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# HAS THE TIME COME? THREE ESSAYS ON THE RELATIONSHIP BETWEEN HISTORICAL CONTINGENCY AND POLITICIZATION IN THE IMMEDIATE, SHORT, AND LONG RUN

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# HAS THE TIME COME?
# THREE ESSAYS ON THE RELATIONSHIP BETWEEN HISTORICAL CONTINGENCY AND POLITICIZATION IN THE IMMEDIATE, SHORT, AND LONG RUN

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## Abstract

Taking the form of startling populist victories, massive waves of protest, or politico-sanitary COVID crises, historical events have returned to the forefront of politics in many places around the world. In this backdrop, this dissertation examines how historical events affect levels of politicization over time time. Previous research is focused primarily on verifying event effects on political attitudes, but provides few insights into how different these effects might be across different events, people, or time horizons, or how they might influence foundational political attributes. This dissertation attempts to move the literature towards these issues by conducting three pieces of research related to these questions. The first of these essays, "Can Historical Events Generate Persistent Cohort Effects in Politicization?" examines how events affect politicization levels in the long run. It uses thirty years of data on political talk to evaluate if twentieth-century events from five Western European countries are associated with cohort-level differences in the frequency with which people talk about politics. Results reveal wide heterogeneities in the performance of events as predictors of this behavior, and associations between robustness of influence and an event's capacity to generate lasting reconfigurations of state action, on one hand, and between direction of influence and the divisive or unifying nature of an event, on the other. The second essay, "Can Historical Events Alter Social Gaps in Politicization?" analyzes how social disparities in political talk changed in West Germany before and during German Reunification, and how these changes compared to equivalent variations for France over the same periods. Results suggest that events increase socioeconomic disparities in political talk for the population at-large while decreasing them for young adults. The third essay, "Taking the Victory of Jair Bolsonaro Personally," shows the results of an ethnographic fieldwork in

São Paulo that investigated the immediate everyday political effects provoked by the victory of Jair Bolsonaro. It describes how this victory generated a political environment of "engagement without action," and suggests that events produce immediate politicization bursts by infiltrating political stories into domains of social experience that are more frequently applied than politics as frames of social experience.

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*To Edith and Fernado,*
*my parents,*

*for everything.*

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# Can Historical events Generate Persistent Cohort Effects in Politicization? Evidence from Western Europe, 1973–2002

## INTRODUCTION

COVID crises; Arab springs; Brazilian impeachments; the victories of Brexit and Donald Trump. After the relative historical restraint of the nineties—a condition that Francis Fukuyama famously captured in his aphorism on the "end of history" (1992)—a diverse field of political contingencies has taken center stage in contemporary politics across the globe. These major political disruptions call for an assessment of the effects they may carry in the long run not only as triggers for institutional change, but also as makers of lasting impacts in individual political attributes.

Although the literature tends to agree in seeing historical events as impactful long-term devices for political socialization, there are still many issues we do not know with sufficient precision concerning how much historical events affect foundational political behaviors or attributes, and how differently they might do so. This gap in the literature is as much a consequence of the substantive focus of previous research, which centers mainly in studying event effects on collective memories and issue and partisan orientations, as of several analytical and theoretical features that have limited the conduction of comparative analyses on event effects.

In this research, I seek to contribute to specific knowledge on how, how frequently, and how much can historical contingencies generate lasting cohort differences in the frequency with which people talk about politics (henceforth "political talk"), a measure of

politicization, and a relevant political conduct in an of itself.[1] By doing so, I hope to make a set of inter-related theoretical, methodological, and substantive contributions.

From a theoretical standpoint, I put into dialogue discussions from event-based and life-cycle empirical research from studies on social networks, social cognition, and memory to argue that the long-term effects of an event follow an asymmetric bell shape that peaks for those who came of age when it occurred. These differential effects are generated by a curve of historical sensibility of a similar shape that provokes differential reactions to an event across age when it occurs. Over time, they are maintained by cohort homophily and by cognitive and memory processes that make experiences from young adulthood better remembered, more frequently rehearsed, and thought of as more important.

Analytically, I develop a quantitative research design to evaluate event effects comparatively. It is based, first, on robustly identifying concrete historical occurrences that approximate the definition of "historical event" developed by event-based (or "evenemential") research within historical sociology; second, in the generation, for each of these occurrences, of an asymmetrical bell-shape variable of event sensitivity, and third, in evaluating the performance that these variables of "event exposure" have as predictors of political attributes by conducting a quantitative analysis that controls for confounding factors associated with historical contextual socialization (as opposed to the contingency-based type of historical socialization related to having experienced a historical event), and for model selection uncertainty.

Substantively, using thirty years of survey data on political talk, I apply this research design to evaluate how twentieth-century historical events from five Western European

[1] Political talk plays an important role in processes of political information diffusion, political identity formation, and interpersonal political influence (Snow and Benford 1988; Steinberg 1999; Polletta 2006). It has also been identified as a relevant factor for democratic quality (Habermas 1984), and individual levels of political participation (McClurg 2006; Klofstad 2010).

countries perform as predictors of this behavior. I center this examination in the meta-analysis of 360 regressions that include cohort-level variables controlling for historical socialization factors related to historical context.

My results produce a more nuanced portrait of event effects than the one currently held by the literature. I find strong evidence that some events are capable of generating persistent cohort differences in how frequently people talk about politics. But more broadly, my results also show wide heterogeneities in how robustly historical contingencies affect political talk, and in what direction they do so. A preliminary examination of these heterogeneities suggests that robustness of influence might be associated with the age of an event, and with its capacity to generate sudden but lasting turnarounds in state action. Direction of influence, on the other hand, might be related to how cohesive or divisive reactions to a historical event are.

The paper is organized into five sections. The first reviews the current state of the literature. The next discusses how long-term event effects on individual-level behavior follow a cohort-based bell curve, and how this curve solves ambiguities in generational and event-based research regarding how to model these effects. The third section describes the paper's empirical research strategy. The fourth section discusses its empirical results. The concluding section recapitulates the paper's findings and contributions, and points to directions for future research.

## HISTORICAL EVENTS AS POLITICIAL SOCIALIZATION DEVICES: WHAT WE KNOW, WHAT WE DON'T KNOW, AND HOW WE CAN KNOW BETTER

Most of what we know about how historical events persistently affect individual-level political attributes comes from investigations that fall under the rubric of "generational

research". The theoretical cornerstone of these works is *The Problem of Generations,* a classic sociological essay written by Karl Mannheim almost a hundred years ago (Mannheim 1952 [1927]). In this text, Mannheim is credited with discussing how historical occurrences unleash processes of "generational imprinting" that make members of cohorts who come of age when they occur react more strongly to them, and acquire political attributes in line with the specific characteristics of the contingency that is occurring.

Empirical findings supporting these processes date back to at least 1966, when Maurice Zeitlin found that the attitudes of working class Cuban men towards Communism and the Cuban Revolution varied according to the particular political situations they experienced when they were young (Zeitlin 1966). In the seventies and eighties, many other investigations began producing similar findings. Most of them came from U.S.-based research that found that people that entered adulthood during the sixties carried distinctive political attitudes that developed in connection to the historically turbulent context under which they came of age. Two works from this line of inquiry stand out for their comprehensiveness. One is Jennings and Niemi's analysis of a longitudinal data that tracked political attributes of 1965 high school seniors and their parents (Jennings and Niemi 1981). They explored how the political orientations of these high school seniors evolved over time, and how this variation differed from the ones their mothers and fathers underwent. The other one is Delli Carpini's examination, using twenty-eight years of National Election Surveys (NES) data, of how the "sixties generation" in the United States carried different political characteristics than the rest of their fellow nationals (Delli Carpini 1986). Both works found that people who came of age during the sixties exhibited particularly distinctive characteristics related to issues that organized the political debate during the sixties—for example, school desegregation.

Processes of generational imprinting have also been identified for party identification since the seventies, when cohort-based analyses made use of repeated cross sectional data (typically, the NES surveys) to show that party adscription was patterned at least as strongly by birth location as by age, which was then dominant explanatory temporal factor to understand people's partisan preferences (Glenn 1972; Abramson 1979). At this time, this body of work did not explicitly relate observed cohort patterns of partisanship to differences in historical experiences, preferring instead to associate them with broader differences in "formative socializations" (Abramson 1976). But after the 1990s, cohort investigations on partisanship have started to relate birth year differences in this political orientation more vocally to differences in the historical occurrences that different people experienced during young adulthood (Osborne, Sears and Valentino 2011; Bartels and Jackman 2014; Ghitza and Gelman 2016).

Research on collective memories has also generated results supporting generational imprinting processes. At the end of the eighties, Harold Schuman and Jacqueline Scott analyzed open-ended surveys on the historical experiences that people considered most relevant. They found that historical occurrences experienced during young adulthood were better remembered and more likely to be considered important (Schuman and Scott 1989). Since then, their colleagues have found similar findings in other national contexts (see Schuman and Corning 2012 for an overview; see also Griffin 2005).

The generational findings produced by investigations on political attitudes, partisanship, and collective memories have been relevant for establishing an agreement in the literature that sees historical events as important long-term devices for political socialization. However, there still remain important gaps in our knowledge of how historical events fulfill this role.

We still don't know much about how events affect political behaviors or foundational political attitudes related to politicization. So far, only Delli Carpini and Jennings and Niemi have investigated generational imprinting processes on factors associated with political engagement. Their findings do not converge. Delli Carpini found that members of the "sixties generation" exhibit lower levels of political interest. Jennings and Niemi's results, on the other hand, showed that members of this cohort segment have a higher sense of political efficacy and give more weight to politics than their parents (Delli Carpini 1986; Jennings and Niemi 19891).

We also don't know much about how historical events might affect people differently in diverse country and historical settings. Many key generational findings, especially those related to political attitudes, come from investigations focused on the study of the "sixties generation" in the United States (Cutler 1974; Miller 1992; Jennings and Markus1984; Jennings 1996; see Dinas 2013 for an exception).

Finally, besides knowing that events "matter" politically for those who became adults at the moment where one occurred, more precise information on how, how frequently, and how much they do has yet to be produced. So far, the literature has mainly focused on verifying whether event effects exist than in researching heterogeneities of political influence among them.

In light of this disciplinary context, conducting a comparative investigation on the effects that events have on political talk, a foundational political conduct and an everyday measure of politicization, appears to be a productive direction of research. In my view, the most fruitful conduction of this investigation will be one that is also capable of addressing several analytical limitations in the way previous research has analyzed long-term event influences on individual-level political attributes.

First, because generational analyses seldom include controls related to the social characteristics of the times when people came of age, they cannot separate the effects of processes of historical socialization related to *historical contingency* (i.e., events) from those associated merely with *historical context* (Inglehart 1981; see also Davis 1975). A plausible explanation about the distinctiveness of the "sixties' generation" can be built around the historical ruptures it experienced during young adulthood, but it can also be connected to the social conditions that prevailed when it came of age—for example, increased affluence, or higher rates of educational attainment level.

Second, generational investigations have often been rather lax in selecting the historical phenomena with which to test generational imprinting processes. Most voices in the literature agree that the specific historical entities that trigger these processes are punctuated, disruptive events (Mannheim 1952 [1927], 303, 310; Jennings 1984, 1001; Weil 1987, 309; Schuman and Scott 1989, 359; Delli Carpini 1989, 24). However, the empirical findings from generational literature frequently stem from investigations that evaluate imprinting processes in connection with wide-range historical periods—like Italian fascism or the Weimar republic (Barnes 1972; Weil 1987)—or nebulous moments of historical contingency—like the "dramatic historical occurrences" of the sixties (Jennings and Niemi 1981, 8). Research that takes a data-driven approach to investigate generational imprinting processes has done a better job in keeping their analysis restricted to specific political ruptures. However, the inductive way in which they identify them also poses analytic challenges: an "event" can be considered everything from the Assassination of John F. Kennedy (Schuman and Corning 2012) to the midterm election in the 1950s (Bartels and Markus 2014).

Third, generational findings regularly stem from quantitative analyses where the imprinting capacity of an event is measured by a cohort dummy separating cohorts that came of age when it occurred from others that didn't. This analytical choice is implicitly but inevitably premised on a particular take on event effects. It sees historical contingencies as capable of generating lasting political influences only for those who were young adults when one occurred, assuming in consequence that people from other ages are historically insensitive. These assumptions are seldom explicitly discussed, commented, or justified. Most generational investigations dedicate small time to theoretically discuss the imprinting process they research. They reference *The Problem of Generations* for more information, and then proceed to conduct their empirical analysis. However, because Mannheim's essay is not mainly oriented at discussing historical contingency as a socializing force (DeMartini 1985), this essay does not contain a compact theory of how generational imprinting processes actually work. Currently, we lack a clear theory of event effects capable of helping us understanding the mechanisms through which event influences originate and maintain over time, and guiding the conduction of comparative work on event effects.

In sum, in the current state of the literature, producing concrete and inferentially sound knowledge of event effects on political talk requires, first, giving sound theoretical foundations to the expectation that events affect this behavior, and second, developing a comparative research design capable of robustly examining them empirically. In the next section, I attempt to perform the first task by putting into dialogue insights from event-based research and life-cycle studies. I assess the second in the one that follows.

## HOW CAN EVENTS PERSISTENTLY AFFECT ENGAGEMENT IN POLITICAL TALK?

This section discusses and gives a theoretical foundation and discusses the shape that event effects on political talk take across all living cohorts when an event occurs. I understand political talk as a interactive process in which people share political opinions and information based on whether they have (1) interest in doing so (Berger and Luckmann 1966); (2) something in common to talk about (Bearman and Parigi 2004); and (3) someone with a similar political position to talk with (Finifter 1974; Noelle-Neumann 1994; Mutz 2002; Baldassarri and Bearman 2007).

Despite the importance that politics plays in the allocation of social, economic, and cultural resources in a society, conversations about politics are infrequent in regular times. This is because the way social reality is naturalized in everyday experiences obscures the recognition of its political quality (Berger and Luckmann 1966; Bourdieu 1972) and because there are few symbolic, material, or emotional rewards associated with talking about it (Eliasoph 1998). These factors tend to depress interest in starting a political conversation.

These conditions, however, change when a "historical event" occurs. Drawing from theoretical discussions from evenemential research on how these historical phenomena should be conceptualized, I understand this historical phenomenon as a political contingency that is *emergent,* manifesting itself abruptly and unexpectedly over a relatively short time span; *dissonant,* triggering reactions that break with normal understandings about the way politics or the everyday unfolds; and *widespread* in its ability to be collectively experienced as a political rupture (Sewell 1996; Wagner Pacifici 2010, 2017).[2]

[2] The third component of this definition seeks to capture the disruptive quality that Sewell emphasizes as a fundamental feature of events. Departing from his original argumentation, the

As major dissonant and emergent occurrences, events increase political attention as they challenge the schemas that people normally use to make sense of politics. As ruptures that are experienced and collectively recognized as such, they also synchronize political attention around themselves. This facilitates their use as a common political subject to discuss between constituencies and individuals that would otherwise not have political thing in common to talk about. Therefore, when an event occurs, as a result of the synchronized attention it receives, political talk increases.

The magnitude of this increase is, however, modulated by the life stage at which people experience the event. This modulation comes as a consequence of life-span differences in political interest, in the size and frequency of use of political interaction networks, and in people's capacity for "political recognition" —that is, the degree to which a person is able to identify politics as an institutionalized and personally accessible realm of practical experience.

From the moment of birth until adolescence, the capacity of an event to generate effects on political talk is mainly determined by how much individuals are capable of political recognition. In infancy, the absence of this capacity precludes events from having any meaningful political consequence (Rochat 2001). During childhood, political recognition starts to develop. Children acquire seminal pieces of political information at school and are occasionally exposed to parental political discussions in the household (Greenstein 1965). However, at the same time, their immersion in politically-averse processes of "child" identity formation prevents them from using these political experiences as a springboard for developing a practical, conscious, and purposive understanding of politics as an accessible social domain (Searing, Schwartz, and Lind 1973; Zelizer 1985). In this backdrop, the

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definition I propose makes the prospective influence of an event an empirical matter rather than an given.

political influence of events on a child is restricted, at best, to acquiring pieces of environmental political information for the formation of political opinions (Arterton 1975).

In adolescence, political recognition reaches a point of maturity. People formally learn "citizenship capacities" in the school (Hart and Atkins 2011). Baseline political attitudes and levels of political interest also develop and start becoming stable (Sears and Brown 2013). In this period, networks of intentional political discussion begin to form as adolescents start to be included political conversations that take place inside the household (Jennings and Niemi 1974). All of these conditions facilitate recognizing an event as a political contingency, which leads to increases in political information and interest (Sears and Valentino 1997), and ultimately, to larger increases in political talk.

Net of other factors, relative to adolescence, political interest also increases during young adulthood, as people become formally enfranchised into the formal political system (Kahn and Antonucci 1980; Zeglovits and Zandonella 2013) and transition into adult identities. This growth in political interest will be channeled to increase environmental political surveillance (as opposed, for example, to interest in the acquisition of textbook information about politics), which will lead to the acquisition of new knowledge from it (Jennings 1996). Due to young adults' comparatively slight practical political experience (Jennings and Niemi 1981:113; Dinas 2013), this newly acquired environmental information will also tend to be more frequently accessed and leveraged for the formation of political opinions. Because of this, an event experienced during young adulthood will receive a particularly high level of attention, and will also be more influential as an element informing political cognitive processes (Schuman, Akiyama and Knäuper 1998).

As a result, the experiential saliency that a historical event acquires because of these processes gives young adults a political subject matter that is both widely known and readily

available to talk about. This will make it easier for young adults to detect the political preferences of their interaction partners, and facilitate the development of sustained political interaction partners (Huckfeldt, Sprague and Levine 2000). In addition, the expansion of the political networks that comes out of these processes for young adults will be further amplified because personal interaction networks at this life stage begin to transition from high school friendships and parental relationships to fresh ties that are less politically averse, like those found in their workplaces or higher education institutions (Bidart 2005).

As a result of the combined action of the attitudinal, cognitive, and structural processes outlined in the paragraphs above, young adulthood is the life stage at which an event is most able to generate large gains in resources for political talk.

After young adulthood, levels of political interest plateau, and foundational political attitudes experience few alterations (Converse 1969; Prior 2010; Rekker et al. 2019). In addition, as adults begin to accumulate political experience (Huckfeldt, Johnson and Sprague 2004) and undergo life events associated with a contraction of public activities (i.e. marriage and parenting [Kalmijn 2003]), the level of attention they give to the political environment—and the degree to which they give this attention precedence to interpret their political environment—starts to decrease (Jennings and Niemi 1981:129; Cohen, Conway and Maylor 1994). Over time, their core discussion networks also begin to erode (2013 Wrzus et al; Marsden 2018). Under these conditions, event-driven increases in political talk will tend to be smaller than those related to young adulthood. Increases will continue to shrink during late adulthood, when further reductions in the size of interaction networks start to become accompanied by steady losses in political cognition and political sensitivity (Lau and Redlawsk 2006).

These attitudinal, structural, and cognitive dynamics indicate that the magnitude of the changes in political talk that an event provokes will be zero or near-zero during infancy and childhood, increase more rapidly during adolescence, plateau during young adulthood, and then gradually decrease until going back to reach near-zero levels throughout seniority. In other words, in the short-run event effects on political talk are filtered by age differentials in historical sensibility that make their magnitude follow an asymmetrical bell shape across age that peaks in young adulthood.

Over time, this age-curve of short-term event effects will morph into a cohort-curve of event effects in the long run by a number of preservation and reproduction processes.

