's subcommission and Grignard's teacher and colleague at Lyon. This interest was encouraged by Beilstein (the person, not the handbook), whom Grignard met at a 1905 AFAS meeting, shortly before the great chemical compilator's death; Nye, *Science in the Provinces*, 181. Ironically, it was Bouveault's proposal for the nomenclature of carboxylic acids, revived by Pictet in 1925, that would be the foremost target of Grignard's criticism.

[67] Grignard modified Baeyer's nomenclature for these compounds; Victor Grignard, "Sur la nomenclature des systèmes hydrocarbonés polycycliques," *Bulletin de la Société Chimique de France* 11 (1912): 124–129.

[68] IUPAC Comptes Rendus 2 (1921): 66. During the war, Grignard worked in Charles Moureu's poison gas research group in Paris. He also traveled to the US for seven months, during which he delivered a lecture promoting the formation of an allied scientific-industrial-economic bloc to compete with German science and industry; Marika Blondel-Mégrelis, "Victor Grignard," in Laurence Lestel, ed., *Itinéraires de chimistes 1857-2007, 150 ans de chimie en France avec les présidents de la SFC* (Paris: SFC, 2007), 226–227.

[69] There's something to be made of Grignard's position in the provinces and the importance that he placed on the social and intellectual category of "French chemistry." Chemists in the American Midwest – Noyes, for example – sought to build up the American Chemical Society and use it to constitute an American chemical community, that is, a community in which chemists at Harvard, MIT, and Johns Hopkins would spent more time looking across the Appalachians than across the Atlantic. (See, e.g., the East/Midwest division over the question of whether to shutter or maintain Ira Remsen's *American Chemical Journal* in 1913. "There is a growing feeling hereabout and also in other localities west of the Alleghenies that Central West Chemists do not have to submit to any humilities from the Eastern crown," wrote an Ohio State chemist to Noyes; n.n. to Noyes, 1 Dec 1913, Noyes papers, Box 4.) As a joint connecting language, chemical reasoning, and access to chemical research, nomenclature was a central site for the construction of imagined and real national disciplinary communities. These communities were especially valuable to those who did not have kind of the local and international interchange available to a metropolitan chemist.

a meeting of the Council, the Union's governing body.[70] Pictet elected to attend the nomenclature meeting; he had met Grignard for breakfast the preceding day, so he may have been warned of what was to come.[71] The twenty chemists in attendance made the session the largest of the conference's six commission meetings; however, Pictet and Holleman were the only two working group members present, and only four other titular members of the international committee were there. Once again, the majority of the meeting's attendees were ad hoc replacements.[72]

As Holleman led this group through a discussion of the 1927 report, Grignard advanced numerous objections and alternatives. To Pictet, it seemed as if Grignard was objecting to every article in the report; Dutch chemist P.E. Verkade, one of the replacements, recalled that Grignard dominated the meeting, presenting convincingly-argued criticism and proposals.[73] The committee meeting morphed into a long discussion between Grignard, Holleman, and Pictet, during which Grignard demanded that his objections and alternatives be printed at the conclusion of the working group's report. Pictet considered this "intolerable."[74] In the end, the commission decided to print Grignard's remarks as a separate pamphlet, and to circulate it along with the additional copies of the 1927 report and reprints of Patterson's article on the numbering of rings to all of the national organizations that belonged to IUPAC.[75]

The text of Grignard's remarks ran to eleven and a half pages – the same length as the working group's report itself – and specifically addressed about a third of the working group's rules. However, this was a deceptive measure of the extent of Grignard's opposition. In fact,

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[70] Pictet, Agenda entry of 5 Sept 1927; IUPAC Comptes Rendus 6 (1925): 4.
[71] Pictet, Agenda entry of 4 Sept 1927.
[72] IUPAC Comptes Rendus 8 (1927): 13–14.
[73] Verkade, *A History*, 127.
[74] Pictet, Agenda entry of 5 Sept 1927.
[75] IUPAC Comptes Rendus 8 (1927): 34.

aside from isolated details that could be settled with relative ease (according to Holleman, at least), Grignard's critiques and proposals could be subsumed under two general points.[76]

First, Grignard proposed an alternative method for numbering the carbon chain. He agreed with the working group's conclusion that the longest-chain approach of the Geneva Congress would not suffice. However, Grignard advocated considering not only functional groups but "anomalies" – a new category that Grignard coined to refer to branchings, functional substituents, and double and triple bonds – in selecting the principal chain. As he put it a few years later, "from a chemical point of view, in a very large measure, what governs all the properties of an organic compound is not the dimension of the fundamental skeleton but the anomalies that it presents."[77] In order to most clearly express the properties of the substance in its name, Grignard advocated selecting the carbon chain with as many such anomalies as possible as the principal chain. The working group had based its version of the rule on the same line of reasoning, but Grignard believed that his approach applied it with greater rigor and generality.

Second, and more importantly, Grignard considered the working group's approach to carboxylic acids "absolutely impossible to accept." Grignard cited the example of various related compounds containing a five-carbon chain (*Fig. 2*). According to the Geneva nomenclature, these compounds all contained the same root and varying suffixes: pentane, pentanol, pentanal, pentanoic acid and pentane-nitrile. The rules of the 1927 report, in contrast, would change the latter two names to butane-carboxylic acid and butane-carbonitrile. Thus, the simple process of oxidation or reduction relating aldehydes and acids would be represented by a shift in names from a five-carbon "pentane" derivative to a four-carbon

[76] Grignard to Gérard, 22 Sept 1927, IUPAC papers, Box 52; Holleman to Pictet, 26 Jan 1928, BGE, Ms. Fr. 3441. Holleman mentions only the acids, and indeed, Grignard did connect his comments on the determination of the principal chain to the problems presented by the naming of carboxylic acids.

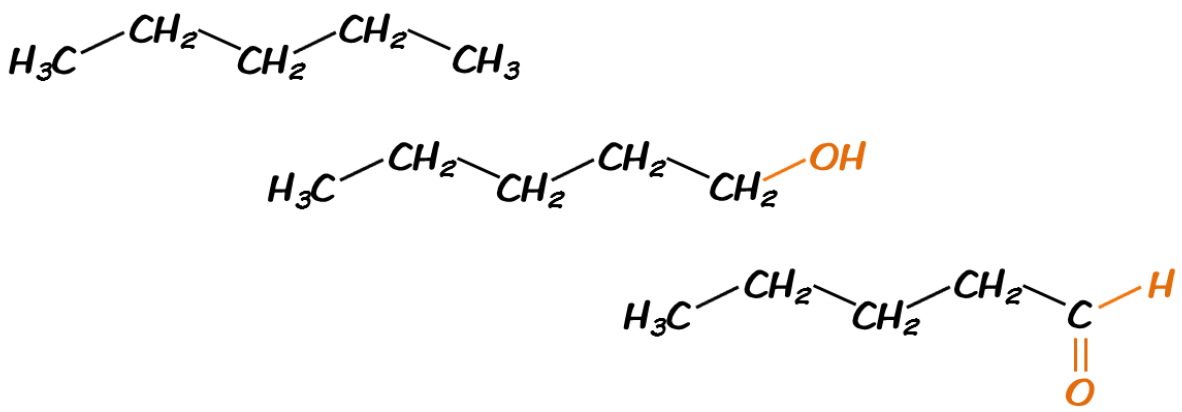
[77] Victor Grignard, *Traité de chimie organique,* vol. 1 (Paris: Masson, 1935), 1074.

"butane" derivative and back – this despite the fact that the oxidation and reduction in question were simple, ubiquitous, and easily reversible reactions, whereas extending and breaking carbon chains was the signal challenge of synthetic organic chemistry.[78] By introducing a distinction between names that obscured both a close chemical relationship and a basic feature of chemical structure, Grignard argued, "this rule destroyed the beautiful order established by the Geneva Congress."[79] He discussed at length the pernicious influence of this rule, providing example after example of the rule "collapsing into illogic" and refuting each of the working group's justifications for making the change.[80]

[78] Grignard had particular reason to be sensitive to the difficulty and significance of forming carbon-carbon bonds. The reaction for which he had won his 1912 Nobel Prize was one of the few methods then available to synthetic chemists for doing so.

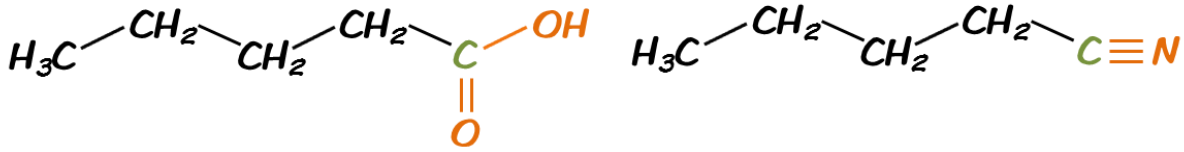
[79] Victor Grignard, *Remarques au rapport du comité de travail de la réforme de la nomenclature de chimie organique* (Paris: Union Internationale de la Chimie Pure et Appliquée, 1927), 5.

[80] Ibid., 5–7.



Geneva nomenclature & working group's report

pentane *pentanol* *pentanal*



Geneva ***pentan****oic acid* ***pentane****-nitrile*

WG ***butane****-carboxylic acid* ***butane****-carbonitrile*

Figure 2. Grignard cited these five-carbon compounds as evidence that the working group's proposed rules introduced inconsistencies not present in the Geneva nomenclature. The working group proposed treating the carbon atoms colored green above as part of the functional group (orange), while the Geneva rules treated them as part of the principal carbon chain (black).

Grignard submitted his edited version of these critiques to Gérard (his former student from Nancy and subordinate in wartime chemical warfare research) at the end of September, a few weeks after the Warsaw conference.[81] Gérard confirmed that he would have Grignard's remarks printed and distributed right away. When Grignard and Holleman met in Paris at the end of October, however, they had not yet been circulated. Holleman and Grignard agreed that

[81] Danielle Fauque, "French Chemists and the International Reorganisation of Chemistry after World War I," *Ambix* 58, no. 2 (2011): 131n85. On wartime chemical weapons research, see L. F. Haber, *The Poisonous Cloud: Chemical Warfare in the First World War* (New York: Oxford University Press, 1986).

the remarks would be distributed along with the revised version of the working group's report that Holleman was in the process of completing.[82]

A few weeks later, having heard nothing more from Holleman, Grignard wrote to Gérard once again to impress upon him (and through him, upon Holleman) the urgency of circulating the nomenclature materials as quickly as possible:

> It appears to me highly desirable that M. Holleman complete his report as soon as possible so that we can have all the necessary time to distribute it, along with my remarks, in the countries of the Union. It is indeed important that all competent chemists be able to read these documents, discuss them and give their opinion to their delegates at the Conference of The Hague. Several months are certainly necessary to attain this goal. And it's only on this condition that the discussion in 1928 can have a chance to attain a result. Otherwise we will find ourselves at exactly the same point as in departing Warsaw. It would not be worth it to attend under such conditions.[83]

Gérard passed Grignard's message along to Holleman, who contacted Grignard to set up a meeting in Lyon in January to discuss these matters. This was not good enough for Grignard; he wrote again to Gérard a week later complaining that Holleman had not taken his admonition about the importance of distributing the reports promptly to heart. Two weeks after that, in mid-December, Holleman notified Gérard that he had completed his revision of the rules and had distributed it to the working group members, soliciting comments by the first of January in advance of his meeting with Grignard. "You see, therefore, that I am active," Holleman concluded rather huffily.[84]

In May, however, with the IUPAC conference only two months away, the reports had still not been printed. Holleman and Gérard had been exchanging corrected proofs for a month, and Grignard was beginning to smell a conspiracy: "If the organic chemists are not put in a

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[82] Grignard to Gérard, 22 Sept 1927, Grignard to Gérard, 1 Oct 1927, Holleman to Gérard, 29 Oct 1927, IUPAC papers, Box 52.

[83] Grignard to Gérard, 21 Nov 1927, IUPAC papers, Box 52.

[84] Gérard to Holleman, 23 Nov 1927, Grignard to Gérard, 2 Dec 1927, Holleman to Gérard, 17 Dec 1927, IUPAC papers, Box 52.

position to give their advice before the meeting of The Hague, all serious work will be impossible and we will play into the hands of the Anglo-Germans."[85] In the wake of the revision of International Research Council rules in 1926 permitting the former Central Powers to join IUPAC and its sister unions, the Union council had begun to make overtures toward German chemists. In a letter written during the Ruhr crisis, Grignard had expressed suspicion that France's former allies Britain and America had turned on his nation in favor of rapprochement with Germany over reparations.[86] Now, Grignard suspected that Holleman's interminable delays were part a similar strategy with regard to nomenclature aimed at delaying any definitive decisions until after the Germans had entered the Union.

In opposition to this supposed scheme to introduce disorder into the nomenclature of organic chemistry, Grignard called his countrymen to arms. In a remarkable conflation of language, nationality, and attitude toward chemical nomenclature, he sketched out the coming struggle in his letters to Gérard. "It is indispensable that all chemists of the French language and with them, I hope, all of the Latin spirit, at least, prepare themselves for battle against the Anglo-Saxon illogic."[87] "I still have no news of the distribution of the Report on Nomenclature. It's an intentional effort to stifle discussion; but I hope that the Latins will not let it happen."[88] In late June, after the reports were finally printed and distributed, Grignard asked Gérard to disseminate additional copies to French organic chemists, along with a note enjoining the recipients to send their opinions to Gérard as quickly as possible, so that he would "be able at the moment of the discussion to rely on the opinion of the majority of French organic

[85] Grignard to Gérard, 15 May 1928, IUPAC papers, Box 52.

[86] This was one of the letters that Noyes published as part of his foray into international diplomacy; Grignard to Noyes, 7 Sept 1923, quoted in Noyes, *Building for Peace II*, 13–15.

[87] Grignard to Gérard, 20 May 1928, IUPAC papers, Box 52.

[88] Grignard to Gérard, 17 Jun 1928, IUPAC papers, Box 52.

chemists."[89] If Holleman would not allow Grignard to be the Condorcet of organic chemical nomenclature, then Grignard would be its Robespierre.

Holleman, for his part, was under pressure from Union President Ernst Cohen to document his commission's tangible achievements and quickly attain new ones. Many of the Union's commissions had perpetually suffered from uncertain attendance, interminable report-writing, uneven buy-in, and a dearth of results. Aiming to eliminate some commissions and reform the rest, Cohen wrote to all of the commission presidents demanding a more precise accounting of their achievements and goals.[90] Cohen may have applied this pressure particularly heavily to his fellow Dutchman Holleman. Despite Grignard's opposition, Holleman was optimistic that he could manage to have the working group's rules approved at The Hague. In January, Holleman wrote to Pictet to describe his parliamentary strategy for the upcoming conference. Since Grignard's ire was focused on Rule 29 (the rule for naming carboxylic acids), Holleman expected that the other rules would pass without difficulty. As for Rule 29, he wrote, "I know that the English and the Americans support it, and the Germans as well, if they are present, whereas the Italians hesitate and a party of French oppose it." Grignard was not wrong to detect the formation of an Anglo-German bloc; with one Commission member per country, Holleman thought there was a good chance that he could win a majority of the vote, and thereby bring the working group's labors to a close. Holleman wrote to Pictet:

[89] Grignard to Gérard, 20 Jun 1928, IUPAC papers, Box 52.

[90] Cohen to Bertrand, 25 Jan 1928, IUPAC papers, Box 19; Roger Fennell, *History of IUPAC, 1919-1987* (Boston: Blackwell Science, 1994), 42–45.

> Even though our committee was declared permanent, I am of your opinion that we will have fulfilled our mandate after the meeting at The Hague. Later on, those younger than us could modify and extend the rules after a certain number of years, if they appear to be useful. I hope, dear colleague, that you will be able to be present at The Hague. You told me, at Warsaw, that you might want to let yourself be replaced on this occasion. Being almost done with our work I ask you please to persevere now up to the end of our efforts.[91]

Grignard was wrong in one respect: Holleman was not seeking to prolong the debate over nomenclature until the Germans entered the Union. Holleman did not want to delay a definitive vote on the working group's rules – he wanted out.

Holleman made a crucial error in his vote-counting, however. While each country in the Union had in principle one representative and thus one vote on the commission, no more than a handful of the titular representatives had ever shown up at a Union conference. The quorum had always been filled out by replacement commission members, who could be from any country. Further, since this practice made the formal composition of each commission highly uncertain, it also enabled curious delegates who were not affiliated with a commission in any way to sit in on its meetings at will.[92]

Numerous such visitors were present as the organic nomenclature commission began its meeting at The Hague. All twenty-nine of the Union's member nations were represented, though only seven by their duly appointed commission members. Unofficial participants – their curiosity or passion piqued by Grignard's salvos against the working group's rules – swelled the group to about fifty.[93] In addition to Grignard and Marquis (as working group secretary, he seems to have been a commission member *ex officio*), at least two other French delegates sat on the commission as replacements. All of these factors – the strong showing by Grignard's fellow

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[91] Holleman to Pictet, 26 Jan 1928, IUPAC papers, Box 52.

[92] Verkade, *A History*, 131.

[93] Pictet described the assembly as "very numerous;" Pictet, Agenda entry of 19 Jul 1928. Holleman retrospectively estimated that about fifty chemists were present; Holleman, "Letter of Greeting," 3.