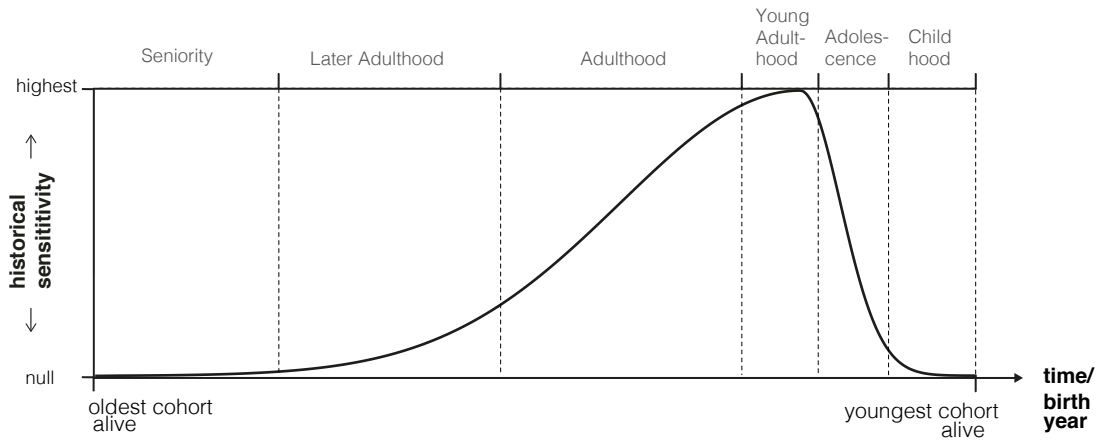
People who were young when an event occurred will have greater resources to engage in political talk later on. This is because the core interaction networks that are formed at this life stage—which for them will tend to be more politically loaded as a consequence of the event's occurrence—tend to persist. Cognitively, the bias towards young adulthood memories that exists in memory retrieval (Schuman and Rogers 2004; Conway 1997) also means that people who came of age during an event will have more resources to engage in political conversation out of these memories. Finally, due to age homophily (McPherson, Smith-Lovin and Cook 2001), this population group will be more likely to interact with people for whom the event had similar political memory, identity, and interest effects. This will stimulate political conversation with established interaction partners, and provide new opportunities to do so with new contacts of the same age.

Viewed together, the arguments above allow mapping the shape of the lasting effects that an event has on political talk across all the cohorts that experienced it, and not just on those who were coming of age.

In a nutshell, the cohort outlook to event effects that I present posits that the prospective influence of a major historical contingency on political talk comes from life-cycle fluctuations in historical sensitivity that generate initial age differences in the strength with which this occurrence affects political talk, and processes of preservation and reproduction of these differences that transform them into long-term cohort divergences over time.

In the long run, the sum of these processes describes a curve of event effects across cohorts that peaks for those who experienced a major political contingency when they were young adults, and decreases until approaching zero for cohorts that experienced it at younger and older ages. The shape of this curve is shown in Figure 1.

**FIGURE 1**
Curve of event effects across cohorts



## RESEARCH DESIGN

### *Hypotheses*

I use the curve of event effects to comparatively evaluate the following propositions:

> *Hypothesis 1: existence of event effects.* Historical events can generate persistent cohort differences in the frequency with which people talk about politics.
>
> *Hypothesis 2*: *widespread event effects.* Times when a historical event occurred generate persistent cohort differences in how frequently people talk about politics.
>
> *Hypothesis 3: positive event effects.* Eventful times produce cohort effects in political talk in a way that increases this behavior the closer a cohort was to young adulthood when an event occurred.

### *Data*

To test these hypotheses, I used the Mannheim Eurobarometer Trend Dataset, a repository of survey items that have been included at least five times in Eurobarometer surveys, which collect representative information from people aged 15 and up in countries that were members of the European Union when they were conducted, from the summer of 1970 to the spring of 2002. One of the variables included in this database refers to a survey question that I refer to as "political talk": "When you get together with friends, would you say you discuss political matters frequently, occasionally or never?" For Belgium, France, West Germany, Italy and the Netherlands—the founding members of the European Union—the Mannheim Dataset contains *political talk* responses conducted at sixty-eight

different survey points between 1973 and 2002.[3] This is a time span and a number of data points that are at minimum comparable to the ones used by recent work that has investigated persistent historical effects on individual-level political attributes (Caren, Ghoshal and Ribas 2011; Ghitza and Gelman 2014).

My analysis examined *political talk* responses for people born as early as 1900 and as late as 1985 (the latter denoting the youngest cohort included in the data) in the five countries I analyze.[4] Since events are contextual disruptions whose "shock" capacity is related to country-specific political contexts, I tested event effects on political talk by conducting country-specific analyses. This strategy also allowed me to control for country-specific idiosyncrasies in political communicative practices.

A number of methodological articles have warned against using cross sectional data to research time-related social dynamics given that such data are prone to exhibit inferential problems related to the age-period-cohort (APC) identification problem: as these three temporal concepts are inextricably co-constitutive and hence perfectly collinear, estimating the separate "effects" of each is unreliable (Mason 1973). This problem is premised on the expectation that (1) age, period and cohort effects are autonomous from one another, that is, causally independent; (2) their effects are monotonous, that is, always showing a consistent direction; and (3) their influence is linear, that is, constant across their values.

[3] Luxembourg is also a founding member of the European Union, but lack of access to high-quality surveys of its twentieth-century history precluded including it in the analysis.

[4] Responses from cohorts born before 1900 were excluded since they were sampled at a life stage when being alive is closely associated with education and income, thus violating missing-at-random assumptions. (See Appendix 1 for the distribution of responses across cohorts.)

For the purposes of this research, assuming linearity of age effects contradicts standard findings about non-linear relationships between age and political engagement[5]. Additionally, if events are indeed capable of generating cohort variations in political talk, the expected association between political discussion and the variables intended to capture these variations cannot be linear either because the temporal location of the events under analysis does not follow a progressive or regressive trend (see Figure 2 in the next section). For these reasons, the APC identity problem is not operative for this research. Thus, rather than seeking to address this challenge by simultaneously including age, period and cohort "effects", my analysis included variables related to these temporal attributes only if their inclusion was justified by theoretical considerations or stable empirical findings.

*Set of Events under Analysis*

Conducting comparative research on event effects first requires defining a concrete set of events to test their persisting influence. To identify this set, I conducted a review of major surveys on twentieth-century Western European history. For each country, I revised at least two major national history monographs. This historiographical survey allowed me to identify a group of historical occurrences that by being described as abrupt turning points in the political history of a country, or as contingencies that that provoked comprehensive, strong, and temporally concentrated changes in political conditions, approximated the notion of "historical event" advanced by historical sociological research. I then examined specialized literature—and when possible, hemerographic material—on each of these

[5] Linear inclusion of age also decreased model fit in comparison to its quadratic inclusion. Additionally, from a broader epistemological standpoint, presentist social research is actually implicitly predicated on the premise that a period "affects" different people in different ways.

occurrences to identify the year(s) when they peaked as political contingencies[6], and identify characteristics related to their duration, the effects they had in the field of politics, and the type and number of the sequences of political disruptions they were associated with.[7]

This analysis led to identifying 51 years featuring empirical operationalizations of "historical events" between 1918, when the first generation under analysis came of age, and April 2002, when the last Eurobarometer survey in the database was conducted. Figure 2, shows each of these years, and the name and a succinct description of the operationalizations of "historical events" they contain. The event set includes contingencies triggered by elections (for example, the 1948 General Election in Italy [Novelli 2008]), sudden changes in state structures (e.g., the establishment of the French Fifth Republic [Rémond 2008]), terrorist acts (e.g., the bombing of the Bologna train station in 1980 [Oliva 2019]), corruption and state malpractice scandals (e.g., the Agusta Affair in Belgium [Barez 1998]), large-scale waves of social mobilization (e.g. the May '68 protests in France [Bavard 2008]), periods of widespread social violence (e.g., the *Biennio Rosso* in Italy [Fabbri 2009]), and contingencies with diverse origins, like the multiple political emergencies unleashed by Germany's 1923 reparation crises (Maier 1975). As emergent political processes, the occurrences in this event set are substantively diverse and unique in the way they came about. However, all share the quality of being abrupt, large-scale political ruptures relative to the time and place where they occurred.

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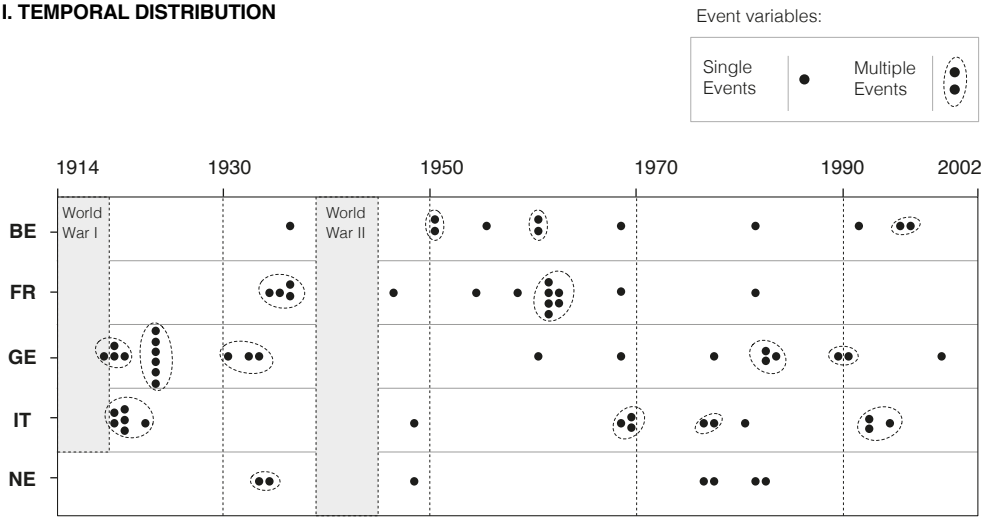
[6] There were several events whose contingency period spanned multiple years. In these cases, the peak year was assigned to the one including most of this period. The peak year of the 1991 General Election in Belgium, for example, was assigned to 1992 because it was held in December and most of its political effects took place in the first quarter of this year. Peace protests in Belgium exhibit bimodal contingency peaks: one in 1981 and another 1983. For parsimony, I codified 1982 as their peak year.

[7] Due to the relative scarcity of historical work on more recent events, my review of these occurrences made intensive use of reference newspapers and weekly magazines. They included *Der Spiegel* for West Germany*; Le Soir* for Belgium*; L'Espresso* for Italy*;* and *Le Nouvel Observateur* for France. Event-specific bibliographies are available upon request.

**FIGURE 2**
Events Under Analysis

**I. TEMPORAL DISTRIBUTION**




**II. DESCRIPTION OF VARIABLES**

| Var Nbr. | Peak Year | Description |
|---|---|---|
| **Belgium** (Main Sources: Arblaster 2005; Judt 2005; Witte et al 2009.) | | |
| **1** | 1936 | – 1936 General Election (Electoral Realignment). |
| **2** | 1950 | – Leopold III Abdication & Julien Lahaut Murder (State Crisis; Strikes; Ethnic Strife, Political Murder). |
| **3** | 1955 | – Collard Law Protests (Social Mobilization). |
| **4** | 1960 | – Congo Intervention (Decolonization, Foreign Intervention). |
| | 1960 | – Unitary Law Strikes (Labor Strikes; Governement Crisis). |
| **5** | 1968 | – Leuven University Split (Social Mobilizations; Ethnic Strife; Government Crisis, Institutional Change). |
| **6** | 1982 | – Neoliberal Turn, Anti-nuclear missile mobilizations[1] (Social Protests, Accountability Crisis, Gov't Change). |
| **7** | 1991 | – Black Sunday (Electoral Realignment; Government Crisis). |
| **8** | 1995 | – Agusta /Dassault Affairs (Corruption Trials, Cabinet Crisis). |
| | 1996 | – Dutroux Affair / White March (Law and Order Scandal, Social Mobilizations, Accountability Crisis). |
| **France** (Main Sources: Shorter & Tilly 1974; Maier 1975; McMillan 2003 (ed.); Judt 2005; Conklin et al. 2010.) | | |
| **1** | 1934 | – Chiappe Riots (Social Mobilizations, Repression; Government Crisis & Ideol. Realignment). |
| | 1935 | – Unitary March (Social Mobilizations, Institutional Change). |
| | 1936 | – 1936 Elections, Strikes (Ideological Realignment; Labor Strikes). |
| **2** | 1947 | – Communist Ministers Ousting (Cabinet Crisis; Labor Strikes; Terrorist Acts). |
| **3** | 1954 | – Dien Bien Phu / Indochina Retreat (Decolonization; Military Defeat). |
| **4** | 1958 | – First Algiers Putsch & 5th Republic Establishment (State & Gov't Crisis, State Reconfiguration). |
| **5** | 1961 | – Algerian Retreat Crises[2] (State Crisis; State-sponsored Repression; Terrorism). |
| | 1962 | – Evian Accords, De Gaulle Murder Attempt (Decolonization;Terrorism; Political Murder Attempt). |
| **6** | 1968 | – Spring Protests, (Student Protests; Government Crisis). |
| **7** | 1981 | – 1981 General Election (Electoral Realignment; Government Ideological Realignment). |

Popular Front Events

Late Algerian Crises

[1] Bimodal peaks in 1981 and 1983; peak centered in 1982.
[2] Algerian Independence Referendum; First Wave of OAS Terrorism; Second Algiers Putsch; October 17th massacre.

**Figure 2 (cont.)**
Events Under Analysis

| Var Nbr. | Peak Year | Description | |
|---|---|---|---|
| **West Germany** (Main Sources: Maier 1975; Tipton 2003; Judt 2005; Kitchen 2012.) | | | |
| **1** | 1918-'19 | – German Revolution (State Crisis; State Reconfiguration; State & Social Violence). | German Rev. Crises |
| | 1920 | – Kapp Putsch and Strikes, Ruhr Uprising (State Crisis; Strikes; State & Social Violence). | |
| **2** | 1923 | – War Reparation Crises[3] (Occupation; State and Gov't Crisis; Social Violence; Strikes). | |
| **3** | 1930 | – Federal Election Results (Electoral Realignment; Government Idelogical Realignment). | Weimar Downfall Events |
| | 1932 | – Von Papen Gov't and Federal Elections (Electoral & State Idol. Realignment; Violence ). | |
| **4** | 1961 | – Berlin Crisis (Military Standoff). | |
| | 1962 | – *Der Spiegel* Affair (Civil Liberties Scandal; Government Crisis). | |
| **5** | 1968 | – Extra-Parliamentary Mobilizations (Social Mobilizations; Repression Acts). | |
| **6** | 1977 | – RAF Autumn Activities (Terrorism). | |
| **7** | 1982 | – 1st Peace March; Flick, *Neue Heimat* Affairs; Helmut Schmidt Downfall ( Scandals; | *Die Wende* Events |
| | 1983 | – Nuclear Action Week (Social Mobilization). Protests, Gov't Crisis). | |
| **8** | 1989-'90 | – Berlin Wall Fall, Unification (State Reconfiguration). | |
| **9** | 1999 | – CDU Donations Scandal (Corruption Scandal). | |
| **Italy** (Main Sources: Maier 1975; Franzosi 1994; Dumage 2002; Clark 2008; Ginsborg 2000, 2007.) | | | |
| **1** | 1919 | – Strikes; Fiume Occupation (Labor Strikes; Gov't Crisis; International Crisis). | Liberal Regime Crises |
| | 1920 | – Biennio Rosso Events[4] (State Failure; State and Gov't Crisis; Strikes; Social Violence). | |
| | 1922 | – Legalitarian Strike, March on Rome (Social Mob. and Violence, State Ideol. Realignment). | |
| **2** | 1948 | – General Election Campaign ( Polarization; State Ideol. Realignment; Strikes; Political Murder Attempt). | |
| **3** | 1968 | – *Sessantotto* Mobilizations (Student and Labor Strikes). | |
| | 1969 | – Autunno Caldo (Student and Labor Strikes, State Repression). | |
| | 1969 | – Piazza Fontana Attack (Terrorism). | |
| **4** | 1977 | – Lockheed Affair (Corruption Scandal, Cabinet Crisis). | |
| | 1978 | – Aldo Moro Murder (Terrorism, State Crisis). | |
| **5** | 1980 | – Bologna Train Station Attack (Terorrism). | |
| **6** | 1992 | – *Mane Pulite*; Falcone/Borselino Murder (Corruption Scandal; Pol. Murders; Gov't Crisis). | Postwar Regime Dissolution |
| | 1993 | – Mafia Terrorism (Corruption Scandal; Poltiical Suicides; Gov't Crisis). | |
| | 1994 | – *Discesa in Campo* and General Election (State Crisis; Institutional Reconfiguration). | |
| **Netherlands** (Main Sources: Judt 2005; Arblaster 2005; Blom and Lamberts 2006.) | | | |
| **1** | 1933 | – *De Zeven Provinciën* Mutiny (Military Rebellion). | |
| | 1934 | – Jordaan Riot (Social Violence). | |
| **2** | 1948 | – '48 Java Police Action (Decolonization; Political Polarization). | |
| **3** | 1976 | – Lockheed Affair (Corruption Scandal). | |
| | 1977 | – Wijster Train Hostage Crisis (Terrorism). | |
| **4** | 1981-'82 | – Antinuclear Protests (Social Mobilizations; Acountability Crisis). | |
| **Wars** (Per country operationalization. Source: Judt 2005.) | | | |
| **a.** | 1914-'18 | – First World War (Belgium, France) | |
| **b.** | 1939-'45 | – Second World War (Belgium, France, Germany, Italy, Netherlands). | |

[3] Alllied Ruhr Occupation; Cuno Strikes; Dissolution of Communist Governments; Hamburg Uprising; Kustrin, Munich Coups.
[4] Ancona Mutiny; Factory Occupations; Palazzo Accursio Massacre; Fascist Violence.
[5]Time range under analysis includes only the last year of First World War. Netherlands excluded due to non-participation.

*Variables*

*Dependent Variable.* The dependent variable is *political talk,* an ordinal variable with three different values: never (0), occasionally (1), and frequently (2).

*Key Independent Variables: Event Sensitivity Variables.* The key independent covariates are country-specific cohort variables of event sensitivity. These covariates capture differential levels of cohort sensitivity for each of the historical occurrences under analysis. To prevent multicollinearity, events whose peak years were contiguous were combined into a single variable.[8] In total, I generated 34 variables of historical sensitivity to events—the "event variables". 8 refer to events that occurred in Belgium, 7 are related to France, 9 to West Germany, 6, to Italy, and 4 to the Netherlands. When referring to a specific event variable, I will use the name of its underlying event(s).

I also generated 7 country-specific variables of historical exposure for the First and Second World Wars.[9] Although not proper events by the definition used here, their importance for twentieth-century European history provided a compelling reason to include them.

Values for each of these variables were assigned according to the birth-year of survey respondents. To preserve causal precedence assumptions, the values of these variables were set to zero if responses came from years before an event's occurrence. In every other case,

[8] As an exception, I also merged the *Biennio Rosso* events of 1919-1920 and the March on Rome of 1922 into a single variable (1919-1922), and the 1930 General Election and the 1932 Political Crises in Germany into another (1930-1932). Although 1921 and 1931 did not feature punctuated political ruptures in these countries, these years did feature high levels of political instability and increasing social violence that connected them with the two concrete events located before and after them.

[9] The Netherlands did not take part in the First World War and was hence spared from its corresponding exposure variable. I did not assign variables of exposure to World War for Germany and Italy either because 1918 was also associated to the German Revolution and the dissolution of the Liberal political system, respectively.

their values followed an asymmetrical bell shape that peaked in the cohort that was 20 years old when an event happened. For simplicity of exposition, I call this birth-year group the "inflection cohort" of such event. More formally, the values of these variables were calculated using the following exponential function:

(1)

$$sensitivity_{i(k),\,j(k)} = \begin{cases} 100 & \text{if } (peak_{j(k)}\text{-}20\text{-}coh_{i(k)}) = 0, \\ 100*\exp[-a*(peak_{j(k)}\text{-}20\text{-}coh_{i(k)})]^2 & \text{if } (peak_{j(k)}\text{-}20\text{-}coh_{i(k)}) < 0, \\ 100*\exp[-6a*(peak_{j(k)}\text{-}20\text{-}coh_{i(k)})]^2 & \text{if } (peak_{j(k)}\text{-}20\text{-}coh_{i(k)}) > 0. \end{cases}$$

where $sensitivity_{i(k),j(k)}$ indicates the degree of event sensitivity that individual *i* in country *k* had towards the *j*-th event in such country, $peak_{j(k)}$ is the year at which this event peaked, and *cohort(i)* is the birth year of individual *i*. The *a* coefficient modulates the decaying rate of the bell. Since previous research regarding the rate at which sensitivity to an event decreases across cohorts is lacking, my analysis used four alternative decay speeds as a robustness check: (a=0.004, a=0.006, a=0.008, and a=0.010).[10]

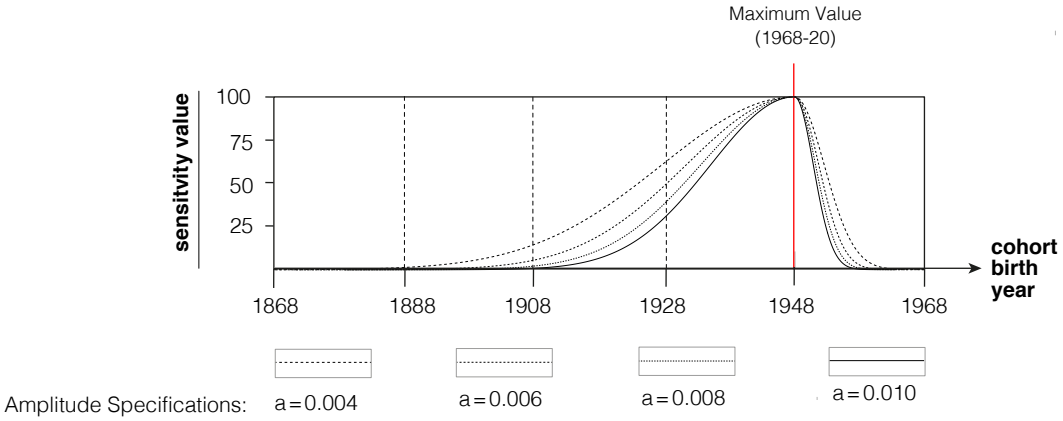
Figure 3 depicts the functional shape of these variables using the 1968 mobilizations in France as an example. The variable peaks at 100 for the cohort that experienced the event at age 20—the event's inflection cohort—. It slowly decreases for contiguous cohorts, then more rapidly, and then again at a slower rate the farther a birth year group is from the inflection cohort.[11] This functional form captures the basic theoretical logic of the curve of

[10] The age range in the top decile of the values for each of these curves is 8.25 (a rough approximation of the 7-year range Mannheim originally proposed as the temporal width of a generation), 9.25, 10.75 and 13.25 years, approximately.

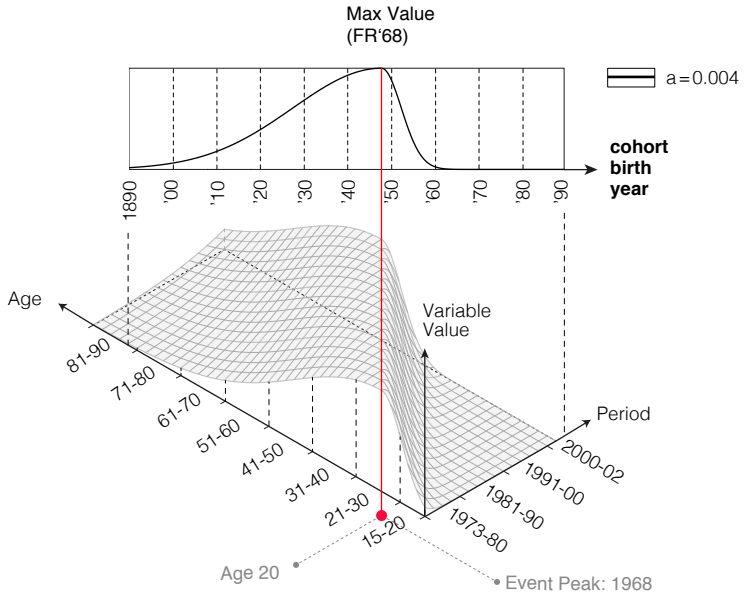
[11] The choice to center the bell of these variables on cohort aged 20 when an event occurred was made because this age starts to exhibit minimal maturation in adulthood maturation processes while still being close to the transition from adolescence to young adulthood.

**FIGURE 3**
Historical Sensitivity Variable
Example: Spring Protests, France (1968)

**I. FUNCTIONAL FORM AND OPERATIONALIZATIONS OF BELL AMPLITUDES**




**II. VALUES OF HISTORICAL SENSITIVITY ACROSS TIME/AGE**



event sensitivity within a relatively simple shape: values from cohorts that experienced an event during young adulthood are highest and have relatively few differences in their magnitude, and then decrease until asymptotically reaching zero for both the oldest and youngest living cohort when an event occurred, and for people not yet born at that time.

*Cohort Controls.* Besides exposure to historical events, other cohort features such as cohort size and level of educational attainment may also affect patterns of political discussion (Ryder 1965:845–846). As a check for cohort-level omitted variable bias, these variables were included as controls through linear or quadratic specifications.

I operationalized *cohort size* as the number of individuals (in thousands) aged 15–20 when the cohort was aged 23;[12] as a measure of *cohort educational attainment,* I used the proportion of university graduates or students among people aged 15 to 25 when a cohort was 20 years old. Values for these variables were obtained using statistical archives, which allowed producing data-strings from 80 different cohorts in five countries in a minimally consistent fashion (see Appendix 2). As an additional check, I also included dummies indicating a respondent's adscription to a particular 5-year *cohort category*.

*Period-Level Controls.* To test the impact of *elections* on political talk (Sears and Valentino 1997), the analysis included dummy variables indicating if a general election had taken place in a year. Yearly GDP per capita growth rates were also included to test the strength of association between political discussion and *economic performance.*

[12] It is likely that *cohort size* and *cohort education* exhibit measurement errors. However, they are expected to be randomly distributed for *cohort size,* and to yield conservative estimates for *cohort education* (see Appendix 1).

*Individual-Level Controls.* As individual-level controls, I used a vector of variables measuring *age*, *income, education*,[13] *male* self-identification and *marital status*. These variables have previously been identified as important predictors for political talk (Marsden 1987; Moore 1990; Huckfeldt and Sprague 1995; Bearman and Parigi 2004). Following standard findings from life-cycle political research, age was included in a quadratic form. I also included *urban* and *metropolitan* residence indicators[14], and dummies indicating *regional location*. For each country, the reference category was its most developed region. Except for *income,* for which a non-response dummy variable was created, no variable exhibited evidence of violating missingness-at-random assumptions.

*Meta-analysis: regression set, and key statistics.*

The absence of prior quantitative comparative analyses testing cohort-level event effects makes my research sensitive to reliability challenges related to model selection uncertainty (Young 2009). For this reason, instead of focusing on a single "best-fit" model, my findings are based on the meta-analysis of an "event regression set" per country composed of 72 different models per country. Each regression in this set included a constant vector of period, event exposure, and individual-level controls, and a specific combination of alternative specifications for *cohort educational attainment* (absence, linear, or quadratic specifications), *cohort size* (absence, linear, or quadratic specification), *cohort category* dummies

[13] For *education,* heterogeneous coding procedures across Eurobarometer waves were collapsed into 5 categories that ranged from not having finished junior high school (1) to having completed college studies (4), plus a fifth indicating continuing education. *Income* was included using a series of five dummy variables associated with monotonically increasing income brackets, measured in 2002 real value local currencies, and a sixth indicating non-response.

[14] The metropolitan variable indicates inhabitants of cities with a core center exceeding a million inhabitants: Paris, West Berlin, Rome and Amsterdam. No metropolitan dummy was included for Belgium.

(presence or absence), and *event exposure* variables (a=0.0025, a=0-.0050, a=0.0075, a=0.0100). Following recent methodological recommendations, these regressions used OLS specifications. These models tend to yield similar results from alternative ordered logistic models, but they allow a clearer substantive comparison between coefficients across clusters of observations (Allison 1999; Mood 2010. As a robustness check, I conducted analysis using ordered logistic models. Their substantive results are similar to the ones I report here. Results are available upon request).[15] To control for error clustering across cohorts, estimated coefficients were calculated using robust-clustered standard errors.[16]

I analyzed results from this meta-analysis using two types of statistics. Magnitude statistics included average values and standard deviations from the estimated coefficients of independent variables across the regression set. Robustness statistics, on the other hand, calculated the systematicity at which a variable attained levels of significance across the meta-analytic set. I use two of them. The first of these indicators was the *rate of significance* (RS). Its value ranges from 0 to 1. It indicates the proportion of regressions for which a variable was significant at standard levels of confidence. The second is the *robust significance statistic* (RSS), an original indicator I adapted from Charles Ragin's discussions on causal sufficiency tests (2000). It uses RS values to calculate the probability that a variable will be significant beyond a given percentage benchmark using the following z-value test:

[15] Applying OLS models to categorical variables may sometimes yield estimated coefficients devoid of substantive meaning. For this research, however, a straightforward interpretation can be given to estimated values below the minimum value (avoid talking politics, for example) and above the maximum (very frequently).

[16] Numerical values for these categories were readjusted for interpretive clarity. Total responses number 70,577, from which only 393 (0.5%) were missing values. Although the question is restricted to engagement in political talk with friendships, some research indicates most political talk occurs between friends (Huckfeldt and Sprague 1995).

$$\tilde{\tau} = \frac{(RS\text{-}p)\text{-}(1/2n)}{[(p(1\text{-}p))/n]^{1/2}} \quad , \qquad (2)$$

where *RS* refers to the rate of significance, *n* refers to the number of models in the regression set, and *p* refers to the benchmark rate of significance, which, following suggestions from the literature (Ragin 2000), was set at $p = 0.75$[17] . Its value ranges from 0 to 1, and its logic of interpretation is the same as a standard *p*-value. For clarity of exposition, I refer to variables with RSS values below 0.1 as robust predictors of political talk.

## RESULTS

### *Descriptive Trends*

As a preliminary step towards presenting the meta-analytic results, Table 1 shows, for each country, cross-cohort descriptive statistics for *political talk*. Cohorts from Belgium exhibit the lowest mean value *of political talk* (0.569). Those from West Germany exhibit the highest mean value (0.906), just below the "sometimes talking about politics" threshold (*political talk* = 1). Consistent with previous findings (Bearman and Parigi 2004), these figures show that political discussion is a relatively rare political behavior.

[17] In this meta-analysis, the total number of regressions was 72. *Cohort dummies* were included in 36 models, and *cohort size* and *cohort educational attainment* in their linear and quadratic specifications in 24 each. Since this number is smaller than 30, tests of systematic significance for *cohort size* and *cohort educational attainment* were conducted using a negative binomial regression.

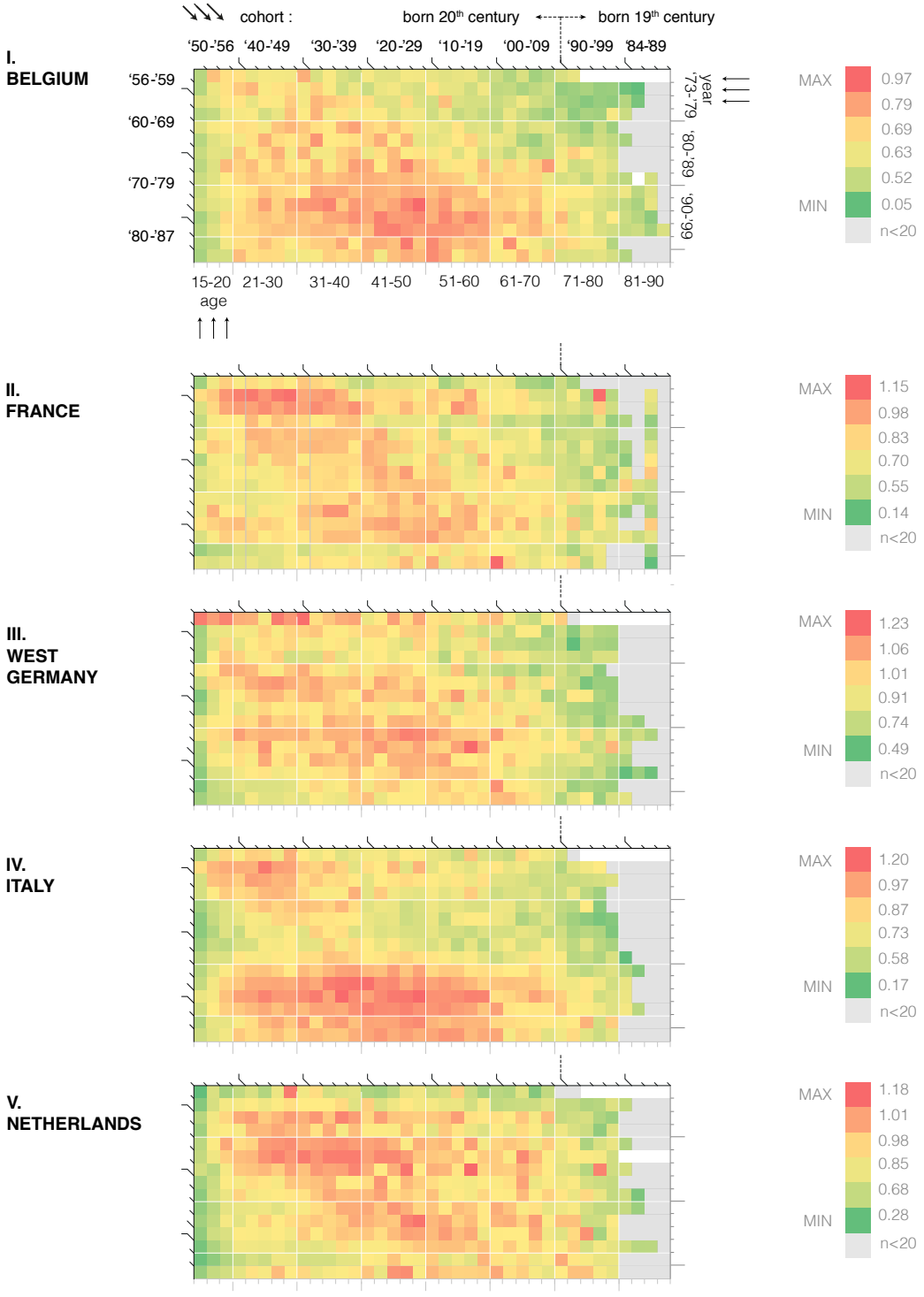
**TABLE 1**
*Political Discussion*: Cohort descriptive statistics per country[1,2]

| | (1) Mean | (2) Standard Deviation | (3) Min. Value | (4) Max. Value |
|---|---|---|---|---|
| Belgium | .569 | (.167) | .120 | .794 |
| France | .748 | (.133) | .350 | .951 |
| West Germany | .906 | (.129) | .444 | 1.063 |
| Italy | .772 | (.129) | .454 | .994 |
| Netherlands | .842 | (.147) | .147 | 1.040 |

[1]Source: Mannheim Eurobarometer Trend Dataset, 1973-2002.
[2]Values calculated from cohorts with 20 or more observations in the data set.

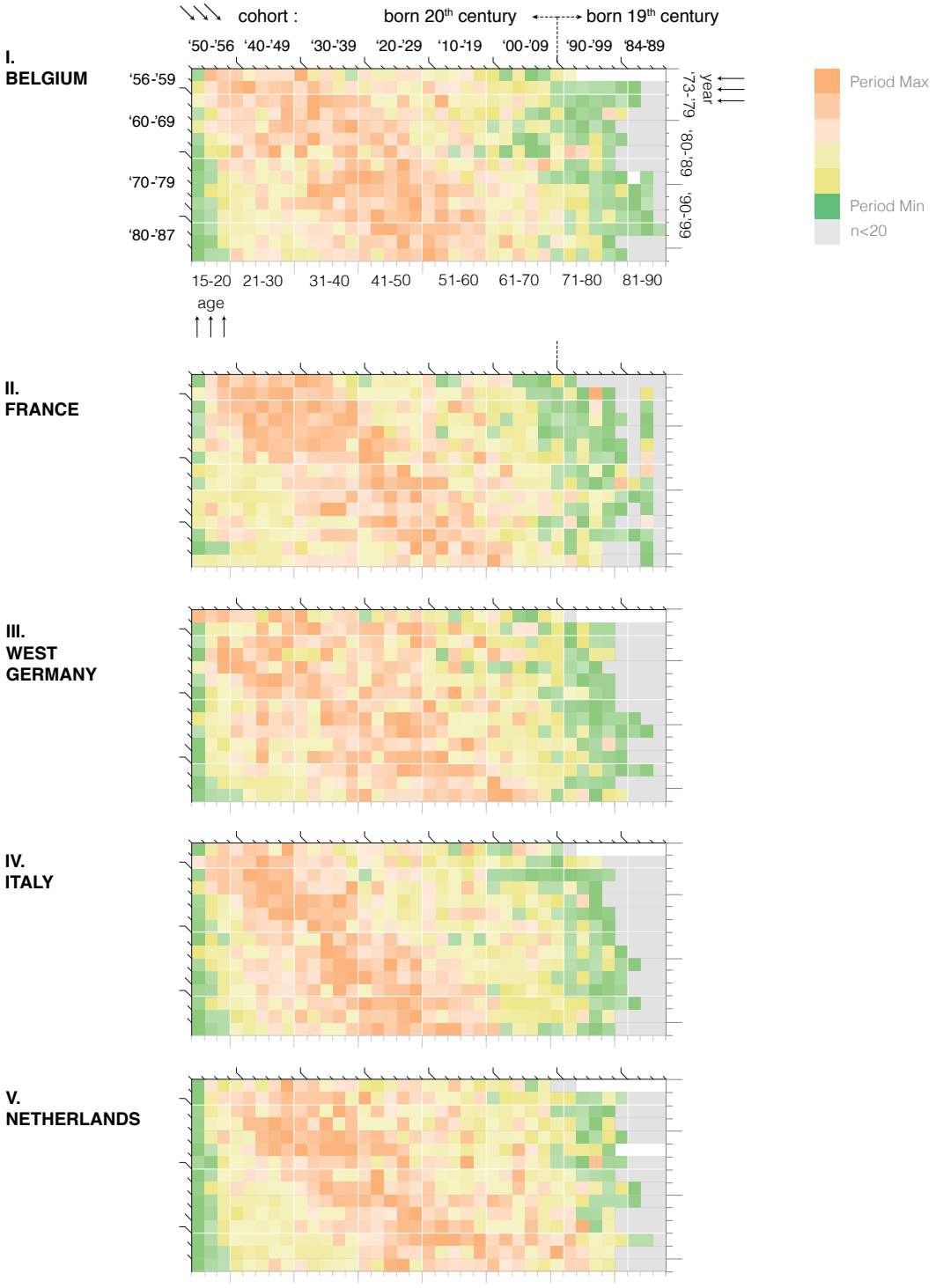
Subsequently, to track the temporal dynamics of political communication across cohort and age, Figures 4A and 4B depict mean values of *political talk* across 2-year age/period cells. Cells from the same biennium are distributed along a single row, and cells related to a particular age are located in a specific column; consequently, cells associated to a specific cohort are distributed across diagonals. In Figure 4A, cells are colored according to their mean *political talk* value relative to all others. Cells with higher means are colored with stronger shades of red, while those with lower values exhibit a darker green; cells with a small sample of respondents (n<20) are shown in gray. In Figure 4B, cells are similarly colored according to their within talkativeness relative to cells from the same biennium.