French chemists, the number of commission members represented by replacements, and the number of unofficial participants – likely contributed to the rapid descent into disorder described at the beginning of this chapter.

From Pictet's perspective, the entire week was one long discouraging take on the capacity of the Union to accomplish anything productive. On the motion of the Spanish delegate, one of Pictet's fellow Union vice-presidents, the IUPAC council approved the addition of Spanish to French, English, and Italian as an official language for Union business. At a meeting the next day, Pope and Armstrong sarcastically moved that Japanese and Flemish be added as official languages as well; an argument almost broke out among the council members.[94] If there was this much strife within the Council as it was, Pictet, wondered, what would happen once the Germans joined? Writing in his diary that evening, Pictet predicted that "the entrance of the Germans, if it takes place, will ruin the Union."[95]

In the wake of the conference at The Hague, Holleman and Marquis tried to pick up the pieces. Pictet's pessimistic judgment that nothing at all had been decided at the commission meeting was not entirely accurate. At some point before Holleman dismissed the meeting, the commission members took a formal vote on Rule 29; by a 19-14 margin, they voted down the working group's version of the rule.[96] On the meager basis of this lone decision, the minutes of the organic nomenclature commission meeting published in the proceedings of the 1928 Union conference put a positive spin on the discussions. The minutes reported that "a small majority of its members are in favor of maintaining the Geneva nomenclature for acids and have voted against rule 29," but that the commission had otherwise adopted the working group's rules.

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[94] Pictet, Agenda entries of 19-20 Jul 1928. On analogous linguistic debates within the League of Nations, see Carolyn N. Biltoft, "Speaking the Peace: Language, World Politics and the League of Nations, 1918-1935" (PhD dissertation, Princeton University, 2010).

[95] Pictet, Agenda entry of 20 Jul 1928.

[96] Victor Grignard, "Quelques remarques à propos du projet de réforme de la nomenclature de chimie organique," *Bulletin de la Société Chimique de France* 45 (1929): 986. (Verkade incorrectly quotes the vote as 19-11.)

According to these proceedings, the commission called upon chemical periodicals to publish these rules, and proposed that a final vote on the report be held at the next Union conference in 1930.[97]

Encouraged by his success at The Hague, Grignard returned to the barricades during the summer of 1929. "We do not forget," he wrote to Gérard in early May, "that the Anglo-Saxons, defeated at The Hague, have in no way lost the hope of taking their revenge and that they have Germany behind them." Ten days later: "If the Anglo-Saxons triumph, all of the logic established in 1892 by the Geneva Congress will be thrown into disarray." Three weeks after that: "Once more, Latin good sense finds itself struggling with the Anglo-Saxon spirit."

Once again, Grignard sought to circulate his critiques via Gérard – not just in pamphlets distributed by the Union, this time, but alongside the text of the working group's rules to be printed in the chemical journals. Grignard further requested that Gérard add a note urging each nation's chemists to undertake discussions of nomenclature in meetings of their respective chemical societies over the coming year. Grignard hoped that by means of such discussions, chemists of each nation would arrive at a general opinion on the rules and Grignard's critiques in advance of the definitive vote to be held at the Union conference in Liège in September of 1930.

Gérard demurred. Mentioning that he'd consulted with Marcel Delépine, President of the Société Chimique de France, Gérard suggested that if Grignard wished to publish his remarks, he should submit them to the journals himself (preferably in abbreviated form).[98] Not to be defeated, Grignard repeated his request once more, in a letter headed "Personelle,"

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[97] The Union decided in 1927 to shift to a biannual conference schedule beginning in 1928; IUPAC Comptes Rendus 8 (1927): 10. Verkade, who was present at The Hague, suggested that Holleman and/or Marquis fabricated the commission's supposed approval of the working group's report; Verkade, *A History*, 131–32. It may be that some collection of commission members met later on during the conference and adopted the decisions described in the minutes; at the very least, the description of the commission's discussions in the proceedings was misleading.

[98] Gérard to Grignard, 5 Jun 1929, IUPAC papers Box 52.

appealing to Gérard not only as IUPAC secretary general but also as a colleague and friend. If only Grignard could present his critiques to a broad international audience, he wrote, "I am persuaded that that in the Anglo-Saxon nations, themselves, which have made a bloc against us, voices would raise up to safeguard the work of the Geneva Congress." This would be his last request on the subject, he promised. "And now, my dear General Secretary, you are sufficiently enlightened. If you do not want to do anything about it, I wash my hands of it and then I will cordially shake yours."[99]

In response to this personal plea from his mentor, Gérard found a solution. He insisted that there was nothing that he could do for Grignard as IUPAC secretary-general. However, he arranged to take action "from a national perspective," securing funding from the Béhal-chaired National Federation of Associations of Chemistry to support the publication of an edited version of Grignard's remarks and pledging to submit them under the banner of the latter organization to all of the journals to which Gérard had sent Holleman's rules under the auspices of IUPAC.[100]

Grignard's remarks were published in several journals, including the *Bulletin de la Société Chimique*, immediately following the report of the working group.[101] At Gérard's urging, Grignard focused on his two major criticisms of the working group's rules – the determination of the principal chain and the nomenclature of acids. At the same time, he also advanced an implicit argument about how rules of nomenclature should be made. Grignard presented his critiques, he wrote, "in the hope that numerous chemists from all countries will discuss them and present their authoritative opinion at Liège, either directly or through their national delegates." Nomenclature rule-making, therefore, was to be democratic. Indeed, Grignard felt, it

[99] Grignard to Gérard, 10 Jun 1929, IUPAC papers, Box 52.

[100] Gérard to Grignard, 17 Jul 1929, IUPAC papers, Box 52.

[101] Grignard, "Quelques remarques." For citations of the reprints of Grignard's remarks in other Swiss, Russian, and French publications, see Verkade, *A History*, 139n35–36.

had to be, for only chemists who had themselves deliberated upon a system of nomenclature rules before choosing to support it would have made "the tacit commitment to teach it and to enforce it locally." Without such a commitment, a system of nomenclature would "remain a dead letter."[102]

In Grignard's view, this did not mean that nomenclature had to be based first and foremost on existing practice, as the working group asserted in the first of its rules. Indeed, Grignard wrote, "It will not do for us to follow the masses and to build upon the errors of our predecessors." Old habits of naming might persist, and teachers might pass them down to their students, Grignard acknowledged, but "this does not prove that their nomenclature is superior to another, even if they appear to be in the majority."[103] Above all, Grignard argued, it was important that the world's chemists not allow the hard-won logic of the Geneva nomenclature to be sacrificed for nothing. **"**The work of the Geneva Conference," he wrote, "incomplete as it was, was perfectly coherent and logical. Before destroying one part of it, we must be certain that we have something better to put in its place."[104]

When it came to nomenclature, Grignard favored the rigorous logic of the Geneva Congress over the working group's willingness to sacrifice internal consistency for accord with general practice. When it came to the making of nomenclature rules, Grignard advocated a process of democratic deliberation, guided by the logic and oratory of a savant like himself. Had the form of the Union's rules of organic nomenclature been settled in a single series of meetings at Warsaw or The Hague, as the Geneva nomenclature had been, Grignard's arguments might well have won the day. But while Grignard was appealing to the world's chemists, Holleman

[102] Grignard, "Quelques remarques," 983.
[103] Ibid., 987.
[104] Ibid.

was negotiating with German editors. In 1930, rules of nomenclature would not be determined either by the chemists of highest professional status or by plebiscite, but by experts.

Negotiating the Liège nomenclature

While Grignard took it for granted that the Germans would support the working group's rules, the members of the "Anglo-Saxon bloc" did not. After the British working group member Barger visited Columbus in May 1928, Patterson wrote to Noyes to confirm that Barger agreed with Noyes, Crane, and Patterson "on the importance of having the Germans in on everything."[105] Noyes wrote to Holleman in November suggesting that he solicit a German representative for the working group, if the new Union President Einar Biilmann would allow it; in fact, Holleman had written to Billman over the summer with the very same request.[106] Meanwhile, both the Chemical Society of London and the ACS decided not to publish the working group's nomenclature rules until they had been approved by a committee that included an official German representative.[107]

With Biilmann's help, Holleman arranged a meeting with the principal German authorities on organic nomenclature in Berlin in May 1930.[108] The foremost concern of the Germans was whether Holleman wanted to establish the nomenclature as official and obligatory. In its reports, the working group had remained studiously silent as to where and how its nomenclature was to be applied, not even mentioning any distinction between index-nomenclature and general nomenclature. However, both Grignard and Holleman had suggested elsewhere that once the rules were approved by the Union, they might be enforced by requiring

[105] Patterson to Noyes, 17 May 1928, W. A. Noyes papers, Box 9, Folder General Correspondence 1928.

[106] Noyes to Holleman, 20 Nov 1928, Holleman to Biilmann, 28 Jul 1928, IUPAC papers, Box 52.

[107] Draft of Gibson to Gérard, 6 Nov 1929, Royal Society of Chemistry Library, Burlington House, London, AR0319; Noyes to Patterson, 16 Mar 1927, W. A. Noyes papers, Box 8, Folder "Patterson, Austin M. 1927."

[108] Verkade lists the German attendees as Prager and Richter of *Beilstein*, Stelzner and Kuh of the *Literatur-Register*, Pflücke of the *Zentralblatt*, Radt, and Stern. The presence of Kuh and Stern illustrates the high-level positions accessible to women in interwar chemical reference enterprises, particularly in Weimar Germany.

authors to provide a name consistent with the rules for each new compound as a condition of having an article accepted for publication. To the Germans, this sounded like an attempt to create an official index-nomenclature, the principle against which Paul Jacobson had built up the practices of nomenclature and indexing that governed the prodigious chemical reference infrastructure presided over by the Deutsche Chemische Gesellschaft. Holleman gave in: the working group would request that journal editors recommend the new rules, but not that they enforce them. The Germans, in turn, would support the text of the rules as it stood when it came time to vote at Liège. Bernhard Prager, editor-in-chief of *Beilstein*, would represent Germany on the organic nomenclature commission and in the working group.[109]

Holleman also took great care to forestall any further debate at Liège. Over the year and a half leading up to the conference, he corresponded with Commission members, soliciting and responding to their criticisms until they assured him that they (or their replacements) would vote in favor of the revised report.[110] At the eleventh hour, Patterson expressed grave concerns about one of the rules; Holleman agreed to suppress it.[111] In order to honor the decision made at The Hague, Holleman rewrote the rule for carboxylic acid nomenclature to permit either the Geneva approach or the working group's approach to be used. This was hardly a victory for the austere rigor of the Geneva rules, but it was apparently enough to pacify Grignard.[112]

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[109] Verkade, *A History*, 146.

[110] Other working group members may have assisted with this correspondence. Patterson alluded to the procedure during a March 1929 lecture on the Union: "It must be apparent that the real work of the commissions is done by the working committees; if these were to report to their commissions by mail, and secure, first, criticisms and then an approving vote, the procedure would be improved. This is now being done in at least one case." Austin M. Patterson, "International Chemistry," *Science* 69, no. 1795 (May 24, 1929): 534.

[111] The rule conflicted with the index nomenclature used in *Chemical Abstracts*; "Statement of Mr. Patterson with regard to rule 49," 5 Jun 1930, W. A. Noyes papers, Box 10, folder General Correspondence 1930.

[112] A description of the Liège nomenclature that Grignard published in 1935 suggests that he had come to believe, perhaps at Holleman's suggestion, that the existence of the entrenched nomenclature systems of *Chemical Abstracts* and *Beilstein* left the working group no choice but to sacrifice the logic of the Geneva nomenclature in order to try to "reconcile the different points of view" of these works; Grignard, *Traité de Chimie Organique*, 1075. Just before the Liège conference, Gérard wrote to Grignard, "I have the honor of sending back to you officially the response that M. Holleman addresses to you and that will be the object of a debate at the Conference at Liège." (Gérard to Grignard, 6 Sept 1930, IUPAC papers, Box 52.) There is no sign that any such debate took place.

Like the International Congress of Chemists in Paris forty-one years before, the IUPAC Conference at Liège took place during an international exposition. Though less grand than the event for which the Eiffel Tower had been built, this exposition, too, was held on a politically-significant centennial – the hundredth anniversary of the establishment of the Kingdom of Belgium. Not, one would think, the most diplomatic occasion for the reintroduction of Germany into international chemistry. Nonetheless, while Holleman was negotiating German assent to the working group's nomenclature rules, Biilmann had been working out the precise conditions under which the Germans would finally join the Union. In the opening session of the conference, he set these procedures in motion, swiftly gaining Council approval for various changes to the Union's statutes that the Germans had negotiated as conditions of their entry.[113] With that, the fourteen German delegates entered into the council chambers, to the applause of all present (this, too, at Biilmann's express request).[114]

The following day, Holleman held one more meeting of the working group to ensure that all of its members, now including Prager, were in complete agreement. The day after that, the Commission met, and the assembly (Grignard included) unanimously approved the proposed rules without discussion. Everything happened quickly, to Holleman's relief.[115] The Council affirmed the Commission's decision – a formality – and the working group's system of nomenclature was on the books.

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[113] Specifically, the council voted to change the name of the Union to the International Union of Chemistry and to establish that the site of its official headquarters, formerly provisionally located in perpetuity in Paris, would instead be determined by vote every four years; IUPAC Comptes Rendus 10 (1930): 17–18.

[114] Ibid.

[115] Holleman, "Letter of Greeting," 3. The hurried pace of the Liège meeting led to subsequent difficulties over the precise text of one of the rules. Patterson thought that a change that he suggested had been added before the final vote. Holleman accordingly sent the revised text of that rule to various publications. Prager subsequently protested that he had been certain that Patterson's revision had not been made, and Patterson diplomatically withdrew his claim. A typescript slip bearing Patterson's corrected (and later withdrawn) text of this rule pasted into the copy of the Liège proceedings in the library of the Paris Faculté de Pharmacie is material evidence of this snafu. See Verkade, *A History*, 135–38.

The sixty-eight rules approved at Liège were little different than the sixty-eight rules in the 1927 report. The principal changes – the suppression of one rule at Patterson's request and the introduction of the Geneva method of acid nomenclature favored by Grignard as an alternative to the working group's original approach – served mainly to introduce even more of the flexibility and room for discretion expressed in the commitment, in the first rule, to making as little change as possible in existing nomenclature practices.

If the rules did not change, however, their framing did. Likely as a condition of the German delegation's acceptance of the rules, Prager wrote an unsigned introduction for the Definitive Report of the Commission, the official text of the rules printed in the proceedings of the Liège meeting and subsequently in numerous chemical journals.[116] In this introduction, Prager took a different perspective on the legacy of the Geneva Congress. Unlike Holleman, Prager acknowledged that the Geneva rules had been expressly intended to establish official names for use in the preparation of indexes. The Geneva rules had not accomplished this aim, he wrote; they were never extended to cyclic compounds, and they generated names that were much too complicated. However, Prager continued, "the Geneva nomenclature has been of much value to organic chemistry, for its fundamental ideas are of great importance from the systematic [i.e., taxonomic] standpoint throughout the entire extent of the science."[117]

In the present day, Prager wrote, there was no longer any reason to try to develop an official index-nomenclature. Many publications had adopted empirical formula indexes in place of subject indexes based on chemical names, and the new edition of *Beilstein* "facilitates bibliographic research in an extraordinary manner" (recall that Prager was its editor). Yet there was a pressing need to provide guidelines that would stem the accumulation of

[116] Verkade, *A History*, 144; Paterson to Members of the American Committee on Organic Nomenclature, 7 May 1931, W. A. Noyes papers, Box 10, Folder Organic Nomenclature, 1931.

[117] Quotations taken from Patterson's translation of the Liège rules, Patterson, "Definitive Report," 3906.

"manifestly incorrect" names for newly discovered organic compounds. Liberated from the requirement of generating official names, the Geneva rules could be revised and extended as such a nomenclature for general use. But only for general use, as Prager's penultimate paragraph emphasized: "This report is not intended to interfere with the editing of *Beilstein* or of *Chemical Abstracts*, publications which have followed for many years their own systems of nomenclature."[118]

Experts in nomenclature reform

The Definitive Report was published widely in chemical journals over the years following its approval at Liège.[119] Holleman had turned seventy-one just before the Liège conference and was preparing to retire from his position at the University of Amsterdam. Having finally managed to shepherd the working group's rules to approval and publication, he was eager to pass on responsibility for nomenclature reform to somebody else. In response to an inquiry from Gérard regarding the future plans of the working group, Holleman wrote that there was indeed much to do, but that he had relinquished his place on the working group and the Commission to his younger Dutch colleague Verkade. Verkade had a strong interest in

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[118] Ibid., 3906–3907.