**FIGURE 4A**.
*Political Discussion* Values across Time[1]
Color Histogram, 1973-2002




[1]Source: Mannheim Eurobarometer Trend data set, 1973-2002.

**FIGURE 4B**.
*Political Discussion* Values across Time[1]
Color Histogram per 2-year period, 1973-2002




[1]Source: Mannheim Eurobarometer Trend Dataset

Figure 4A shows that *political talk* trends across time are sensitive to historical environmental conditions. For instance, engagement in this behavior transitioned in Italy from moderate levels in the 1980s to maximum values in the early 1990s, when Italy's political system was being shaken by a massive corruption scandal, mafia-linked terrorism, and the collapse of its postwar party system (Gundle and Parker 1996).

Figure 4B, on the other hand, presents supportive evidence that net of historical contexts, political talk is also patterned by birth year. The diagonal organization that cells with maximum values of *political talk* in a biennium follows is indicative of a notable stability in the distribution of relative rankings of political talk across cohorts. This trend suggests that cohorts that underwent a "talk bump" when they entered young adulthood continue talking about politics more frequently in subsequent periods, after controlling for historical contexts.[18] It also suggests that cohorts that experienced a major political contingency during young adulthood rank among the most talkative cohorts across time later on. For example, people that came of age in the Netherlands during the mid seventies, when the country was stricken by terrorist attacks and a corruption scandal (Bootsma 2015, Kroeze 2017), became relatively frequent political talkers and continued being so later on. By contrast, cohorts that entered young adulthood in the second half of the eighties—when times were historically subdued in the Netherlands—were infrequent political talkers at that time and continued being so later on.

[18] Uniform age patterns in relative values of *political talk* across periods are limited to adolescence and the oldest seniors. People located in these life periods are the ones that talk the least about politics.

These trends suggest the existence of cohort trends in *political talk* attributable to the occurrence of an event. The rest of the section presents the results of the meta-analysis I conducted to formally test these influences.

*Formal Analysis*

*Goodness of fit.* For each country, Table 2 compares R-squared values between baseline models regressing *political talk* only on individual- and period-level controls, and the average and maximum values of this fit statistic for the set of regressions that included events as independent variables. Goodness-of-fit statistics exhibit satisfactory values in the baseline models, and further increase when variables of event exposure are added as regressors. Adjusted R-squared values are the highest in Italy, where the best-fit model explains 14% of total variance, and the lowest in the Netherlands, where the best-fit model explains 7.4% of the variance.

**TABLE 2**
Adjusted R-Squared Statistics, by Country

| | Baseline Model[1] | Event Regression Sets | | | |
|---|---|---|---|---|---|
| | (1) | (2) Average | (3) Std.Dev. | (4) Min. | (5) Max |
| Belgium | .109 | .114 | (.0018) | .110 | .116 |
| France | .100 | .106 | (.0015) | .102 | .107 |
| West Germany | .088 | .091 | (.0008) | .089 | .092 |
| Italy | .136 | .140 | (.0010) | .138 | .141 |
| Netherlands | .063 | .072 | (.0033) | .064 | .074 |

[1] Includes only individual and period-level control variables.

*Control Results.* *Education* and *income* brackets, *female* self-identification, *rural* residence, *regional* controls, and *electoral* years are robust predictors of *political talk*; their direction of influence is consistent with standard findings from the relevant literature. (Full control results are reported in Appendixes C and D; Figure 6 plots maximum effects of influence for categories of control variables that were robustly significant). *Education* brackets hold a consistently positive linear relationship with *political talk*. As a set, they are its single most important predictor. *Income* also exhibits a generally positive association with *political talk*, but the rate at which this behavior increases across income brackets tends to be smaller.[19] The association between *political talk* and *electoral years* is also positive, but its magnitude is also small. *Female* self-identification, on the other hand, is negatively associated with political discussion. Residence in *rural* areas and in less developed *regions* are in general also negative predictors of political talk, but the strength of their association with it is marginal.[20]

*Age, marital status* and *GDP growth* are robust predictors of *political talk* (with exceptions in West Germany for *age*, in Belgium for *marital status*, and in Italy and the Netherlands for *GDP growth*). Age follows an inverted-u relationship with inflection points in either adulthood or senior years. *GDP growth* has a consistently positive (but marginal) relationship with *political talk* in Belgium and France, and a negative one in West Germany. The influence of *marriage* on *political talk* also varies. Its effect is not significant in Belgium, negative in Italy and France, and positive in West Germany and the Netherlands. The relationship between *economic performance* and *political talk*, on the other hand, is marginally positive in Belgium and France, not significant in Italy, and marginally negative in Germany

[19] In Belgium, the average estimated coefficient for the highest income bracket is smaller than the one for middle-income brackets. In West Germany and the Netherlands, on the other hand, middle-income brackets talk less about politics than the lowest income bracket.

[20] Italy is an exception: living in a rural or a relatively poor region is positively related to *political talk*.

and the Netherlands. Among cohort-level controls, *cohort education* is robust, and surprisingly, a negative predictor of *political talk* in France, Germany, and the Netherlands. In Italy, its relationship with the dependent variable is quadratic. Other cohort-level controls were not found to be significant predictors of political talk.[21]

*Event Results.*

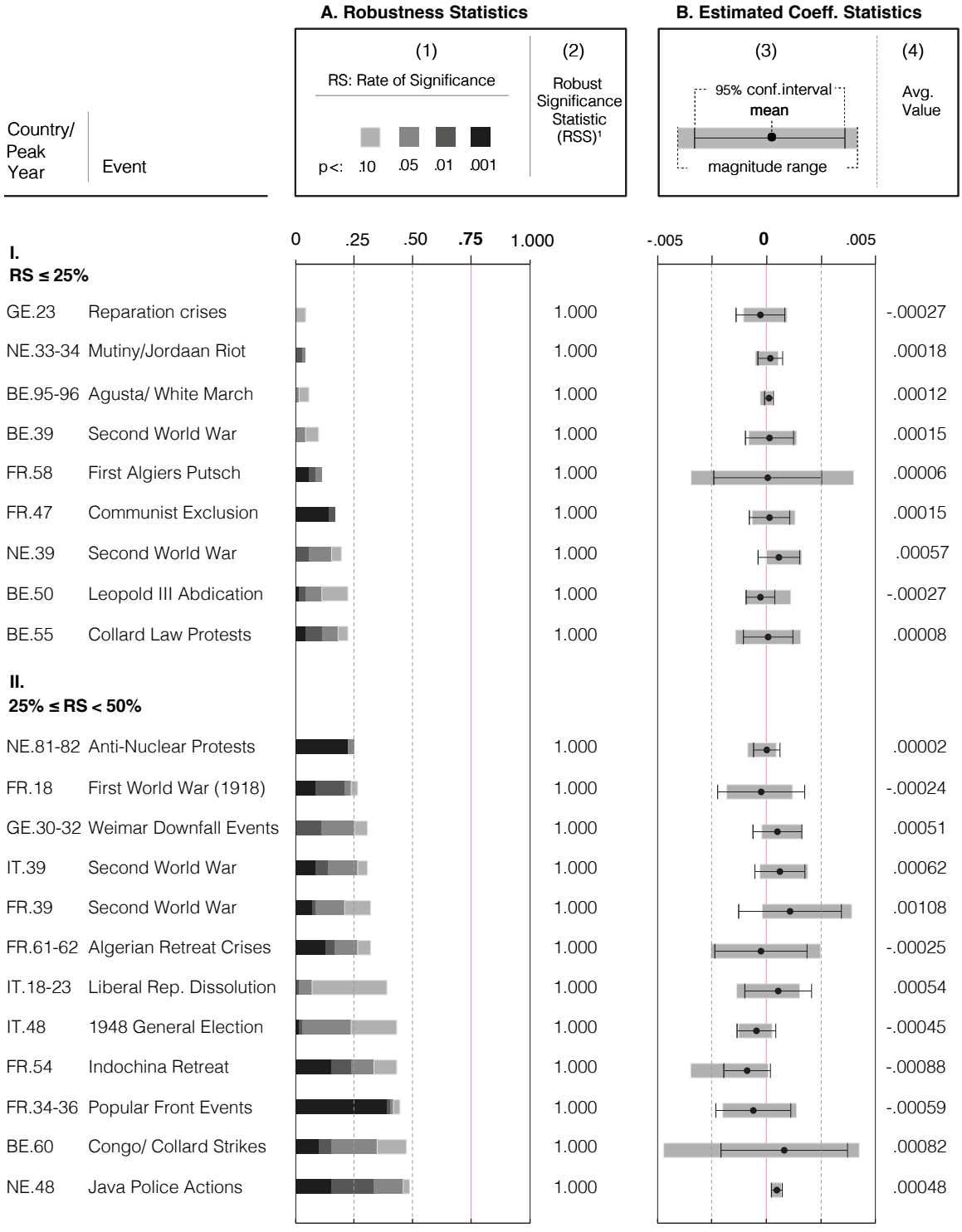
I first discuss results for specific event variables, and then proceed to discuss how the event set as a whole performs in terms of robustness and directionality of influence.

*Robust predictors of political talk.* Figure 5 reports graphically and numerically meta-analytic statistics for each event variable. Panel A plots the rate at which an event attained standard levels of significance across the meta-analytic regression set, and reports values for the Robust Significance Statistic (RSS). Panel B plots average, minimum and maximum, and 95% confidence intervals for the values of estimated coefficients.

The set of nine events shown in the first group of this Figure can be considered robustly non-significant predictors of *political talk*. They attained statistical significance only in 25% or less of the regressions. The event that was least frequently associated with *political talk* was the 1923 German Reparation Crises. It became a significant predictor of this variable only in 3 regressions (RS = .041). Other examples of events that were significant covariates only exceptionally include the political crises and military putsch that led to the establishment of the French Fifth Republic in 1958 (RS=0.11); and the Second World War experiences of Belgium (RS=0.097) and the Netherlands (RS=0.194).

[21] The only consistently significant cohort controls were 1916–1951 cohort brackets in Italy (*cohort categories* results available upon request). Results from cohort-level cohorts should be considered only preliminary, as the low number of regressions in which they were included (24 for *cohort size* and *cohort education*, and 36 for *cohort dummies*) might have lowered robustness indicators.

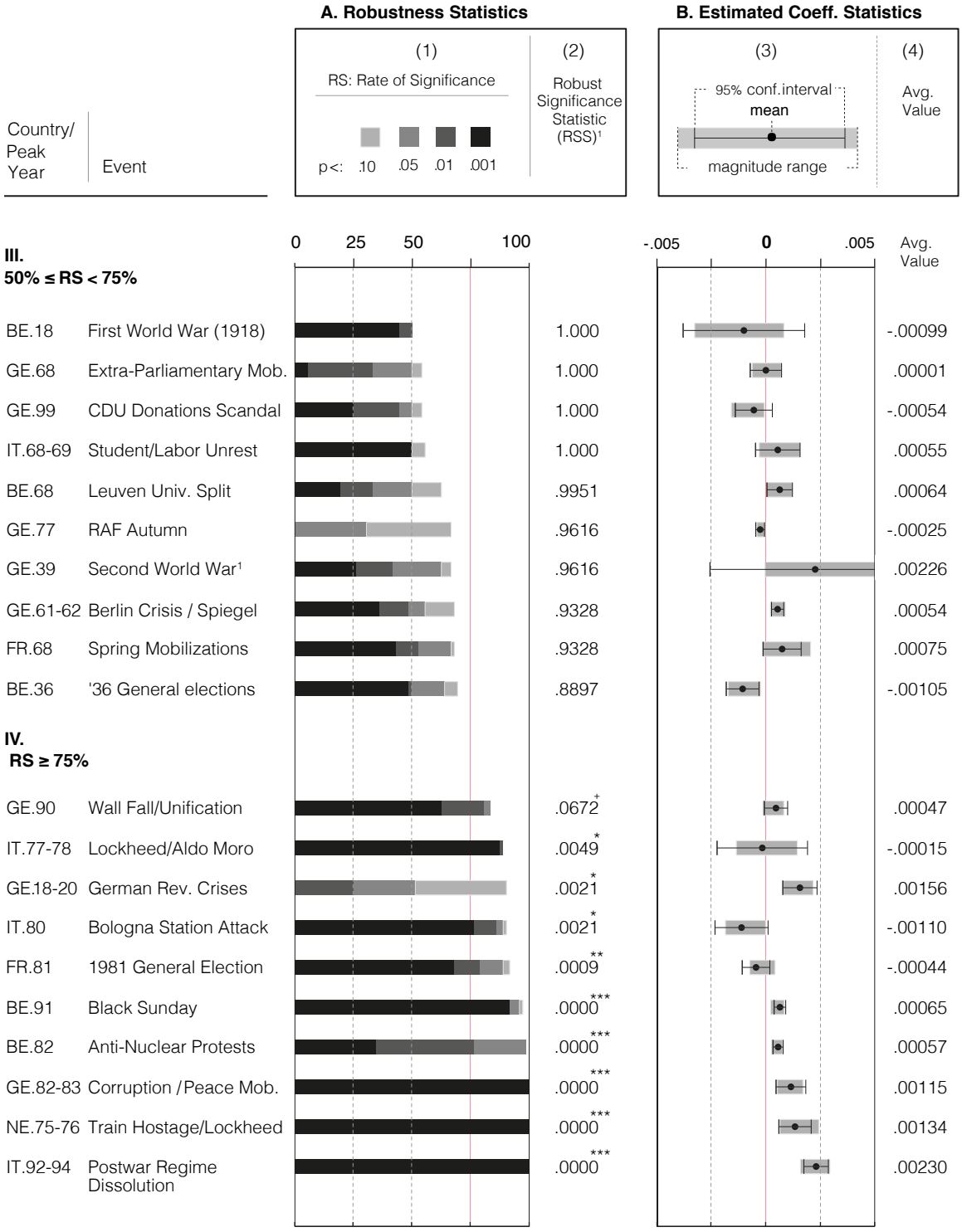
**FIGURE 5**
Results for Event Exposure Variables: Meta-Analytic Statistics of Estimated Coefficients




[1]Estimated coefficient statistics beyond [-0.025, 0.025] range. Full numerical results available upon request.

[2] Indicator of Systematic Significance: Probability that a variable will be significant 75% or more across models:
*Significant at the .1 level; ** Significant at the .05 level; ***Significant at the .01 level; ****Significant at the 0.001 level.

**Figure 5 (cont.)**
Results for Event Exposure Variables: Meta-analytic statistics of Estimated Coefficients




[1]Estimated coefficient statistics beyond [-0.025, 0.025] range. Full numerical results available upon request.

[2] Indicator of Systematic Significance: Probability that a variable will be significant 75% or more across models:
*Significant at the .1 level; ** Significant at the .05 level; ***Significant at the .01 level; ****Significant at the 0.001 level.

The second and third groups of events in Figure 5 can be characterized as unsystematic predictors of *political talk*. Their RS values range between 25% and 75%. Five war variables and seventeen event variables—half of the whole set—fall into this category. These sets include the '68 events from France, Germany, Belgium and Italy, and historical contingencies related to decolonization processes in Congo, Indochina, Algeria, and Indonesia.

Robust predictors of *political talk* are the final group of events shown in Figure 5. They were significant predictors of this variable 75% or more of the times, and hold significant RSS values. This group includes 10 events. Two are from Belgium: the results of the December 1991 snap election, which saw a steep increase in the vote share of the extreme right, and ended the near uninterrupted 11-year old tenure of Wilfried Martens as prime minister; and the anti-nuclear weapons protests and the neoliberal turn in economic policies that took place in the vicinity of 1982.[22] Another one is French: the victory of Socialist François Mitterrand in the French presidential elections of 1981 and the unprecedented parliamentary majority that the left obtained in the legislative elections that year. In West Germany, events that are robust predictors of *political talk* include the Fall of the Berlin Wall and the German Reunification process of 1989-1990; the collection of corruption scandals, political and economic policy turnarounds, and anti-nuclear weapon protests that occurred between 1982 and 1983, a time that was then often referred to as *Die Wende*—the "turnaround—; and finally, the events related to the German Revolution that shook the country from its defeat in the First World War in 1918 to the final consolidation of the Weimar Republic in 1920. Robust Italian events include the political murders, terrorist attacks, corruption scandals and political crises associated with the dissolution of the Italian

[22] As stated earlier, the peace marches in Belgium exhibited bimodal peaks in 1981 and 1983. For parsimony they were assigned a single-year peak in 1982.

postwar political system between 1992 and 1994; the terrorist attack against the Bologna train station in 1980; and finally, the 1977 parliamentary inquiry commission into kickbacks connected to military contracts to Lockheed and the wave of terrorism that peaked in the kidnap and murder of former Prime Minister Aldo Moro in 1978. The remaining one is from the Netherlands. It is related to the Dutch version of the Lockheed corruption scandal in 1976, and the terrorist acts staged by Moluccan separatists in the Low Countries that year and the next.