[119] Verkade knew of about twenty separate publications of the Liège nomenclature. Both Verkade and Patterson published versions of the report liberally annotated with examples and commentary. (Verkade, *A History*, 137–38; Patterson, "Definitive Report"; P. E. Verkade, "La révision récente de la nomenclature des combinaisons organiques," *Recueil des travaux chimiques des Pays-Bas* 51, no. 2 (1932): 185–217.) Gérard praised Verkade's account: "If many of our colleagues undertook a task like that which you have realized, the question of nomenclature would quickly make very great progress." (Gérard to Verkade, 14 May 1932, IUPAC papers Box 52.) The editor of the *Journal of the American Chemical Society*, reluctant to devote twenty-one pages to Patterson's annotated account, suggested instead simply printing a translation of the Definitive Report. Noyes wrote the editor stating that "the value of the report would be almost completely destroyed" without Patterson's comments and examples, and that "it would be a great misfortune and greatly injure the prestige of American chemists if it does not appear in our Journal." The editor capitulated. (Noyes to Lamb, 16 Jan 1933, Noyes papers, Box 11, Folder General Correspondence 1933.) The American Chemical Society committee on Nomenclature, Spelling, and Pronunciation sent reprints of Patterson's annotated Liège rules, along with the *Chemical Abstracts* Directions for Assistant Editors and Abstractors (presumably also written by Patterson), to chemists with questions regarding matters of nomenclature. (Crane to members of ACS Commission on Nomenclature, Spelling and Pronunciation, 5 Jun 1939, Noyes papers, Box 14, Folder General Correspondence 1939.) By 1946, the committee had distributed 4,000 separate copies of Patterson's article; Verkade, *A History*, 146.

matters of nomenclature and had been participating in meetings of the organic nomenclature commission since 1922; Holleman hoped that he would be named its next president.[120]

The Union, however, was not ready to let Holleman go. The IUPAC biochemical nomenclature commission had arrived at an impasse, and Biilmann and Gérard wanted Holleman to remain president of his commission for a little while longer to help clean up the mess. Holleman reluctantly agreed, provided that Biilmann and Gérard understood that Verkade, not Holleman, would be the one reading and commenting on nomenclature rules.[121] This was fine with the IUPAC leaders. The conferences of The Hague and Liège had shown that the making of international nomenclature rules was as much a matter of diplomatic maneuvering as chemical logic, and it was in the former area that Holleman's assistance was decisive.[122]

The comments on the Union's decisions on the nomenclature of biochemistry, submitted under Holleman's name but presumably penned by Verkade, illustrated this dual nature of the nomenclature expertise that the working group had developed.[123] On the one hand, they critiqued specific rules that the commission had decided upon. Holleman and Verkade were especially critical of decisions that coined and reassigned suffixes with little regard for existing usage, and the use of generic terms to refer to groups of substances that shared no distinctive structural feature. On the other hand, the two Dutchmen also proposed a set of best practices in nomenclature rule-making, including naming a working group to bring

[120] Holleman to Gérard, 24 Oct 1931, IUPAC papers, Box 52.

[121] Verkade did not mention assisting in such a way before 1930; in fact, he criticized the drafting of the Liège rules as "careless" and "not in general inspired," and Holleman's leadership of the working group as "authoritarian." Verkade, *A History*, 124–25.

[122] Gabriel Bertrand, president of the biochemical nomenclature commission since 1922, was upset at Gérard and Biilmann's proposal that the organic nomenclature commission review the biochemical commission's decisions. He agreed to do so only after a meeting with Holleman, Gérard, and Biilmann in Paris. Gérard and Biilmann to Bertrand, 24 Oct 1932, Bertrand to Biilmann, 22 Dec 1932, Gérard to Holleman, 28 Feb 1933, IUPAC papers, Box 52. There is a long story behind the dysfunction of the biochemical nomenclature commission. Bertrand consistently advanced his own ideas, to the frustration of the commission's British and American members.

[123] Holleman signed and dated the report, but interlinear corrections to the typescript were all in Verkade's hand.

together the scattered decisions of the commission and critiques thereof and to develop a list of rules for collective consideration and debate. The members of this working group – especially its president – should read and speak French, English and German. They even suggested that the president of the organic nomenclature commission also be named president of the biochemistry nomenclature commission.[124] This attempted annexation came to nothing, but in 1936, the Union did reorganize its biochemical nomenclature commission on the model of the organic nomenclature working group, as Holleman and Verkade had recommended.[125] The episode confirmed the status of the organic nomenclature working group as experts on matters of both nomenclature and nomenclature-making.

In 1934, Verkade formally succeeded Holleman as president of this commission. In submitting his vote for Verkade, Patterson wrote that like Holleman, Verkade "combines the advantages of excellent linguistic ability and citizenship in a small nation."[126] Patterson stayed on as representative of *Chemical Abstracts*. Friedrich Richter – who became editor of *Beilstein* after Prager, a Jew, stepped down under ominous circumstances – rounded out the working group.[127] The incumbent British and Italian representatives remained nominal members of the committee, but the group's work increasingly took on the character of negotiations between the American and German institutions of chemical information, mediated by Verkade.[128]

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[124] Holleman [Verkade], typescript comments on biochemical nomenclature of 15 April 1933 (7pp) and undated (1p; probably enclosed with the former), IUPAC papers, Box 52, Folder 1932; Holleman to Parravano, 5 Nov 1934, IUPAC papers Box 52.

[125] IUPAC Comptes Rendus 11 (1934): 17–18; IUPAC Comptes Rendus 12 (1936): 15–16.

[126] Patterson to Gérard, 8 Jan 1934, IUPAC papers, Box 52.

[127] The reorganization of the organic nomenclature working group in this manner took place in two stages, at Union conferences in 1934 and 1936. The larger Commission was dissolved. The inorganic and biochemical nomenclature commissions were reorganized along the same lines. Verkade, *A History*, 189–94.

Prager, the longtime editor of *Beilstein*, stepped down in May 1933, shortly after the Nazi regime took power. According to his colleague Dora Stern, Prager was pressured to lay off two of his subordinates because they were Jewish, and he chose to resign instead. He died a year later. Dora Stern, "Bernhard Prager – Editor of *Beilstein* 1907-1933," *Journal of Chemical Education* 24, no. 12 (1947): 592– 593.

[128] According to Verkade, Barger and Betti did not actively participate in the committee's work after 1934. Whether this was out of lack of interest, knowledge, linguistic ability, or opportunity is not clear. Verkade, *A History*, 199.

Through correspondence with Verkade and Richter, Patterson drew upon aspects of the nomenclature used in *Chemical Abstracts* and in *Beilstein* to formulate a rule for naming organic compounds containing arsenic, phosphorus, and antinomy. Richter agreed to Patterson's rule; in return, Patterson and Crane agreed to change *Chemical Abstracts* index names to conform to the rule.[129] After much negotiation, Crane agreed to exclude certain common ring systems from Patterson's system for ring numbering in *Chemical Abstracts*; in return, Richter agreed to adopt Patterson's nomenclature and numbering in *Beilstein*.[130]

It was not entirely smooth sailing for Verkade's committee. In 1930, Grignard had taken the helm of the *Traité de Chimie Organique,* a massive collaborative project to produce an authoritative French-language reference text.[131] In 1935, the first volume of Grignard's *Traité* was published. Grignard did not adhere to the Liège rules in the work, but developed an alternative, more rigorous system of nomenclature incorporating the proposals that he had advanced during the late 1920s. In consultation with the presidents of the inorganic and biochemical nomenclature commissions, Verkade determined that Grignard might have to be brought back into the organic nomenclature commission, which was then without a French representative. Verkade was to contact his fellow committee members "to consider whether the presence of M. Grignard would be absolutely necessary." The invitation was never issued; Grignard died unexpectedly in December 1935. Later on, others would step up to defend the priorities for which Grignard had fought – logic and rigor in nomenclature over simplicity of

[129] Patterson to Members of the American Committee on Organic Nomenclature, 7 May 1936, W. A. Noyes papers, Box 12, Folder "Noyes 1936."

[130] Verkade, *A History*, 197.

[131] Blondel-Mégrelis, "Victor Grignard," 227. The *Traité* was published in 23 volumes from 1935 through 1959. According to Mary Jo Nye, Grignard's decision not to make use of the electron theory of valence, then emerging as an important means of predicting and explaining the course of chemical transformations, limited the use of the text; Mary Jo Nye, "Chemical Explanation and Physical Dynamics: Two Research Schools at the First Solvay Chemistry Conferences, 1922–1928," *Annals of Science* 46, no. 5 (1989): 472–473.

names and consistency with existing practice.[132] But never again would a chemist of Grignard's stature seek to intervene in matters of nomenclature. Such matters were left to the experts.

In 1940, the catalog of ring systems that Patterson had been working on since 1921 was finally published. The *Ring Index* provided names and numberings for more than 4,000 ring systems over 660 pages, following rules definitively approved by Verkade's committee.[133] It was a considerable achievement for Patterson, *Chemical Abstracts* (where Patterson's procedures had long been in use), and IUPAC. Patterson dedicated the work to Holleman.[134] Its publication, however, was overshadowed by events of greater import for international scientific cooperation.