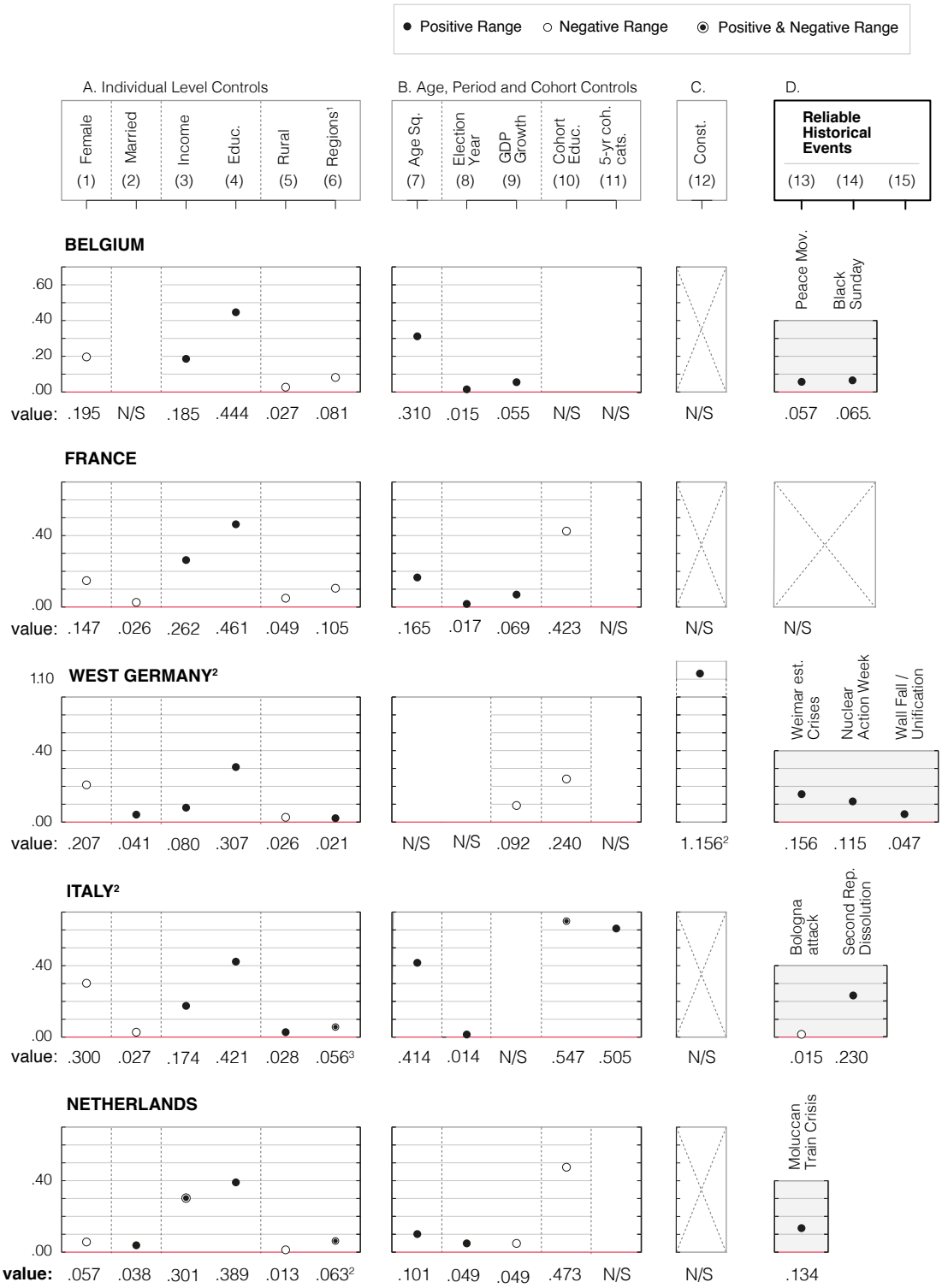
*Unidirectional predictors of political talk.* I now turn my attention to Panel B in Figure 5. Fourteen events—41% of the event sample—hold unidirectional associations with *political talk* at significance levels of 0.1 or below. These include, from Belgium, the 1991 General Election; the anti-nuclear protests and the neoliberal turn of the early 80's (peak in 1982); and the large electoral success of the extreme right in the 1936 general elections; from France, the summer '68 protests and the political convulsions they produced throughout the summer; from Germany, Reunification (1990); the *Wende* events (1982 and 1983), the terrorist acts staged by the Red Army Faction (RAF) in 1977, the erection of the Berlin Wall in 1961 and the *Der Spiegel* civil liberties scandal one year later, and the German Revolution events (1918-1920); from Italy, the events associated with the dissolution of the postwar political regime (1992-1994), and the Bologna train station attack (1980); and from the Netherlands, the Lockheed scandal and the Moluccan terrorist attacks (1976-1977).

The directionality of influence of 11 of these events goes in the expected direction: they are positively associated with *political talk*. However, three of them—the Bologna train station attack (1980), the RAF terrorist acts (1977), and the 1936 General Election in Belgium— are negative predictors of this behavior.

*"Reliable" predictors of political talk: systematic significance and consistent directionality.* My meta-analysis identifies eight events that are "reliable" predictors of *political talk*. They robust predictors of this variable (i.e., their RSS values are significant), and their relationship with it is unidirectional at standard levels of confidence. This set includes the results of the December 1991 general Elections in Belgium (peak in 1992); from Germany, Reunification (1990), the *Wende* events (1982 and 1983), and the German Revolution (1918-1920); from Italy, the events associated with the dissolution of the postwar political system (1992-1994), and the bombing of the Bologna train station (1980); and from the Netherlands, the Lockheed Scandal and the Moluccan terrorist attacks (1975 and 1976).

Most of these events are relatively recent. On average, they occurred twenty-six years before the last year of surveys included in my analysis. With the exception of the Bologna train station attack, these robust predictors of political talk are positively and substantively associated with *political talk*. Figure 6 plots the absolute value of the maximum predicted effects on *political talk* generated by these event variables, and compares them to equivalent figures for control variables that were also robust predictors of this behavior. Black dots refer to positive effects; white ones to negative effects. A black circle inside a white dot refers to effect ranges that included both negative and positive values. Overall, this graphic shows that robust events exhibit a non-negligible effect on *political talk*. The magnitudes of their estimated effects are similar to or larger than those related to community size, marital status, or gender self-identification. The predicted effects for the most influential event, the Postwar Regime Dissolution crises in Italy (1992-1994) are comparable to income effects.

**FIGURE 6**
Magnitudes of influence for Systematically Significant Covariates




N/S: Not systematically significant
[1]Reference category: highest-development region. Regional categorization available upon request.
[2]Estimated Magnitude of Influence beyond 0.4
[3]Variables include both positive and negative estimated effects.

The results I just discussed allow identifying those events that hold a strong and analytically relevant association with *political talk*. But beyond that, they also begin to show patterns of evenemential influence that are more heterogeneous than expected. I evaluate this issue more directly by examining how rates of significance and directionality of influence trend across the whole set of events I analyzed.

*Robustness of significance across events.* In Table 3, columns 1 and 2 present average values and standard errors of rates of significance for the full set of events, and for analytically relevant groups.

The mean RS value for the event set is 0.552: on average, events were significant regressors of *political talk* in slightly more than half of the regression set.[23] A large standard deviation reveals wide differences among events in their ability to be systematically associated with engaging in political discussions. These results confirm that historical events can generate long-lasting political behavioral influences, but they also indicate not all of them are endowed with the capacity to do so. All in all, they are strongly suggestive of wide heterogeneities in the degree to which historical contingencies are able to generate persistent cohort differences in political talk.

As a first step to explore factors that might be associated with this heterogeneity, I inspect the relationship between RS values and a number of event-level properties.

[23] In the rest of the section, the estimate values I report exclude results for the First and Second World Wars variables.

**TABLE 3**
Event Exposure Variables: Rate of Significance
By type

| | RS Statistics (1) average | RS Statistics (2) standard deviation | Coeff. Stats. (3) rate of positive association[1] | N |
|---|---|---|---|---|
| **ALL** | **.515** | **(.304)** | **.588** | **34** |
| **I. By Experiential Intensity** | | | | |
| Moderate Intensity | .530 | (.289) | .600 | 15 |
| Higher: Terrorism, Economic Crises | .607 | (.340) | .571 | 14 |
| Highest: Generalized Political Violence | .463 | (.334) | .600 | 05 |
| **II. By narrative complexity** | | | | |
| One Sequence of Disruptive Sequences[2] | .605 | (.279) | .699 | 10 |
| Two Sequences | .476 | (.324) | .583 | 12 |
| Between Three and Five Sequences | .569 | (.354) | .444 | 09 |
| Six or More Sequences | .625 | (.345) | .666 | 03 |
| **III. By political disruptiveness** | | | | |
| No Government Fall | .559 | (.308) | .600 | 15 |
| One Government Fall | .563 | (.278) | .555 | 09 |
| Two or more Government Falls | .531 | (.375) | .600 | 10 |
| **IV. By Lasting Political Changes Effected** | | | | |
| Did Not Produce Lasting Political Turnarounds | .512 | (.306) | .592 | 27 |
| Produced Lasting Political Turnarounds | .704 | (.308) | .571 | 07 |
| **V. By Type of Disruption** | | | | |
| Insurrections | .506 | (.559) | .500 | 02 |
| Terrorism | .784 | (.166) | .000 | 02 |
| Political Crises | .440 | (.233) | .571 | 07 |
| Protests/Strike Waves | .607 | (.316) | 1.000 | 04 |
| Elections | .753 | (.246) | .250 | 04 |
| Corruption/ State Malpractice Scandals | .298 | (.343) | .500 | 02 |
| Multiple | .543 | (.347) | .692 | 13 |

[1] Proportion of events whose average estimated coefficient was positive.
[2] Sequences of disruptive occurrences narratively independent from one another.

I begin by inspecting the relationship between rates of significance and the experiential intensity of an event. One way in which an event might become more robustly associated with political talk is by generating strong and widespread disruptions in people's lives in the form of sharply felt economic readjustments, increases in perceptions of insecurity related to terrorist attacks, or the emergence of widespread social violence. In Table 3, the first group of estimates reports RS statistics for groups of events related to these kinds of disruptions. They show little evidence that rates of significance are associated with experiential intensity. "Moderate" events hold smaller average RS values than terrorist attacks or economic crises. At the same time, they are significant regressors of *political talk* more frequently than events associated with generalized political violence.

I also explored if rates of significance were related to the number of autonomous "stories of disruption" an event is composed of.[24] The larger the number of these narrative pieces, the more likely an event might be able to bring attention to politics and generate larger effects in the long run. Section II in Table 3 reports results related to this potential relationship. If anything, what they describe is a u-shaped relationship between robustness of influence and sequential diversity: the most narratively simple and narratively complex events tend to be more robust predictors of political talk than events of moderate narrative

[24] The minimum number of disruption stories an event carried was 1. For example, the Collard protests in Belgium began, developed, and ended as a burst of collective action narrowly directed against a set of educational reforms introduced by a Socialist government right after gaining power (Tyssens 1997). For the Algerian Retreat crises, on the other hand, I counted seven, the maximum number of the event set. Thy include the 1961 Referendum on Algerian independence (February '61), a first wave of terrorist attacks staged by the OAS in France in the first half of 1971, a failed Putsch attempt against President Charles de Gaulle on April '61; the crushing of a demonstration of Muslim Algerians in Paris on October '61 and the subsequent repression of left-wing protests against these violent actions on February '62; A second wave of OAS terrorism in the beginning of 1962 and a failed murder attempt against De Gaulle on September that year; the signature of the Evian agreements that granted independence to Algeria on April '62, and the massacres of French Algerians in Bab-el-Oued and Oran in March and July of the same year, respectively.

complexity. The average rates of the former are larger than 0.65, while those from the latter are 0.47 and 0.56.

Robustness of influence might also be related to how disruptive an event was for politics when it occurred. As a measure of this disruptive capacity, I counted how many governments fell in the peak years of the events I investigated.[25] Results on this possible association are shown in Section III of Table 3. RS figures do not provide evidence that events that were more politically destabilizing are more robustly associated with *political talk*. The average rates of significance for events that provoked more than one governmental change is very similar to the ones exhibited by events that generated only one, and by those that produced none.

Another way in which an event's relationship to politics might affect the systematicity of its association with *political talk* is by triggering abrupt, comprehensive and lasting turnarounds in state action. RS figures related to these types of events are shown in section IV of Table 3. They show that events that suddenly changed the focus of state policies (for example, the German Revolution or the neoliberal turnarounds in Germany and Belgium) are much more frequent predictors of political talk (average RS=0.704) than the ones that did not, and than the event set as a whole.

The temporal characteristics of an event might also affect its capacity to become robustly associated with *political talk*. Longer events, for example, literally have more time to generate changes in people's cognitive and structural resources for political engagement. And newer events, on the other hand, might also be more likely to be robust predictors of

[25] The constitution of a new government after a regular election, or another headed by the same prime minister, were not counted as a government falls. Twenty-two events did not provoke government changes. The events related to the development of the Popular Front in France (1934-1936) were associated to six government falls, the maximum number of the event set.

political talk because the original burst of political attention is fresher and less distant from present political experiences than those from older events.

The scatterplots of Figure 7 graph the relationship between rates of significance and these temporal features—duration in Panel A and year of occurrence in Panel B.[26] The chart shows that rates of significance are positively but very weakly associated with the length of an event (r=0.061)

Figure 7 shows, on the other hand, that the age of an event and its rate of significance are related. The correlation between this meta-analytic statistic and the year of occurrence of an event (r=0.451) is relatively strong and goes in the expected direction: the capacity of an event to be robustly associated with political talk descends as it fades from present experience.[27]

Finally, to assess if substantively different events are also different in the robustness of its association with *political talk*, I grouped events according to the "type" of disruption they were most importantly associated with. RS statistics for each of these groups are shown in section V of Table 3. Among them, events driven by disruptions related to electoral campaign or results, terrorist attacks, and protests, hold average RS values that are notoriously higher than the global value of this estimate. Corruption scandals, on the other hand, show average rates of significance that are much smaller. These results are suggestive, but the small number of events they come from greatly limits their inferential power. Most of the events I analyzed include more than one dominant type of disruption. The average RS value for this group is very similar to its global figure (0.543 vs. 0.515).

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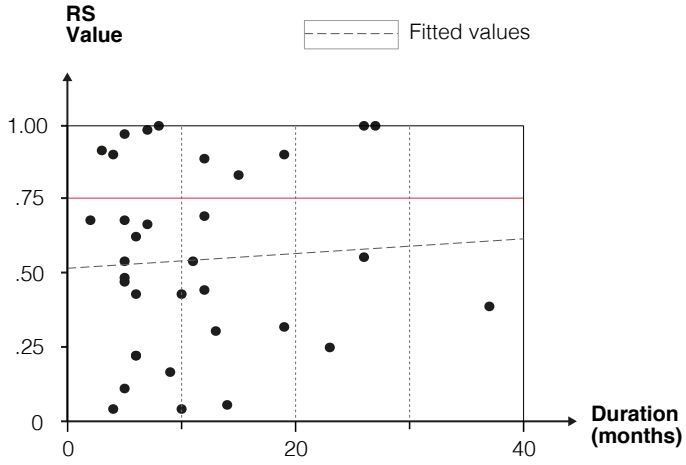
[26] The length range goes from 2 ('68 events in France) to 37 (downfall of Liberal Republic in Italy, '18 to '22).

[27] There are, however, important exceptions. The German Revolution events, for example, were significant predictors of political talk in almost all regressions. The Agusta and Dutroux Scandals in Belgium (1995–1996), by contrast, are among the least robust predictors of *political talk* despite being some of the newest events in the analysis.

**FIGURE 7**
Results for Event Exposure Variables
Scatterplots betwen Rate of Significance (RS) and Temporal Features of Events

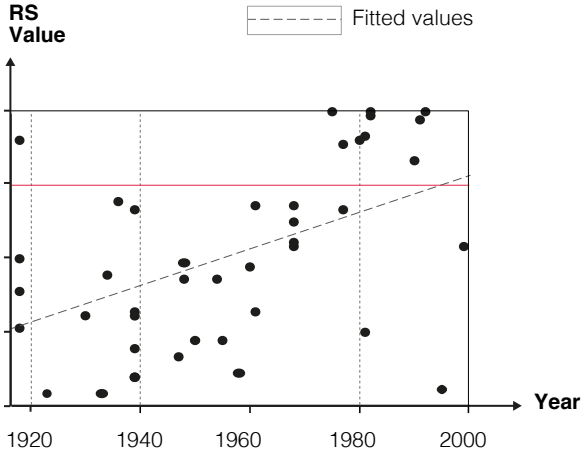
**A. BY DURATION (MONTHS)**

correlation
coefficient: .061




**B. BY YEAR OF OCCURRENCE**

correlation
coefficient: .451



*Directionality across events.* In Table 3, the third column reports, for each group of events, the proportion of events with an average estimated coefficient larger than zero—the "rate of positive association", or RPA. A value of one indicates that all events yielded a positive average estimated coefficient; a value of zero reports than that none did.

Most of the events in the analysis—20, or 58.8%—are on average positively related to political talk. This is, however, a rather thin majority. It does not provide strong evidence that the effects of historical contingencies on *political talk* are necessarily positive. What it also suggests is that there are important variations across events with regard to *how* they relate to *political talk*.

The RPA's for most of the groupings of events shown in Table 3 fluctuate between 0.4 and 0.6. These numbers indicate significant internal variations in directionality within the groups of events I inspected.

Several sets stand out for exhibiting RPA's that are substantially different from the global values of this statistic.

All four events related to instances of protest are positively associated with *political talk* on average. Events with a low and high number of disruptive sequences, on the other hand, also show a disproportionately higher rate of positive association with *political talk* (RPA's of 0.699 and 0.66). Events that encompass multiple types of disruptions also show RPA's higher than the global mean (0.699). Finally, two small-n groups show a more frequent negative relationship with political talk: electoral events (RPA=0.250), and instances of terrorism (RPA= 0.000)

## DISCUSSION AND CONCLUSION

In this investigation, I took a comparative approach to investigate long-lasting event effects on politicization. I explored in what way historical events from Belgium, France, West Germany, Italy, and the Netherlands were associated with persistent cohort differences in the frequency with which people talked about politics from 1973 to 2002.

Previous investigations have researched event effects mainly from a verificatory standpoint. Most of them focus on examining whether historical events produce lasting attitudinal influences in the specific historical contexts they examine. In my research, I also examined if there were events that were robustly associated with political talk (Hypothesis 1) in the period and countries I analyzed.

My findings provide strong supportive evidence that this is the case. Ten events—almost a quarter of the historical contingencies I analyzed—were robustly associated with persisting cohort differences in political talk engagement: they were significant predictors of this behavior in 75% or more of the regressions I analyzed. Eight of them were also "reliable" predictors of political talk, exhibiting unidirectional influences at standard levels of confidence. Their magnitude of influence was also non-negligible. The range of their predicted effects on political talk was similar to the one for gender or rural/urban residence; in one case, it was large enough to be comparable to the range of predicted effects for income. These results show that besides being able to impact attitudes and collective memories, some events might also be capable of persistently affecting politicization levels.

But besides exploring *whether* event effects exist for political talk, I also used my investigation to inquire about the logic of this influence by exploring *how, how frequently,* and

*how much* such historical moments were capable, as a category of historical phenomena, of affecting political talk. This exploration allowed me to evaluate two hypotheses related to how the literature tends to conceptualize event effects on political attributes. One predicted that as a whole, historical events would tend to be systematically associated with lasting cohort influences in political talk (Hypothesis 2), and the other contended that these would be positive (Hypothesis 3).

By taking a comparative, diversity-oriented approach to exploring event effects, I expected my analysis to reveal heterogeneities across the historical contingencies I examined on how well each of them would support these hypotheses. But the heterogeneities I found were so large and widespread that they ended up fully disconfirming them.

I found that historical contingencies were on average significant predictors of *political talk* in slightly more than half of the regressions I analyzed. Individually, however, the rate of significance for each of these events was widely and quite evenly distributed across the range of possible values this statistic could take. Seven variables attained significance as predictors of political talk in 25% or less the regressions; nine did so between 25% and 50% of the times; eight were significant in between 50% and 75% of the regression set; and finally, another ten—the events that were robust predictors of political talk—were significant independent variables in between 75% and 100% of the regressions I conducted. Jointly, these results strongly disconfirm Hypothesis 2.

Hypothesis 3, which predicted that events would be positively associated with political talk, is also strongly disconfirmed. Of the 34 variables of event influence I analyzed, only a slim majority (twenty, or 58.8% of the event set) exhibited on average positive associations with political talk. Among the events I found to be reliable predictors of *political*

*talk,* one—the terrorist attack against the Bologna Train Station (1980) — was also a negative predictor.

These heterogeneities are so widespread that they suggest that events are not different in *degree* but in *logic* of influence with respect to political talk. My analysis found events that were robust predictors of political talk, contingencies that were robustly inconsequential regressors of this behavior, and occurrences that were unsystematically associated with it in a significant way. In a similar vein, with respect to directionality, my results did not identify enough negative predictors of political talk to dismiss the generalizability of Hypothesis 3. What they strongly suggest is that there are some events that might bring political talk, and there might be some other that might bring it down.