In April 1940, with war sweeping through Europe, the ever-upbeat Patterson wrote to Gérard: "The commission of organic nomenclature seems to be inactive, as I have not heard from Chairman Verkade for a long time. I will hold myself in readiness, however, for any work which the Chairman may think it advisable to undertake." Patterson and Verkade did return to work in 1946. Their commission, *Chemical Abstracts*, the Liège Nomenclature, the International Union, and even *Beilstein* all survived the war – crucial, if little-noticed, parts of the foundation on which postwar chemical science and industry would be built.

Conclusion

Considered as a means of ordering chemical compounds, the sixty-eight rules of the Liège nomenclature were quite similar to the sixty-two rules codified by the Geneva Congress. Each system addressed more or less the same collection of organic chemical compounds,

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[132] E.g., Noel Lozac'h, "French Organic Nomenclature," *Journal of Chemical Documentation* 1, no. 1 (1961): 1–6.

[133] M. F. Roy, "The Ring Index: A List of Ring Systems Used in Organic Chemistry (Patterson, A. M.; Capell, L. T.)," *Journal of Chemical Education* 18, no. 8 (1941): 398.

[134] Verkade, *A History*, 180–81.

prescribing nearly the same set of prefixes, suffixes, and roots to be assembled in the same general way to form very similar systematic chemical names.

Considered as a means of ordering chemists and chemical texts, however, the two sets of rules were very different. The Geneva rules were a system of names to be used exclusively in chemical indexes, created for index-makers but not by them. The Liège rules were a nomenclature specifically *not* to be used in indexes, developed in large part by chemists who had dedicated their careers to making such reference works. The Geneva rules offered the world's chemists a top-down logical procedure for uniquely naming every compound, developed by the greatest authorities in the field. The Liège rules codified a set of rules and a method of rule-making that *protected* chemists against the imposition of any such logically rigorous nomenclature from above. Both at the level of nomenclature and nomenclature rule-making, Holleman's working group *codified flexibility*.

The lesson of the four days of the Geneva Congress had been that chemical naming could be reduced to an algorithm and deployed as a precise tool for ordering chemical knowledge. The lesson of the six years that it took to create and secure the approval of the Liège nomenclature was that chemical names were always more than tools. They were the embodiments of different ways of thinking about chemical compounds and their relationships. For some, they were an expression of the spirit of different national chemical communities. Most importantly, they were the backbone of enormous reference enterprises, relied upon by chemists around the world and sources of power and prestige for the chemical societies that published them – a backbone that could be broken if reconfigured too quickly or by someone without the proper experience.

In 1930, IUPAC President Einer Billman addressed special thanks to Arnold Holleman "for the fashion in which he has conducted, these past years, the work of the Commission for

the Reform of the Nomenclature of Organic Chemistry."[135] The process had not been pretty, but Holleman had managed to negotiate a set of nomenclature standards "based on" the Geneva nomenclature, a set of rules understood as superseding those of the Geneva Congress rather than merely extending them or entirely replacing them. Other chemists had contributed to this achievement: Patterson, though his deep insight into the interdependent of matters of nomenclature, Pictet, through his steadfast voluntarism (until 1928, at least), and even Grignard, whose antagonism helped to redirect the path to IUPAC nomenclature through negotiations with *Beilstein* and *Chemical Abstracts*. Still, Holleman had earned the special thanks for which Biilmann singled him out. By recognizing and navigating the delicate relationship between the lexical order of chemical compounds and the social order of interwar chemistry, he laid the foundation for the future of international chemical nomenclature, and, to no small degree, for IUPAC itself.

[135] IUPAC Comptes Rendus 10 (1930): 41.

Conclusion

> Ramon Fernandez, tell me, if you know,
> Why, when the singing ended and we turned
> Toward the town, tell why the glassy lights,
> The lights in the fishing boats at anchor there,
> As the night descended, tilting in the air,
> Mastered the night and portioned out the sea,
> Fixing emblazoned zones and fiery poles,
> Arranging, deepening, enchanting night.
>
> Oh! Blessed rage for order, pale Ramon,
> The maker's rage to order words of the sea,
> Words of the fragrant portals, dimly-starred,
> And of ourselves and of our origins,
> In ghostlier demarcations, keener sounds.

Wallace Stevens was no chemist. As both a poet and an insurance executive, however, he knew something about two principal features of the chemical sciences: the extravagant allure of creation, and the power of mundane instruments of order. The final stanzas of his 1934 poem "The Idea of Order at Key West" are an assertion that order and creation are linked: human-made nets of words give shape to the world that they capture. The nature of the joints between language, experience, and reality is a signal concern of modern humanism. This dissertation is a history of such joints, manifested as chemical names, nomenclature rules, classification schemes, international commissions, editorial enterprises, and reference publications.

By way of conclusion, I will revisit the main threads of this history and consider its legacy through the decades following the 1930s. I will close by addressing a development that, according to some, was destined to cut through systematic nomenclature's Gordian knot of

classification and identity: techniques for managing chemical information on machines rather than on paper. As the philosopher François Dagognet wrote in 1969, when studying contemporary chemistry, "One must pass from the old problem of order and of ordination to that of the indispensable *ordinateur*" – the computer.[1]

Indeed, the cross-disciplinary focus on molecules throughout post-World War II science, technology, and medicine emerged alongside the increasing reliance of these fields on computer-based tools and methods. The development of the digital infrastructure of chemical information played a central role in each of these trends. The molecularization and the computerization of science shaped and reinforced each other through the medium of databases and data structures for putting molecules on computers.

That is a story for another day. However, a brief sketch of the work of one pioneer of such systems will suggest that, in fact, computer-based representations of organic compounds were grounded in the principles of systematic nomenclature, and that they entrenched these principles still more deeply within the structure of organic chemistry. It will also suggest how the impression of a hard break between chemical names and their digital analogs came to be.

The history and historicity of chemical names

In the 1895 course on nomenclature discussed in Chapter 4, Amé Pictet explained to his students that the chemical names of their day were heterogeneous and historical, an accretion of chemical ideas, some obsolete, some enduring. The same was true of systematic nomenclature and chemical reference works in the mid-1930s. *Beilstein* mixed information published in the recent and distant past under the headings of individual organic compounds. The nomenclature and notation systems used in this and other reference works expressed

[1] François Dagognet, *Tableaux et langages de la chimie: Essai sur la représentation* (Seyssel: Champ Vallon, 2002), 154. Originally published in 1969.

diverse notions of chemical order, developed at different times and under different circumstances. The chemical reader of the 1930s encountered simple empirical formulas side-by-side with complex systematic names, system numbers locating compounds within an elaborate classification and "half-rational" trivial names festooned with Geneva-style prefixes and suffixes. Organic chemical nomenclature, circa the mid-1930s, was a palimpsest of gaps crossed and recrossed: gaps between ideals and practices of nomenclature, between elite academic chemists and anonymous editors and indexers, between international competition and international coordination.

To sum up a few of these crossings: chemists of the 1880s coined names based on structural formulas to help keep track of new synthetic organic compounds, but these names brought about synonymy and confusion instead. Charles Friedel and his Parisian atomist allies took this confusion as an opportunity to reform chemical pedagogy within France and to burnish French chemistry's international prestige. At the Geneva Congress, Adolf von Baeyer redirected nomenclature reform toward the sole purpose of chemical bibliography. However, editors of chemical reference works found Geneva names unhelpful for their purposes and developed alternative approaches to naming and notation. Elite chemists took up nomenclature as a centerpiece of transnational cooperation in the years before World War I; during the war, they turned to chemical reference works as a pivotal resource for economic and intellectual competition. Within the International Union of Pure and Applied Chemistry, in the 1920s, organic nomenclature reform was at first a mechanism for legitimating an organization that excluded many of the world's leading chemists and then a means of bringing these German scientists and editors back into the fold. The Union's nomenclature rules, approved in 1930, derived authority from the support of representatives of *Beilstein* and *Chemical Abstracts*, even though both publications were explicitly exempt from following these rules. Each of these shifts

deepened the sense that, on the one hand, systematic names should in principle correspond in a unique, rule-bound fashion to structural formulas, but that, on the other hand, they did not and perhaps could not do so in practice.

World War II and the Cold War brought profound changes to the organization and funding of science, scientific international relations, and the instruments and methods of chemistry. Nevertheless, many of the institutions, publications, and ideas addressed in this dissertation endured, though not without difficulties. As with the International Association of Chemical Societies during World War I, most of the activities of IUPAC came to a halt during World War II. General Secretary Jean Gérard kept the Union headquarters open and maintained communication as best he could. Carrying on such activities in occupied Paris meant cooperating with the German occupiers; after the liberation, Gérard was arrested for collaboration and reluctantly resigned from his post within IUPAC. Resuming contact after the war, IUPAC officers representing the victorious Allies expelled Germany and Japan from membership in the Union, and removed suspected Nazi sympathizers from leadership positions. Still, they saw the well-established Union as their best option for the continued pursuit of international cooperation in chemistry. IUPAC leaders built ties with UNESCO and other international institutions. In 1951, Union delegates voted to readmit Japan and West Germany. Over the ensuing decades, the Union faced the political challenges common to international scientific organizations during the Cold War: visas denied, meetings cancelled, clashes over the politics of geopolitical neutrality. Though troublesome, such conflicts were a mark of the ever-growing scope and influence of the Union's activities in developing chemical standards and facilitating international exchange in emerging fields of chemistry.[2]

[2] Roger Fennell, *History of IUPAC, 1919-1987* (Boston: Blackwell Science, 1994).