My research contributes to advance the understanding of the role that historical events have as long-term devices for political socialization by adding to the methodological repertoire of the literature a research design aimed at evaluating these influences from a comparative quantitative perspective, and by revealing wide and previously unidentified heterogeneities in the robustness and direction with which historical contingencies might impinge on a foundational political behavior such as political talk.

As I see it, empirically and theoretically exploring these heterogeneities constitutes an exciting direction for future research. While this task is outside the scope of this investigation, the bivariate relationships that I inspected between robustness and direction of influence, on the one hand, and political, experiential, and substantive attributes of events, on the other, allow identifying several interesting associations that might serve as useful points of departure to start this exploration. I conclude this research by discussing several of them.

*Event effects might erode with time.* In its current state, the literature often describes event effects as if the maintenance of cohort differences related to an event's occurrence was secondary to its generation. This assumption could be expected if there was no association between the newness of an event and the robustness of its association with political talk. In my findings, however, more recent events tended to be more robust predictors of political talk. In fact, the relationship between the rate of significance of an event and the year when it emerged was the strongest I found. This suggests that event influences on politicization might be subject to processes of fading. One explanation for this could be that the political disruptions that an event generated tend to be harder to be resonant with present political conditions as time goes by. Another possibility is that people that might have be the most exposed by an event, after their years of maximum political engagement irremediably age and enter into life stages related to political retraction and political cognitive decay.

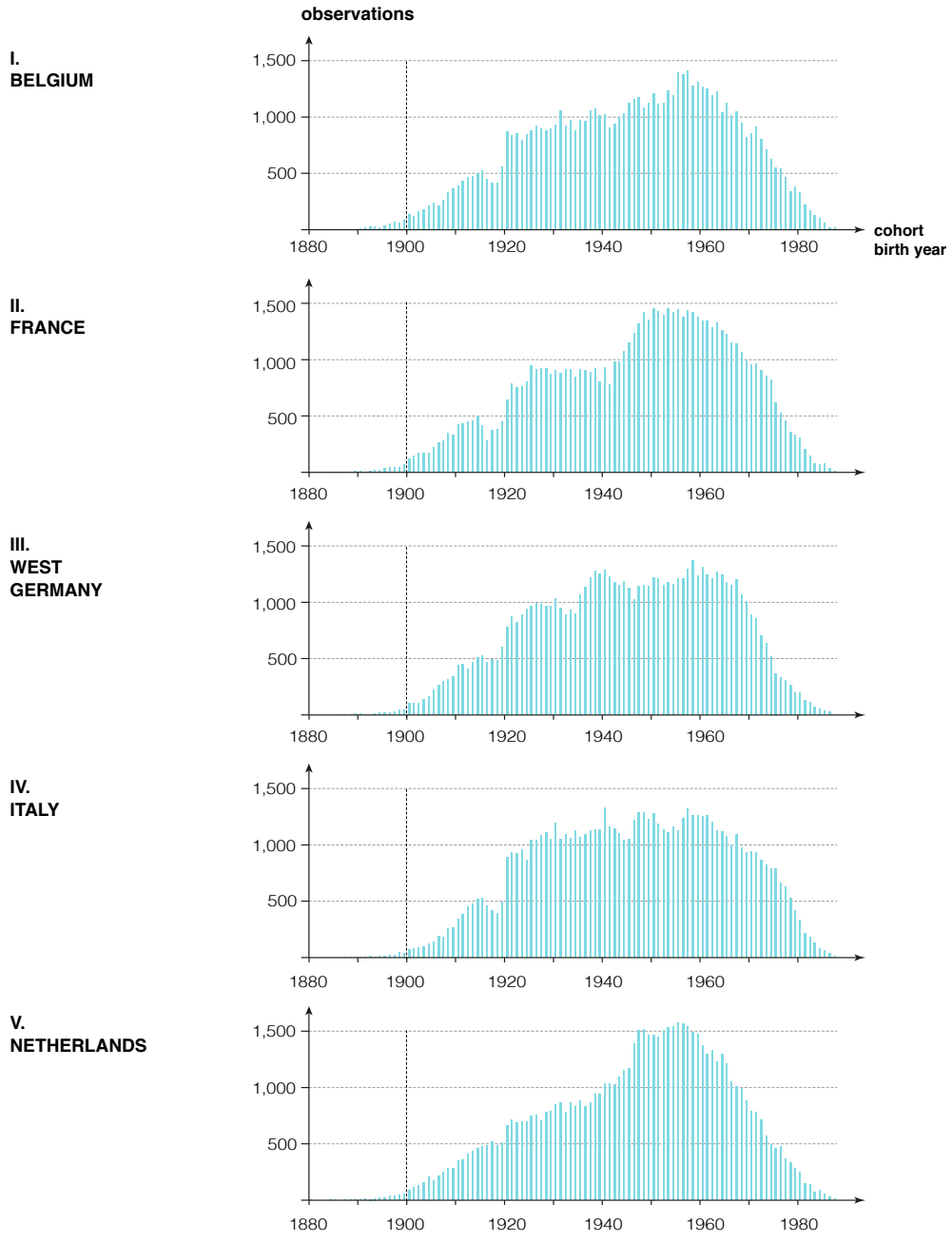
*"Effectiveness" rather than "intensity" might be more important in generating robust event effects.* It is often assumed that more intense events have stronger lasting influences on political behavior. However, when inspecting rates of significance, events that scored high in political disruptiveness, experiential strength, and narrative complexity were no more robust as explanators of political talk than events with lower scores.

On the other hand, my findings suggest that events that were able to abruptly and persistently affect the direction of state policies and actions were, in general, more likely to be robust predictors of political talk, regardless of how intense they might have been. These findings suggest that "effectiveness" —that is, the capacity of an event to gain steady access to state structures and exert lasting modifications in the policies they implement, might be a more important factor than intensity in explaining the robustness of influence of a historical contingency.

*An event's direction of influence on political talk might be related to how divisive or cohesive it is.* More divisive events might be positively associated with political talk because they facilitate identifying ideological affinity in political interaction partners, and because they increase cognitive engagement in politics as they encourage people to search for political information to define where they stand in relation to an important and highly contentious issue. Events that are cohesive, on the other hand, might make it more difficult to detect politically close individuals as they might make people talk about an issue towards which they have similar positions. In addition, because cohesive events might lack the polemic nature of their divisive counterparts, they may not motivate as much additional cognitive investment in politics. As a result, cohesive events might obstruct instead of promoting the acquisition of resources for political talk, which would ultimately affect political talk negatively in the long run. This argument might be helpful to explain why events related to terrorist acts are negatively associated with political talk. It might also be helpful to understand why events related to protests, in principle, a much more divisive political disruption than terrorist acts, are all positively associated with political talk.

# APPENDIX A

Distribution of *Political Discussion* Responses by Cohort




[1]Source: Mannheim Eurobarometer Trend data set, 1973-2002.

APPENDIX B
*Generation of* cohort size *and* cohort education *variables*

Theoretically, including *cohort size* and *cohort education* as independent variables required assigning to ach cohort under analysis a time-invariant value capturing potential long-term influences related to the size and educational attainment of a generation, even if natural attrition and late educational enrollment meant that the cohort values of these variables changed over time. Practically speaking, on the other hand, it demanded the construction of two full, internally consistent series of data for 86 different cohorts. Given the absence of a single stream of relevant historical data to build these data strings, I generated them using several sources that differed in their conceptual focus and their temporal scopes, that often held non-standardized coding procedures, and that were disrupted by two full-scale wars and one country partition. The paragraphs below discuss how I used these sources to construct data series indicating (but not directly measuring) cohort size and cohort educational attainment.

*Cohort size*

A full theoretical discussion of how cohort size relates to political talk still awaits elaboration. However, noting the particular importance that age/cohort homophily has for this behavior at young adulthood (see theoretical section), I contend that a theoretically relevant indicator of *cohort size* is related to how many coevals a person had available to interact with during young adulthood given that this might push up or down the number of potential political discussion partners she might have available. Based on this argument, the value of a respondent's *cohort size* was constructed in the following way:

(1)

$$cohort\ size_{i,j(i),k(i)} = \begin{array}{l}\text{number of people in country } k \text{ aged 20-25} \\ \text{when cohort } j \text{ was 23 years old}\end{array}$$

, where *i* refers to the *i*-th respondent of a survey, *j(*i) refers to her cohort membership, and *k(i)* to her country citizenship. I chose the age of 23 as a reference age because it is the median age of the 20 to 25 year-old age category.

The data I used to build this variable for cohorts up to 1950 was Mitchell's *European Historical Statistics* (1976). It provides country-level population data by 5-year age brackets for the years when a census was conducted (in general every 10 years). I assigned values for inter-censual years using linear imputations. From 1951 on, I used yearly data on country population compiled by the United Nations. Early census data from the countries under analysis (especially Belgium and Italy) include groups of people without age data. However, the volume of these age "non responses" was marginal and did not significantly alter estimated figures.

Most of the time these sources had direct data on the number of people aged 20 to 25. One exception was the data from Germany in 1939, which used a wide age category to report population data. In this case I imputed an estimate of the size of the 20-25 year old population, by weighting available pooled figures by the share of the population that people aged 20 to 25 occupied within this larger age category in the last year for which this data was available.

Frontier changes also challenged the comparability of data. However, relative to original country populations, changes in population resulting from border definitions were relatively small except for two cases. One was Germany at the end of the 1930s, when the National Socialist regime annexed Czechoslovakia and Austria. Due to the lack of data with which to weight 1939 population figures in Germany, I introduced this figure without

corrections. The other was again Germany after its partition at the end of the Second World War. After 1945, data on *cohort size* is based on population figures from West Germany. From 1951 onwards, these numbers were obtained using German Federal Republic Statistical Yearbooks; in 1946, they came from Mitchell (1973). From 1947-1951, I used linear imputations using 1946 and 1951 figures.

*Cohort Education*

Keeping into account that educational attainment levels has been found to be one of the strongest predictors of political engagement, this variable sought to capture possible effects related to having a more or less educated pool of potential political talkers. Under the expectation that the effects of the size of this pool are larger during young adulthood, cohort education measures centered in measuring levels of higher education enrollment when a generation was entering young adulthood. I focused on this level of educational attainment because it is the one that has been found to have stronger effects in terms of political cognition, information, and the size of political interaction networks. More specifically, I used the following formula to build an indicator of *cohort education*:

$$cohort\ education_{i,j(i),k(i)} = \frac{\text{number of university students in country } k \text{ at the year when cohort } j \text{ was 20 years old}}{\text{number of people aged 20-25 in country } k \text{ when cohort } j \text{ was 20 years old}} \quad (2)$$

, where $i$ refers to respondent $i$, $j(i)$ refers to $i$'s cohort membership, and $k(i)$ refers to her country citizenship. I chose age 20 as the age reference because it constitutes the typical median age for a higher education student. I chose the total number of students enrolled in

universities as a proxy for higher educational involvement because it was the only data on higher education available for the oldest cohorts. Due to the difficulty of obtaining figures about the numbers of people aged 18-22 from original data age categories, I chose 20 to 25 years old as the next best alternative.

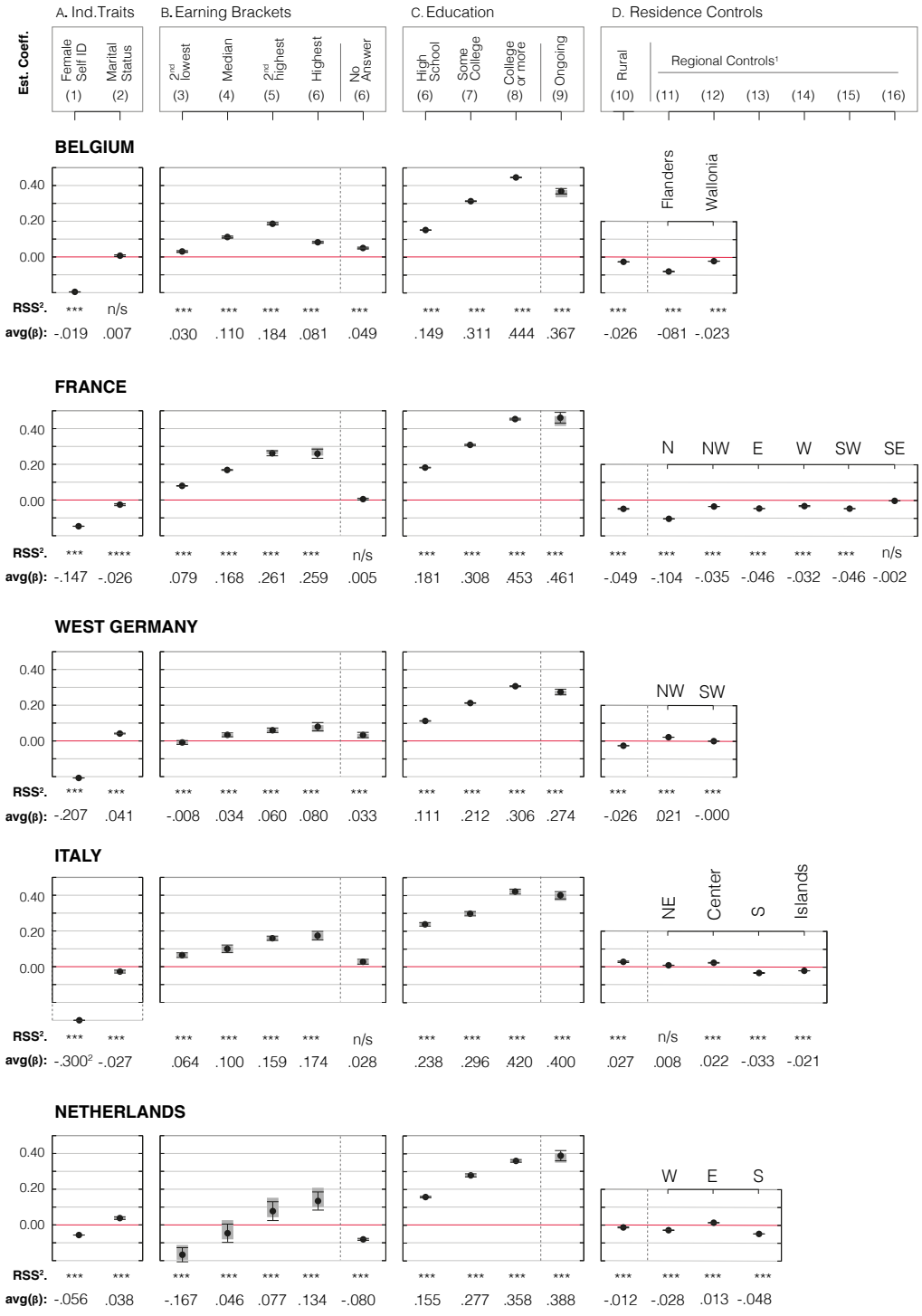
Data related to the number of people aged 20 to 25 was obtained from the figures I used for calculating *cohort size*. Data related to the number of university students were obtained from two different sources. Up to 1970 I used Mitchell's historical statistics series, which provided direct figures of university students enrolled for each of the five countries under analysis. In years with missing data, values were imputed using linear estimations analogous to the ones I used for *cohort size* except when missing data was located in years of military conflict (this happened for Belgium in World War I, and for Germany and the Netherlands in World War II). In these cases, a linear imputation would not have been able to capture decreases of university students generated by full military mobilization. For this reason, estimates on the population of university students from war periods were imputed by weighting the size of this population in the last peaceful year by percentual changes in university enrollment over the war years relative to the last peaceful year *in countries with a roughly similar war involvement* and for which complete data series on the size of university students exist during war years. Thus, for Belgium in the First World War, an estimation of higher education students in 1915, for example, was obtained by multiplying the number of university students that Belgium had in 1914 by the percentage that the number of university students enrolled in France in that year represented relative to its university student body in 1914. For Germany in the Second World War, an analogue operation was made using the United Kingdom as a reference. For the Netherlands in the Second World War, the reference was Belgium.

For 1971 onwards, raw data comes from UNESCO estimates on the numbers of students enrolled in "tertiary education". This source was chosen because data on student enrollment in university, strictly speaking, stopped being reported after 1970. Since tertiary education is a broader category than a university education, I rescaled these figures to make it minimally consistent with *cohort education* figures drawn from information of earlier years.

An optimal rescaling procedure would have required knowing the proportion that university students represented out of the total mass of tertiary education students. While I was unable to locate systematic data on this proportion in France, Belgium, Italy and the Netherlands, I was able to do so for the Federal German Republic and for unified Germany after 1989. I used these figures to calculate proxies for the size of university students elsewhere. A key assumption here is that patterns of higher education enrollment in the countries I analyzed are similar to Germany's. A priori, this does not seem to be a problematic assumption since all countries developed fairly extensive higher education systems after the Second World War. Nine data points lacked sufficient information to impute an estimate of university students using German information. In these cases, a linear imputation using the closest real values was used.

# APPENDIX C

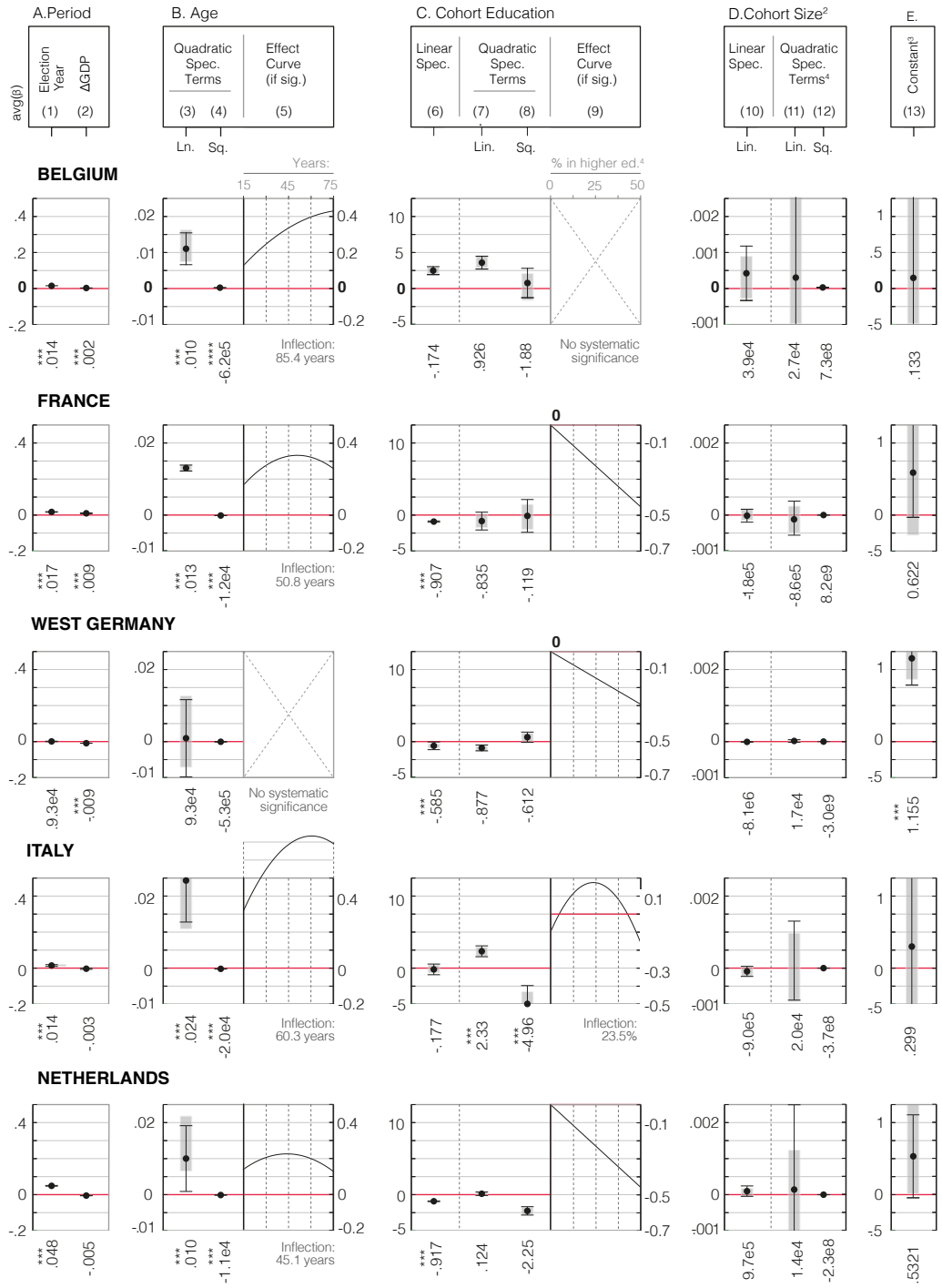
## Meta Analysis Resuts: Estimated Coefficient Parameters for Individual-Level Controls




[1] Reference categories: Most developed regions.
[2] Levels of significance for the Robust Significance Statistic:
n/s: Not significant; ⁺Significant at the .1 level; * Significant at the .05 level; **Significant at the 0.01 level; ***Significant at the 0.001 level.
[3] Estimated coefficient beyond [-0.2,0.4] range.