The Union's organic nomenclature commission took longer than the Union itself to return to work. One member died during the war; another, *Beilstein* editor Friedrich Richter, was expelled along with the rest of the German delegates. However, the commission president P.E. Verkade and longstanding member Austin Patterson remained dedicated to the work of nomenclature reform, and in 1951, Richter was restored to his place in the group. The commission resumed its efforts to expand the 1930 Liège rules into a system of organic nomenclature that would, like no previous international system, actually apply directly to most compounds of interest to organic chemists. In 1957, the group published the first two sections of its revised nomenclature system, addressing hydrocarbons and rings – the parent compounds whose names would serve as the roots for the names of most others. In 1964, the commission finalized its rules for naming derivatives of these compounds containing functional groups. Further sections followed, addressing compounds on the border between organic and inorganic chemistry.[3]

In this manner, the postwar IUPAC nomenclature commission moved slowly but steadily forward, extending previous rules to encompass more and more organic compounds rather than rewriting the rules from the ground up. In a 1951 newsmagazine column, Patterson told a general readership of American chemists what to expect from an upcoming international symposium on chemical nomenclature: "Some decisions will become definitive, some will be tentative, and some matters will merely be discussed without decision. That's the way the international machinery works."[4] The debates over foundations that characterized nomenclature reform efforts from the 1880s through 1930 gave way to an "international machinery" of protocols and experts.

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[3] Pieter Eduard Verkade, *A History of the Nomenclature of Organic Chemistry*, trans. S. G. Davies (Boston: Reidel, 1985), 203–232, 299–319.

[4] Austin M. Patterson, *Words about Words: A Collection of Nomenclature Columns* (Washington, DC: American Chemical Society, 1957), 4.

As for *Beilstein*: even before World War II, the publication was in trouble. The editorial staff had to make do without some of their most experienced colleagues; editor-in-chief Bernhard Prager and other Jewish staff members had resigned or been fired under pressure from Nazi authorities. By the end of the war, a handful of staff members and a mountain of unprocessed literature were all that remained of the imposing enterprise that Paul Jacobson had built. However, IUPAC, the American military authorities governing much of postwar Germany, and the influential German chemist Otto Hahn, among others, had various interests in resuscitating the formidable publication. With their assistance, *Beilstein* survived. Though the information in its supplements typically lagged two decades or more behind the primary literature, *Beilstein* nevertheless retained its place as the authoritative source for information about organic chemical compounds.[5] *Chemical Abstracts* faced no such existential threat; still, keeping up with the literature grew ever more expensive. In the mid-1950s, the American Chemical Society leadership decided to remove *Chemical Abstracts* from the society's annual budget. Henceforth, the newly-christened Chemical Abstracts Service would be expected to run as a self-sufficient enterprise.[6]

The idea of chemical naming first articulated at the Geneva Congress – the principle that an organic compound's name ought to precisely express the pattern of atoms and bonds in its structural formula – faced perhaps the gravest challenge of all in the decades following the 1930s. The development of quantum chemistry and stereochemistry provided chemists with formalisms and justifications for extending their visual reasoning about molecules in new directions. According to these new theories, organic compounds were understood as dynamic and three-dimensional, in contrast to the static, two dimensional depictions presented by

[5] Michael D. Gordin, *Scientific Babel* (Chicago: University of Chicago Press, 2015), 283–289.

[6] Dale B. Baker, interview by Robert V. Williams and Leo B. Slater at Columbus, Ohio, 9 June 1997 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0160), 37–38.

structural formulas.[7] And yet, Richter noted in 1951, both systematic names and structural formulas themselves "persist more or less unchanged in spite of deep changes due to evolution of the ideas connected with them."[8] In fact, if anything, it was the capacity of new mathematical models and data from new analytical tools to be translated into the established graphical idiom of structural formulas that secured a place for these theories and instruments in organic chemistry at large.[9]

Following this reasoning, the persistence of systematic organic chemical nomenclature might therefore be explained as a side-effect of the persistent fondness of organic chemists for the structural formulas that are the basis of these names. "We are hooked on these little diagrams, aren't we?" mused chemists Roald Hoffmann and Pierre Laszlo in 1991.[10] This is undoubtedly so. But why *these* little diagrams – conventional representations which predated, inter alia, the discovery of the electron, the foundation for explanations of chemical phenomena since the early twentieth century? The "little diagrams" that chemists continue to use are those on which the Geneva Congress established its approach to chemical representation and classification. This is no coincidence. The principles of systematic nomenclature have helped to

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[7] Kostas Gavroglu and Ana Simões, *Neither Physics nor Chemistry: A History of Quantum Chemistry*, (Cambridge, MA: MIT Press, 2012); Peter J. Ramberg, *Chemical Structure, Spatial Arrangement: The Early History of Stereochemistry, 1874-1914* (Burlington, VT: Ashgate, 2003); John W. Servos, *Physical Chemistry from Ostwald to Pauling: The Making of a Science in America* (Princeton: Princeton University Press, 1990); Mary Jo Nye, *From Chemical Philosophy to Theoretical Chemistry: Dynamics of Matter and Dynamics of Disciplines, 1800-1950* (Berkeley: University of California Press, 1993); Jeremiah James, "Modeling the Scale of Atoms and Bonds: The Origins of Space-Filling Parameters," in *Objects of Chemical Inquiry*, ed. Ursula Klein and Carsten Reinhardt (Sagamore Beach, MA: Science History Publications/USA, 2014), 281–320.

[8] Friedrich Richter, "Basic Features of Nomenclature in Organic Chemistry," in *Chemical Nomenclature*, Advances in Chemistry 8 (Washington, DC: American Chemical Society, 1953), 72.

[9] Carsten Reinhardt, *Shifting and Rearranging: Physical Methods and the Transformation of Modern Chemistry* (Sagamore Beach, MA: Science History Publications/USA, 2006); Buhm Soon Park, "In the Context of Pedagogy: Teaching Strategy and Theory Change in Quantum Chemistry," in *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives*, ed. David Kaiser (Cambridge: MIT Press, 2005), 287–322; Leo B. Slater, "Instruments and Rules: R. B. Woodward and the Tools of Twentieth-Century Organic Chemistry," *Studies in History and Philosophy of Science Part A* 33, no. 1 (2002): 1–33; Thomas Steinhauser, "The Synergy of New Methods and Old Concepts in Modern Chemistry," in Klein and Reinhardt, *Objects of Chemical Inquiry,* 259–279.

[10] Roald Hoffmann and Pierre Laszlo, "Representation in Chemistry," in *Roald Hoffmann on the Philosophy, Art, and Science of Chemistry*, ed. Jeffrey Kovac and Michael Weisberg (New York: Oxford University Press, 2012), 164.

maintain the relevance of a 125-year-old conception of chemical identity through the present day. The following story suggests how.

"A third-order science of organization"

Shortly after the end of World War II, the British chemist G. Malcolm Dyson published a book entitled *A New Notation and Enumeration System for Organic Compounds*. He began *A New Notation* with an attack on an old notation: systematic nomenclature. Dyson asserted that the divergence of nomenclature practices in different nations and publications had rendered the 1930 Liège nomenclature a dead letter as soon as it was approved. Further, Dyson held that the lack of a unified international system of organic nomenclature was to blame for a twenty-five-year backlog in the compilation of chemical reference works. Dyson quoted a lament of his countryman Clarence Smith, editor of the *Journal of the Chemical Society*: "Could we but wipe out all existing names and start afresh, it would not be a very difficult task to create a logical system of nomenclature." But this was impossible, Smith (and Dyson) concluded; chemists had to "suffer for the sins of our forefathers in chemistry" when it came to nomenclature.[11]

Dyson had an alternative: an approach to chemical notation that, in his view, would indeed allow to chemists to "start afresh." Looking beyond the limited vocabulary of chemical symbols and numbers, Dyson mined the rest of the keyboard to develop a system of unique, compact, text-based representations of structural formulas. He called them "ciphers."[12]

Ciphers, Dyson argued, offered chemists two principal advantages over systematic names. First, unlike nomenclature, there were no pre-existing ciphers to impose difficult tradeoffs between the new system's internal logic and its consistency with established usage. Second, ciphers were well-adapted for use on punched cards, a medium of information

[11] G. Malcolm Dyson, *A New Notation and Enumeration System for Organic Compounds* (London: Longmans, Green, 1947), 2.

[12] Ibid., 4.

technology that was just beginning to be applied to ordering chemical compounds.[13] Recorded on punched cards, ciphers allowed chemical records to be rapidly sorted and searched by structural features; machines could even calculate the empirical formula of a compound from its cipher.[14] Dyson proposed that the pairing of ciphers and punched cards was the resource that editors needed to clear the backlog in indexing.

A New Notation made Dyson an authority on machine handling of chemical information. Shortly after its publication, Dyson was appointed president of a new IUPAC Commission on Codification, Ciphering, and Punched Card Techniques.[15] In the late 1950s, he took up a position as research director at Chemical Abstracts Service, charged with devising ways to use automation to cut costs and generate profits for the newly self-reliant institution.[16] Both organizations adopted his cipher.[17]

During the early 1960s, in partnership with the chemical firm Du Pont and with funding from the US federal government, Dyson's research department at Chemical Abstracts Service undertook a project to bring digital computers to bear upon the processes of compiling and accessing chemical information. Moving beyond the cipher, the chemists and engineers involved in this project developed a two-sided approach to the unique, unambiguous representation of chemical structures.

The first part was a digital record identifying each atom and bond that made up a molecule, which its creators called a "connection table." This table was formed from a

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[13] On the history of punched card data-processing systems, see James W. Cortada, *Before the Computer: IBM, NCR, Burroughs, and Remington Rand and the Industry They Created, 1865-1956* (Princeton: Princeton University Press, 1993).

[14] Dyson, *A New Notation*, 7.

[15] Verkade, *A History*, 204–205.

[16] Baker, interview by Williams and Slater, 15.

[17] National Research Council Committee on Modern Methods of Handling Chemical Information, *Survey of Chemical Notation Systems: A Report* (Washington, DC: National Academies, 1964), iii. Wiswesser Line Notation (WLN), a cipher which required less specialized knowledge and equipment than Dyson's approach, became more widely used among chemical firms, at least in America. Ibid., 29, 46–47.

structural formula in several stages. One worker filled out a sheet identifying the atoms and bonds of a structural formula, and then another transferred the sheet's data to a punched card. The punched card was input into a computer, and a specially-designed algorithm checked the input data for errors, transformed the table into a compressed form for more efficient storage and search, and reordered the table to ensure that each structural formula corresponded to a unique connection table, regardless of how that formula was initially drawn or recorded.[18]

However, connection tables themselves were not, under most circumstances, to be used to identify chemical compounds. Instead, they served as a starting point for generating a more convenient identifier, which Dyson christened a "Registry Number."[19] Each unique connection table in the computer system was assigned one such number, an arbitrary string of digits containing no structural information. As one of the project's engineers explained, the Registry Number was "the thread that ties together all information associated with a particular compound throughout the developing CAS computer system."[20]

The connection table represented chemical structure; the Registry Number represented chemical identity. The latter enabled Chemical Abstracts Service editors to draw together material from various sources – abstracts, patent records, physical properties, biological behavior, industrial applications – without parsing and comparing connection table after connection table, a prohibitively time-consuming task, even for computers.[21] This, in turn, made it possible for the enterprise to compile (and to sell) specialized reference works for individual clients or niche markets. Linked within the computer system of Chemical Abstracts

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[18] D. J. Gluck, "A Chemical Structure Storage and Search System Developed at Du Pont," *Journal of Chemical Documentation* 5, no. 1 (1965): 43–51; H. L. Morgan, "The Generation of a Unique Machine Description for Chemical Structures," *Journal of Chemical Documentation* 5, no. 2 (1965): 109–110.

[19] Originally, a "register number." G. Malcolm Dyson, "Current Research at Chemical Abstracts," *Journal of Chemical Documentation* 1, no. 1 (1961): 24–28.

[20] Morgan, "Generation of a Unique Machine Description," 107.

[21] Fred A. Tate, "Progress Toward a Computer-Based Chemical Information System," *Chemical & Engineering News* 45, no. 4 (1967): 81–84.

Service, Registry Numbers and connection tables tied chemical identity to structural formulas without requiring chemists to navigate a labyrinthine representation of a structural formula in order to get at chemical identity.

The machine was making the systematic name a thing of the past – so Dyson had suggested in 1947, and so it began to seem to others. In his 1969 book *Tableaux et langages de la chimie*, Dagognet took up the transition from systematic nomenclature to machine-aided management of chemical information as the conclusion of his philosophical study of the history of chemical representation. Dagognet noted the gap between the ideal and the practices of systematic nomenclature and saw a project that had run out of steam. The lesson of the persistent diversity among nomenclature systems, he argued, was that the longstanding dream of "voco-structural correspondence" between chemical substance and chemical word had come to an end. Dagognet took the gap between the ideal of unique, unambiguous names and the actual systematic names used by chemists as a repudiation of systematic nomenclature in general.[22]

In the twilight of one form of representation, Dagognet predicted the dawn of another. Computers, he suggested, would soon become the basis of "a new scriptural discipline" of chemistry, in which laboratory research would take a back seat to the manipulation of chemical information. The chemistry of the future, Dagognet wrote, would be a "third-order science of organization."[23] He believed that the leap into chemistry as information science would also lead chemists back to trivial nomenclature, with the computer filling the role of universal thesaurus: "[T]he chemist will be able to 'babelize' as he pleases. A computer will register, translate, and provide information on the spot. The chemistry of the 20th century will pass from the signal

[22] Dagognet, *Tableaux et langages de la chimie*, 198 and passim.
[23] Ibid., 156, 198.

problem of the formula to techniques of information which liberate the signs."[24] Less artfully put: Dagognet believed that systematic nomenclature was a hopeless mess and that computers would somehow put everything in order. In a way, they did, but not by leaving the ideals of systematic nomenclature behind.

Dyson's critique of nomenclature was polemic. This is clear from the fact that he blamed delays in chemical compilation on the diversity of nomenclature without even mentioning the war that had just concluded. Reviewers noticed. One otherwise positive review took Dyson to task for his harsh treatment of systematic nomenclature, the subject of so much hard work on the part of Patterson and others.[25] Another review described Dyson's own cipher as an adaptation of the Geneva system.[26] The description was apt; Dyson's method followed the procedure set out in the Geneva rules, beginning by locating the longest carbon chain in the structural formula, then addressing side chains, then functional groups. Dyson used symbols rather than prefixes and suffixes, and his ciphers addressed a far broader range of compounds than any international nomenclature rules codified to that point. But the logic of his approach was the logic of the Geneva Congress.

The same was true of connection tables, but the process by which they were formed rendered this logic both more palatable to chemists and more difficult to discern. The easy part about systematic nomenclature was parsing a structural formula into atoms and bonds. The hard part was following the arcane, complex, and frequently counterintuitive rules that were needed to transform this network into one and only one name or cipher. The process of encoding a structural formula as a connection table separated these two parts, entrusting the

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[24] Ibid., 171–172.

[25] F. Y. Wiselogle, "*A New Notation and Enumeration System for Organic Compounds*. G. Malcolm Dyson (Review)," *Chemical & Engineering News* 25, no. 30 (1947): 2175.

[26] James W. Perry, "*A New Notation and Enumeration System for Organic Compounds*. G. Malcolm Dyson (Review)," *Journal of Chemical Education* 27, no. 10 (1950): 581.

first to a human, who could fill out the table of atoms and bonds in any order, and the second to a machine implementation of an algorithm, which guaranteed that no matter how a structural formula was drawn and encoded, it would be mapped to a unique connection table. Whereas it took a chemist with more than a year of special training (and a paycheck to match) to assign systematic names to compounds, connection tables could be created by clerical workers without any particular expertise.[27] Furthermore, since the Geneva Congress, attempts to put the ideal of unique, unambiguous representations of chemical structure into practice had to reckon with the competing pressures of identifying a chemical compound and precisely classifying it according to its chemical structure. Dyson and his collaborators separated these two functions into two forms of representation.

The representation of chemical substances on computers was not a departure from the tradition of systematic nomenclature. It was the apotheosis of that tradition. The disparity between ideals and practices of chemical naming disappeared, but it did not go away.[28] Rather, this gap was removed from the view of chemists who used computer-based versions of chemical reference works, hidden behind Registry Numbers and within algorithms. There it remains. Systematic nomenclature has become, to paraphrase Austin Patterson, simply the way the machinery of chemical information works.

[27] Baker, interview by Williams and Slater, 25, 32; Gluck, "Chemical Structure Storage and Search," 44–45.

[28] For contemporary examples, see Leah R. McEwen and Robert E. Buntrock, ed., *The Future of the History of Chemical Information* (Washington, DC: American Chemical Society, 2014).

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