# APPENDIX D

## Meta Analysis Resuts: Magnitude of Estimated Coefficients for Age and Cohort Controls[1]




[1] Standard significance levels drawn from Robust Significance Statistic (RSS):
+Significant at the .1 level; * Significant at the .05 level; **Significant at the 0.01 level; ***Significant at the .001 level.
[2] Cohort size was not graphed as it was not significant in all countries under analysis.
[3] Some confidence intervals or variability ranges beyond the range plotted. Numerical results available upon request.

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# Can Historical Events Alter Social Gaps in Politicization? Evidence from the German Reunification Period

## INTRODUCTION

Although an agreement has gradually been formed in the literature on the impact that historical events have on individual-level political attributes, many questions remain unexplored or unsatisfactorily answered on how they do so. We know very little, for example, of how event effects unfold in the short run, and how they might be differently distributed across different social identities. Are the politicization increases that major political contingencies putatively generate widespread across society, or are they concentrated in specific demographic groups? Are they neutral to pre-existing gaps in political engagement across age, gender, and socio-economic status, or are they capable of modifying them?

These are relevant questions that can help us better understand if historical contingency provokes diverse social effects instead of having a homogenous impact, as it is commonly assumed. They are also important to shed empirical light on the short-term effects that events have when they occur. Although the literature agrees that the effects that events produce at this moment are key for their long-term influence, little research has been made on the effects that events produce shortly after they happen. Finally, in a historically turbulent period like the current one, exploring these influences is also important to better understand the legacies that the many political contingencies that have recently occurred across the globe may have on people that experienced them, and if they might attenuate

social disparities in political engagement—or if, on the contrary, they lead to their exacerbation.

In this paper, I investigate the questions laid down above by identifying and testing three sets of hypotheses on how shifts in politicization generated by political contingencies distribute across social traits. I derive two of them from event-based and generational theories of event effects. These propositions stem from a theoretical outlook that regards event influences as top-down processes that "imprint" on people. One set of hypotheses related to event-based research holds that event effects politicize across the board, but does not say much about whether these gains in politicization are stronger for some people more than for others. The other set of propositions, derived from generational studies, suggests that only young adults politicize when an event occurs, and that in consequence, age disparities in political engagement diminish. In addition, it also considers that because educated young adults are especially sensitive to the occurrence of an event, SES disparities in political socialization in this age group should increase.

While these works focus on explanatory forces that go straightforwardly from events to individuals to understand how they become influential, I develop another perspective that brings attention to how events are individually filtered by the perceptual characteristics of its recipients. This "perception-based" outlook contends that the politicizing effects of a major political contingency are determined by how large and how elastic the political interest of a person is, and by her level of "political environmental attention" —that is, how much she observes and thinks about ongoing external political information. As a result of how these factors vary across age, gender, and socio-economic status, I contend, first, that an event produces generalized gains in politicization that peak during young adulthood, and decrease

for younger and older ages; and second, that while events diminish political engagement disparities for the population at large, they have the opposite effect for young adults.

Using Eurobarometer survey data on the frequency with which people talk about politics—a rare everyday measure of politicization, I evaluate these three sets of hypothesis by analyzing differences in the performance of indicators of education, income, gender, and age as regressors of the frequency with which people talk about politics (henceforth "political talk") between West Germany and France before and during the German Reunification period (November 1989 – December 1990). I justify the inferential relevance of this analysis by developing an original empirical indicator of "eventfulness" based on newsweekly cover topics and applying it to inspect levels of eventfulness in these two countries over the last two decades of the twentieth century. I show that West Germany experienced high eventfulness during Reunification and a very low level of this historical property in the period that immediately preceded it; and that France held very low levels of eventfulness in both periods. As a result, I argue that comparing shifts in political talk between these two countries over these two periods generates a comparative research environment that approximates natural quasi-experimental conditions, where West Germany can be identified as a "treatment country" and France as a comparison, "control" polity.

The results of my analysis favor perception-based hypotheses over the distributional implications of event-based and generational theories. I found that the Reunification period provoked widespread (but not comprehensive) increases in politicization that reorganized how political talk related to different social characteristics. Across age, increases in political engagement follow a gradient centered in young adulthood. Young adults' gains in politicization during Reunification in West Germany were so large that they erased pre-existing differences in political engagement between them and mature adults. I also found

that gaps in political talk across education and income increased during Reunification for adults and seniors, but diminished for young adults. Overall, these findings identify historical events as phenomena with dynamic distributional logics across age. They also suggest they increase inequalities in political engagement for society at large, but might concurrently act as a politically equalizing force for young adults.

The paper is divided into five sections. In the first, I identify the distributional implications of generational and event-based theories of event effects, and develop a third outlook to the distributive properties of event effects based on historical perception features. The second section parses out the hypotheses derived from these discussions. The third section describes the research design of my investigation. It discusses German Reunification as a robust research environment to study short-term event effects. It also introduces the original empirical indicator of eventfulness I develop, and applies it to show variations of eventfulness in West Germany and France between 1980 and 2000. The fourth section presents the analysis' results, and the fifth and final one discusses their implications for the hypotheses under investigation. It also recapitulates the contributions of my investigation and discusses directions for future research.

## DISTRIBUTIONAL THEORIES OF EVENT EFFECTS ON POLITICAL ENGAGEMENT.

### *"Imprinting" Based Theories: Generational and Event-Based Outlooks*

What we know about how major political contingencies shape individual-level political attributes (which I will call "event effects") comes mainly from event-based or

"evenemential" research within historical sociology, and generational studies within the literature on political socialization.

Evenemential research has been largely responsible for rehabilitating the notion of "historical event" as a meaningful category for sociological analysis (Sewell 1996; Wagner Pacifici 2017). Works from this line of research have described these phenomena as emergent concatenations of occurrences that were collectively experienced as important political disruptions when they happened, and that provoke "durable transformations of structures". These transformations are often described as leading to moments of full-fledged political engagement. Some investigations, for instance, have related historical events with Durkheim's notion of "social fact" (Berezin 2012). Others have done so by regarding diffuse, all-encompassing social categories as the main recipients of the political transformations they produce. For example, in an influential essay, Sewell describes how the Storming of the Bastille "revealed itself in the days that followed [it] as a concrete, unmediated, and sublime instance of *the people* [my emphasis] expressing its sovereign will [Sewell 1996, 852])".

Generational literature, on the other hand, has focused mainly on empirically evaluating two event-based processes of political socialization introduced by Karl Mannheim in *The Problem of Generations* (1952). "Generational formation" is the broadest of them. It occurs "when birth cohorts experience especially striking historical events" during early adulthood (Weil 1987, 309; I will also call this life period 'youth'). By combining historical inexperience and environmental interest, this age group makes a "fresh contact" (Mannheim 1952, 253) with history that makes it especially sensitive to conditions prevailing in their present, giving historical events—in the case they emerge—ample opportunity to

reconfigure their political attributes. (Jennings and Niemi 1981; Delli Carpini 1986; Schuman and Rodgers 2004; Griffin 2005; Osborne, Jennings and Valentino 2011).

The Generational Formation hypothesis has important distributional implications. If young adults are affected by events mainly because they start to take interest in their surroundings but lack a large repository of historical experience, it follows that the political attributes of older people, whose historical experience is already saturated with political memories, and also of younger persons, who are allegedly still inattentive to social conditions, prevent them to be affected by a historical occurrence. Hence, generational research suggests that young adults are the only group capable of being affected by an event. As a corollary, given that young adults tend to lag behind seniors in political engagement, this literature suggests that events may act as equalizers of political engagement across age.

The second process of evenemential socialization that generational research has studied is the development of "generation units"—groups of young people with high levels of cultural capital and political expertise that develop a distinctive political outlook out of the historical circumstances they experience (Mannheim 1952, 253; Braungart 1974; Jennings 1987; Marwell, Aitken and Demerath 1987; Fendrich and Lovoy 1988).[28] Expcting the formation of these groups also has distributional implications. To the extent that cultural capital and political expertise is empirically associated with self-identifying as a male, and being enrolled in higher education, the formation of generation units would yield increases in gender and socio-economic disparities in political engagement among young people.

[28] I use the term "political expertise" in the sense that research on political cognition uses it (Fiske 1990).

Both the evenemential and generational literatures carry insightful propositions on how event effects on politicization might distribute differently across social groups. However, they have not been able to provide definitive answers to this question yet. Their distributional implications are mutually exclusive, and neither has been robustly verified. Few investigations have taken an evenemential approach to study event effects in the short run, and those that exist do not examine if the event they study have heterogeneous influences on different people (Sewell 1996; Pennebaker and Harber 1993; Dinisen and Jaeger 2004). Generational research, on the other hand, has produced a large amount of evidence confirming event effects on young adults (Jennings 1984; Schuman and Scott; Weil 1987; Dinas 2013). But their findings center on collective memory, partisanship, and issue attitudes, and have yet to be extended to more foundational political attitudes and behaviors.

Beyond the need to evaluate empirically the distributional implications of evenemential and generational research, understanding better how event effects are distributed in the short run also requires addressing the tendency of both of these literatures to conceptualize event influences as first and foremost macro-sociological processes. By omitting a direct discussion of the specific processes through which people attend, make sense, and react to events, in their current form the generational and the evenemential literatures implicitly characterize event effects as top-down processes of "imprinting" with a causal force mainly generated by the very emergence of a historical contingency (but see Dinas 2013). In its best-possible interpretation, this portrayal generates an outlook to historical socialization that gives precedence to mere "occurrence of history" over the actual

"experiencing" of it. In its worse, it reifies a vision of individuals as passive recipients of history.

Departing from this outlook, in this essay I characterize event effects as processes of influence whose causal potential is set up at the macro level by the development of a large-scale occurrence, but whose effective influence is defined by how this potential for influence is actively filtered by the different ways in which people engage with it.

*A perception-based theory of event effects*

As an alternative to the "imprinting-based" theories from evenemential and generational research, I see event influences on political behavior as being determined by political-perceptual conditions that filter an event's capacity to generate political engagement.

The first factor that filters an event's ability to generate political change is political interest. People that are more interested in politics will be more capable of attending to an event, identify it as a distinctive political stimulus, and consider it an important issue to think about. In consequence, they will also become more engaged in politics. Conversely, the less political interest a person has, the less likely she will be conscious of an event's occurrence, and the less capable she will be of recognizing its relevance. This will make her less prone to make politicization gains out of an event's occurrence.

In addition, the capacity of an event to produce politicization gains will be shaped by two additional political sensing features. One is elasticity of political interest. Because the comprehensive and sudden qualities of evenemential disruptions erode the everyday's naturalization as a de-politicized realm of experience (Berger and Luckmann 1966; Bourdieu

1972), if a person attends an event when she has fluid opinions about how consequential politics is, the more likely she will be to adjust upwards her perception about the importance of politics.

Finally, another factor that regulates event influences in politicization is the degree to which a person engages in political environmental attention. The more a person seeks and uses information on ongoing issues to form her political opinions (as opposed to, for example, using "textbook knowledge" about politics, ideological positions, or previous political memories), the more likely she will be politicized by an event.

Identifying these factors as forces that impinge on an event's ability to effectively change levels of political engagement does not necessarily disconfirm the expectation that events produce generalized increases in political engagement. Events are phenomena that powerfully increase incentives for political attention. They challenge people's understandings about politics and its relationship with their everyday. And by concentrating political attention into a single stimulus, they generate a political subject of common interest to talk about. These characteristics make politics more *interesting* to attend when an event occurs—which will drive political engagement up—regardless of how much people were politically engaged in the first place (Salgado and Bobba 2019).

However, the way in which political interest, the elasticity of this political attribute, and engagement in environmental political attention change across different social characteristics does make the perception-based perspective to event effects I introduced have distinctive distributional implications.

Like generational research, I contend that young adults are the age group that makes the largest evenemential gains in politicization. But this expectation is based less on

conceiving youth as a life stage that is politically inexperienced and a passive recipient of history than on considering it as one endowed with particularly political sensorial characteristics. In liberal democracies, enfranchisement increases political interest at this life stage (Zeglovits and Zandonella 2011). In addition, young adults carry political positions that have not been fully crystallized, and a comparatively short stock of political memories (Jennings and Niemi 1981: 113; Dinas 2013). They also take part in interaction foci that are more politically loaded than the ones they frequented as teenagers (Bidart and Lavenu 2005). These conditions make young adults exhibit a high degree of elasticity in their levels of political interest, and a relatively high level of political environmental attention. Because of this, an event might generate gains in politicization not only for young adults that are already interested in politics, but also for those who were previously politically apathetic. In consequence, young adults will experience large increases in political engagement when an event occurs, but in a way that will decrease politicization gaps across gender and education.

These unique political sensorial characteristics erode with age. Political memories will inevitably accumulate, and foundational political attitudes—including political interest—will tend to crystallize and become inelastic. In consequence, for those who are no longer young adults, evenemential gains in political talk will tend to be defined more strongly by pre-existing levels of political interest. Currently, political interest tends to be larger for men, more educated people, and individuals with more economic resources (Beauregard 2018; Bovens and Wille 2010; Armingeon and Schädel 2015). Because of this, events will tend to increase disparities in political engagement across gender and socio-economic status.

On the other hand, adolescents have not yet experienced the enfranchisement bonus of political interest. They socialize in environments that carry a very weak political load, and their levels of political interest tend to be low, more volatile, and tend to reflect politicization

patterns inside the domestic household (Jennings and Niemi 1974). In consequence, politicization gains in this group will tend to be lower than in young adulthood, and will also tend to increase socio-economic disparities in political engagement.

## HYPOTHESES

Based on the discussions above, I next parse out the distributional hypotheses on evenemential influences in politicization that I will test in the empirical part of this investigation.

### *Evenemential Hypotheses*

The first two hypotheses are related to how event-based research sees events as capable of provoking comprehensive increases in political activation. A "maximalist" interpretation of this contention sees events effects as full-fledged positive and completely equalizing:

> *Hypothesis 1: Fully equalizing effects.* Evenemential increases in politicization are overarching, and eliminate socioeconomic status, gender and age gaps in political engagement.

A "moderate" interpretation, on the other hand, regards event effects of politicization as wide-ranging and of equal effect across social cleavages:

> *Hypothesis 2: Experiential neutrality.* The occurrence of an event provokes comprehensive increases in political talk that do not trigger changes in socioeconomic status, gender and age gaps in political engagement.

Finally, a "minimalist" interpretation posits that events generate at least comprehensive positive changes in political engagement:

> *Hypothesis 3: Comprehensive increases.* The occurrence of an event provokes comprehensive increases in political talk.

*Generational Hypotheses.*

One generational hypothesis contends that politicization gains are exclusive to young adulthood:

> *Hypothesis 4: Generational imprinting.* The occurrence of a historical event increases politicization levels only for those that experience it during young adulthood.

A second generational hypothesis derives from theories of generation-unit formation. It posits that event effects on politicization for young adults are socially regressive because the segment of this group that is most likely to capitalize from it carries attributes disproportionately located in men and more educated individuals:

> *Hypothesis 5: Generational unit formation.* The occurrence of an event will increase education and gender gaps in political engagement for young adults.

*Perception-Based Hypothesis.*

Finally, a perception-based outlook to event effects contends that evenemential increases in politicization follow a young adulthood gradient, reduce social disparities in political engagement for young adults; and increase socioeconomic gaps in political engagement for the population at large.

> *Hypothesis 6*. The occurrence of an event will produce (a) generalized increases in political engagement that (b) will follow a young adult gradient across the life-span, (c) will increase gender and socio-economic disparities for the population at large, and (d) will decrease them for young adults.

## GERMAN REUNIFICATION AS A RESEARCH SETTING FOR EVENEMENTIAL RESEARCH

I evaluate the hypotheses presented above by comparing changes between West Germany and France in the performance that age, income, and gender categories have as predictors of engagement in political talk, an everyday measure of politicization, before and during the German Reunification period (November 1989-December 1990). By discussing the characteristics of this historical event, and by using an original indicator of eventfulness to inspect patterns of contingency in these two countries at the end of the twentieth century, I show how this comparison allows analyzing event effects under an inferentially robust quasi-experimental research environment.

*German Reunification as an Evenemential Research Context*

In 1989, the political and migratory crises that had been shaking the German Democratic Republic (GDR, or more simply East Germany) all along that year acquired a historical logic of their own in November, when massive demonstrations against the Communist government, the collapse of the inner German border (Hirschmann 1993), and the implosion of the GDR that resulted from both these processes suddenly started to make feasible something that up until the very end of that year seemed to belong to the realm of wishful thinking: the possibility of German reunification (Bozo, Rodder and Sarotte 2017). In the next fourteen months, this project began to concretize vertiginously, first through the Fall of the Berlin Wall on November 9, and then by the remarkable speed with which Helmut Kohl, who was then the Chancellor of the Federal Republic of Germany (FGR, or West Germany), was able to solve with remarkable expediency several barriers for delivering German reunification: negotiating a monetary union according to the interests of West Germany and the needs of the GDR; securing satisfactory terms of reunification to both East and West German constituencies, and persuading the nuclear powers —including the Soviet Union—not to oppose a unified German State. By the second half of 1990, these issues had been worked out. The GDR merged into West Germany on October 3. A few weeks later, the first all-German democratic elections since 1932 took place on December 2.

With respect to West Germany, Reunification is a historical environment particularly well-suited to test event effects for several reasons.

First—it goes without saying—it aligns closely with the definition of "historical event" advanced by evenemential research. It emerged without notice, was widely recognized as a powerful political rupture, and decisively changed what was thought to be politically possible at the end of the nineties.

Second, the political experiences from young adulthood of West German seniors and adults that experienced German Reunification were associated at some degree with contingencies that also related to German Unity: the *de facto* partition of the country in 1945, the 1948 Berlin crisis, and the construction of the Berlin Wall in 1961. This allows examining evenemential effects in a context where people of different ages carried political experiences from young adulthood that were minimally resonant with issues related to the event under analysis.

Third, because Reunification originated independently from West German politics and because the issue of German union was of prime interest for pretty much everyone in West Germany, using Reunification to analyze event effects allow doing so isolation from potential differences in evenemential reactions related to participatory effects (McAdam 1989), or ideological differences in evenemential attention.

Last but not least, the historical location of German Reunification generates a research context that approximates natural quasi-experimental conditions. I justify this contention in the paragraphs below.

*Eventfulness in West Germany and France at the End of the Twentieth Century*

A natural quasi-experimental environment occurs when a "treated" entity is affected by an exogenous, analytically relevant treatment condition immediately after a time when no such condition was present, and when there also exists a comparable "control" entity where the treatment condition was absent at both of these "post-test" and "pre-test" periods. For my investigation, identifying these conditions requires finding a "pre-test" moment when major historical contingencies were absent from the political experiences of two comparable

polities, and a "post-test" time adjacent to this moment when one of these polities was "treated" with an exogenous historical event and the other (the "control" entity) remained uneventful.

With respect to West Germany, Reunification seems to be an adequate operationalization of an evenemential treatment: it was unexpected, occurred exogenously, and was an epoch-changing happening. Identifying the period that preceded this event as uneventful, on the other hand, is harder with the methodological tools currently available. The literature tends to measure eventfulness dichotomously, assessing whether in a given period a historical event happened or not. This strategy can identify accurately periods of high historical contingency, but it cannot distinguish times nearing "zero" values of contingency from others when a contingent political story might have occurred but failed to be strong enough to be collectively experienced as a defining political rupture.

To address this limitation, I develop a quantitative indicator of "eventfulness" that understands historical contingency as a continuous rather than as a binary temporal property. I then apply this indicator to show that the period that preceded Reunification in Germany was largely uneventful, and that in France, a comparable country, both the Reunification period and the time before it were also historically subdued periods.

*A quantitative measure of eventfulness.* The indicator of eventfulness I develop measures the degree to which the historical conditions were characterized by politically contingency. It does so by calculating the degree to which media coverage concentrated across on-going political issues at a given time. Given that stronger and more consequential occurrences tend to be covered more frequently and longer by media (Oliver and Maney 2000; Almeida and Lichbach 2008; Herkenrath and Knoll 2011), a high degree of concentration of media attention towards a single event would indicate higher levels of contingency. Conversely,

equal media attention across developing political stories might be indicative of low eventfulness.

Based on this relationship, I develop a quantitative indicator that uses covers from newsweeklies to generate an empirical measure of eventfulness. This indicator is calculated following the formula below:

$$Eventfulness_t = \frac{n_t}{ENE_t} = \frac{n_t}{[\,1/\sum_{i=1}^{m}\left(\frac{ev_{it}}{n_t}\right)^2\,]}, \qquad (1)$$

where $n_t$ refers to the aggregate number of newsweekly covers dedicated to developing political stories during period $t$, and $ev_{it}$ to the number of covers that each of these political issues received.

The denominator of the formula is the "Effective Number of Events" (ENE). It adapts the Laakso-Taagepera index of party fragmentation (Laakso and Taagepera 1979) to generate an indicator of how fragmented or concentrated media coverage was across conjunctural political topics. Using observed inequalities in coverage across the political issues that became cover stories in a given period, the ENE calculates the number of current political stories that would gain equal coverage as cover stories. For instance, if newsweeklies covered only 3 developing political topics over a year, and each of them was a cover topic 15 times on average, the ENE would equal 3. By contrast, if one topic had been displayed in covers only 2 times on average, the ENE value would decrease to 2.25; if only one topic had been showcased in covers 15 times on average, and the rest only 2, the ENE value would decrease further to 1.54.

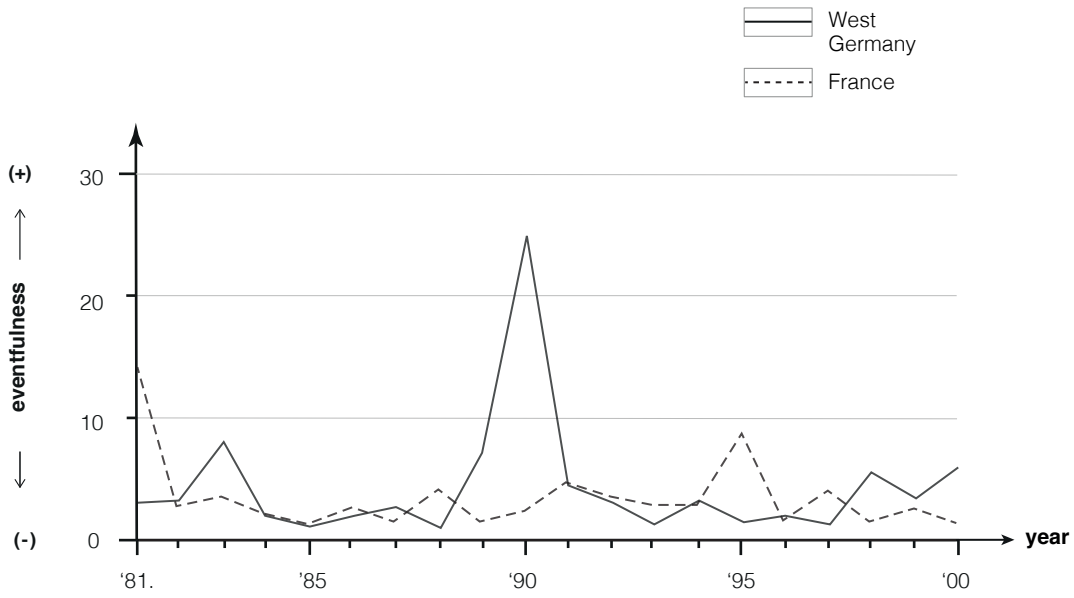
As a final step towards the calculation of *eventfulness*, I divide ENE values into the number of times that newsweeklies showed developing political stories as cover topics. The number that results from this division can be interpreted as the average amount of weeks that an event was featured as a cover topic, controlling for coverage inequalities across them. Larger numbers indicate higher eventfulness. Key values for this statistic are 0 when no current topics were headlined, 1 when no current topic became a cover theme more than once, and the total number of weeks in a given period (52, in a one-year metric) if a single event monopolized frontline media coverage in that period.

*Eventfulness in West Germany and France, 1980-2000*. In Figure 1, the continuous black line graphs trends in *eventfulness* values for West Germany between 1980 and 2000. I calculated these values using data on cover topics of *Der Spiegel,* West Germany's reference newsweekly. [29]

Over the last twenty years of the twentieth century, *eventfulness* figures experienced a moderate peak in 1983, when Helmut Kohl's arrival to power ended 14 continuous years of Socialist governments. It then exhibited minimal levels up until 1988. This year exhibited the lowest value of the *eventfulness* in the period I inspected. No current issue became a headline story more than once in this year (*eventfulness = 1*). The next year, *eventfulness* grew starkly to 7.08. This increase was entirely produced by the persistent coverage that the

[29] In the period I inspected, *Der Spiegel* was considered the most influential weekly magazine in West Germany; it also held the largest circulation number of any Wes German newsweekly (Langenbacher and Conradt 2013).

**FIGURE 1**
Yearly Values of *Eventuflness*[1]
France and Germany, 1980-2000




[1]Source: Newseelky Covers for *Der Spiegel* and *Le Nouvel Observateur.*

Fall of the Berlin Wall and its aftermath received in the covers of *Der Spiegel.* In the last quarter of the year, 10 out of 12 covers were dedicated to this event.[30] In 1990, when most of the processes related to German occurred, *eventfulness* underwent an even sharper increase—in fact, the largest I observed in the period inspected. This spike was nearly fully related to the frontline coverage of Reunification-related issues. The many subplots of this process became cover topics 29 times (a little bit more than seven months) in *Der Spiegel* over the year, pushing *eventfulness* figures to an all-time high of 24.941. The next year, my indicator of historical contingency drops sharply again. Most of the nineties exhibit minimum levels of *eventfulness* until 1998, when it started to grow as a result of Germany's military involvement

[30] In the first three quarters of the year, *eventfulness* values never go beyond 2.5. From the third to the fourth quarter, however, this figure increases more than six-fold, from 1.5 to 10; see Appendix A for a quarterly graph of *eventfulness* in this quarter and in the rest of the Reunification period.

in Kosovo and the return of the Socialists to power after 16 years of right-wing governments. At the end of 1999, the eruption of a scandal regarding the financing practices of the CDU, Germany's Demochristian party, kept increasing this statistic for the rest of the decade. The *eventfulness* value for the year 2000 is locally a peak, but its value (5.83) is smaller than that of 1990. These trends confirm that the German Reunification period was a punctuated moment of high historical contingency, and also show that its immediately preceding period was very low in eventfulness.

I also used my indicator of *eventfulness* to evaluate dynamics of historical contingency at the end of the twentieth century for France. This county was comparable to West Germany in several key social and historical trends by the end of the 1980's. Between 1975 and 1990, the correlation between GDP per capita variations between these two countries was 0.9973. In addition, the correlation between the share of the young adult population in higher education institutions between these countries in this period was 0.996. Both countries also underwent similar politico-economic contexts throughout the eighties: they underwent a steep economic crisis, adopted neoliberal reforms to tackle them, and experienced a long period of government stability led by Socialist François Mitterrand in France and by Demochristian Helmut Kohl in West Germany.[31]

[31] Data on GDP per capita comes from the World Bank; data series on the share of young people enrolled in higher education come from my comparative investigation on the relationship between events and political talk in Western Europe. Unemployment rates also correlate highly (r=0.88) between West Germany and France between 1975 and 1990 (data comes from the World Bank for France and from the OECD for West Germany). Strike activities trend similarly over time in both countries (Bordogna 2010). Both countries differ, on the other hand, on protest trends. In the eighties, demonstrations increased in West Germany (Koopmans 1993) but diminished in France (Fillieule 1997). They are also idiosyncratic in their political constitution and party system. West Germany is a federal parliamentary republic with a stable and little-fragmented party system. France, on the other hand, is a unitary presidential state with a more fragmented and unstable party system. It is unclear how these differences might strongly affect comparability between these two countries for the purposes of my investigation.

In Figure 1, using data on cover topics of *Le Nouvel Observateur,* the dotted line shows how values of my indicator of *eventfulness* trend for Germany's western neighbor between 1980 and 2000.[32]

In France, the eighties started with high *eventfulness* values in 1981, when François Mitterrand won the presidential elections and the French left obtained an absolute parliamentary majority for the first time in the twentieth century. Afterwards, the values of this statistic rapidly recede and approach 1 until jumping to 4.42 in 1988. This figure is relatively large, but it is related to media attention to regular Presidential and Legislative elections (10 covers in aggregate) rather than to unexpected events. Taking these pre-scheduled occurrences out of consideration, in this year the value of *eventfulness* also equals 1. Its value for 1989 was 1.5, and for 1990 it keeps being close to 1 (*eventfulness* = 2.5). The topic that was frequently covered that year was the breakout of protests and riots in working-class neighborhoods on the outskirts of Paris. They became headline news four times, less than a sixth of the frequency with which Reunification was showcased in the covers of *Der Spiegel.*

Overall, the trends in *eventfulness* I described above confirm that Reunification can be characterized as a "historical event"—a large and abrupt discontinuity collectively experienced as a social contingency. But in addition, they show that the particular historical location of this event makes it particularly appropriate to research event effects. In the 14-month long period that preceded the Fall of the Berlin Wall—a time span I will call the "pre-Reunification"—both West Germany and France exhibited low degrees of eventfulness. They also show that in the 14 months separating the Fall of the Berlin Wall in November

[32] In France, *Le Nouvel Observateur* held a less preponderant position as a reference newsweekly than *Der Spiegel* in West Germany, and in terms of circulation numbers was second to *L'Express,* France's oldest newsweekly (Moores 1998). I chose *Le Nouvel Observateur* because its political orientation was roughly similar to *Der Spiegel*'s, and because of the frequent changes in the editorial teams and the political leanings of *L'Express* during the time I analyzed.

1989 from All German elections of December 1990—a period I will call "Reunification"—West Germany can be considered as having been treated with a major political contingency, and France, which kept exhibiting minimum levels of eventfulness, can be considered a "control" country.

As a result of these historical trends, and of the specific characteristics that Reunification had as a political rupture for West Germany, centering my investigation of event effects on politicization in this country and its western neighbor is likely to yield inferentially robust results as it approximates a natural quasi-experimental environment.

## RESEARCH DESIGN

### *Data, Dependent Variable, and Model Specification.*

I used French and West German data from Eurobarometer surveys between July 1988 and December 1990.[33] They include a question that provides a rare opportunity to analyze politicization using an everyday behavioral measure: "When you get together with friends, would you say you discuss political matters frequently, occasionally or never?" This question, which I will refer to as "*political talk",* was included five times in the 14-month long period of Reunification, and three in the 14-month long prior period.[34] Jointly, they provide data on

[33] Since 1973, Eurobarometer surveys have systematically collected representative information on socio-demographic characteristics and political and social attitudes of people older than 14 in countries that were members of the European Economic Community up to 1973, and of the European Union from that year up to the present day.

[34] Surveys conducted during pre-reunification are Eurobarometer (EB)30, conducted in 1988 between October 17th and November 9th in West Germany, and between October 22nd and November 10th in France; EB31 (Mar 13th- April 10th in West Germany and March 17th –April 5th in France ); and EB31A (June 19th – July 4th in both countries). Surveys conducted during reunification are EB32A and EB32B, both conducted between October 19th and November 18th in West Germany

*political talk* from 10,340 respondents aged 15 and up. These numbers provide a rare opportunity to examining political talk patterns in a short span using a relative wealth of observations, and doing so with information from adolescents, which permits a more thorough life-span relative hypotheses than more frequent adult-only surveys (Griffin 2013, Osborne and Jennings 2011).

*Dependent Variable.* The dependent variable is *political talk,* an ordinal covariate with three different values: never (0), occasionally (1), and frequently (2).

*Model Specification.* Following recent methodological recommendations, I regressed *political talk* using OLS specifications (Allison 1999; Mood 2010). OLS models tend to yield similar results to ordered logistic models, but allow comparing estimated coefficients more clearly across clusters of observations.[35] I used robust-clustered standard errors to control for error clustering across cohorts. (As a robustness check, I regressed *political talk* using ordered logistic models. Their results are similar to the ones I present in this investigation; they are available upon request.).

*Regressors*

*Key independent variables.* I included four sets of categorical dummies that indicate age, educational attainment, income earning, and gender ascriptions.

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and October 16th and November 10th in France; EB33 (March 24th through April 16th in West Germany and March 19th to April 16th in France); EB 34 (October 13th – October 31st and October 12th through October 29th) and EB34.1 (November 6th to November 20th, and November 2nd to November 18th.)

[35] One drawback of applying OLS models to categorical variables is that sometimes they may yield values that are hard to interpret substantially. For my research, I ruled out this possibility as a straightforward interpretation can be given to estimated values below the minimum value (avoid talking politics, for example) and above the maximum (very frequently).

For age, I include four categories indicating a respondents' ascription to one of four *age brackets*: 15 to 19 years, 20 to 29 years, 30 to 59 years, and 60 years and more. I will refer to each of these variables as *adolescence, youth, adulthood,* and *seniority,* respectively.

I measured gender through a dummy variable indicating whether a respondent was coded as *female* (1) or not (0). I included information on income through categorical dummies that indicated a respondent's adscription to one of five monotonically increasing earning brackets.[36] Relying on information about the age at which respondents completed their last year of education, I measured educational attainment using five categorical dummies: *below high school* (education finished at 15 years old or less)*, high school* (finished between 16 and 18 years old)*; some college* (finished between 19 and 21 years)*; complete college* (finished at 22 years old or more). I also included a variable indicating *ongoing studies.*[37]

Finally, to evaluate how Reunification might have affected young adults across gender and education, I included three interactions indicating specific socio-demographic characteristics of young adults: *youth x education,* which indicates if a young respondent was enrolled in higher education (3rd, 4th and 5th *education* categories); *youth x male*, indicating if a respondent was a young man; and *youth x Berlin* indicating residence in Berlin or elsewhere.

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[36] For Germany, income brackets for each of these categories were below 3,000 Deutsche Marks (DM); between DM 3,000 and less than DM 6,000; between DM 6,000 and less than DM 9,000; between DM 9,000 and less than DM 12,500, and 12,500 marks or more. Equivalent brackets for France are below 1,000 francs (F); between F 1,000 and less than F 2,000; between 2,000 and less than F 3,000; between F 3,000 and less than F 4,000 francs; and F 4,000 francs or more. Currency values are nominal.

[37] I used information on age during the last year of education to construct these categories because Eurobarometer surveys lacked direct questions on educational attainment.
Due to continuing adult education, these dummies are imperfect indicators of educational attainment and provide increasingly conservative estimations for higher educational attainment categories. However, as some data suggest that relatively few people took part in continuing education, and that a disproportionate part of those who do already have higher degrees of educational attainment (Nuissl and Pehl 2004), I expect this bias to be marginal.

In my models, the reference category for categorical dummies of age, income, and education, is the one expected to be associated with the highest level of political engagement: *male, highest income earning, adulthood,* and *complete higher education* (Neundorf and Smets 2017; Mayer 2011; Schlozman et al. 2005; Verba, Burns and Schlozman 1997). This decision allowed me interpreting observed variations in coefficient magnitudes as reductions or increases of disparities in political talk. A decrease in the magnitude of the estimated coefficient of a variable indicates that the talk gap between the category it measures and the most politically engaged group of its class (the reference category) decreased. Positive variations indicate this gap diminished. For *youth characteristics,* the references were being a young male, having college experience, and being a young Berliner/Parisian.

*Controls.* Controls are also included as categorical dummies, and use as reference the categories more strongly associated with political engagement.

Informed by findings from previous research, I included as controls an indicator of *rural*/urban residence, and a marital status dummy that indicated if a respondent was married (1) or not (0) (Campbell at al. 1960; Laux and Simms 1973; Bearman and Parigi 2004). I also included regional controls. The reference categories were the Center West for West Germany and the Center region for France.[38] I also included a dummy indicating whether a respondent lived in Paris or Berlin (1) or not (0), and another that identifies if a young adult lived in Berlin or Paris, or elsewhere.

*Missingness at random.* Except for *income,* for which I generated a non-response dummy variable, no variable showed evidence of violating missingness-at-random assumptions.

[38] German regional categories are Northwest, Center-West, and Southwest. In France, regional categories are seven: North, Center, (including Paris), Northwest, East, West, Southwest, and Southeast.

## RESULTS

I leverage the quasi-experimental research context that German Reunification approximates to conduct two comparative analyses that test the distributional logic and breadth of the impact it had on political engagement.

The first is a single-difference analysis that evaluates how engagement in political talk and its relationship with socio-economic characteristics changed in West Germany before and during Reunification. In this analysis, I first evaluate changes in the performance of socio-economic variables as regressors of *political talk* over these two periods to gage how this historical moment changed pre-existing social gaps in political talk engagement. I do so by comparing and evaluating the statistical significance of variations in magnitudes of estimated coefficients for these variables, which I interpret as changes in observed disparities in *political talk* between each of these variables and their reference category. I next analyze variations in predicted values of *political talk* to investigate how comprehensive gains in political talk were in this period relative to its preceding one.

My second comparative examination is a "difference-in-difference" analysis of *political talk* between West Germany and France over the periods I investigate. Here I discuss and statistically evaluate differences in variations of the magnitude of estimated coefficients between these two countries, and then analyze divergences in changes in predicted values of *political talk* between them.

### *Political Talk Before and During Reunification in West Germany.*

*Changes in estimated coefficients.* Did Reunification diminish or amplify social disparities in political talk in West Germany?

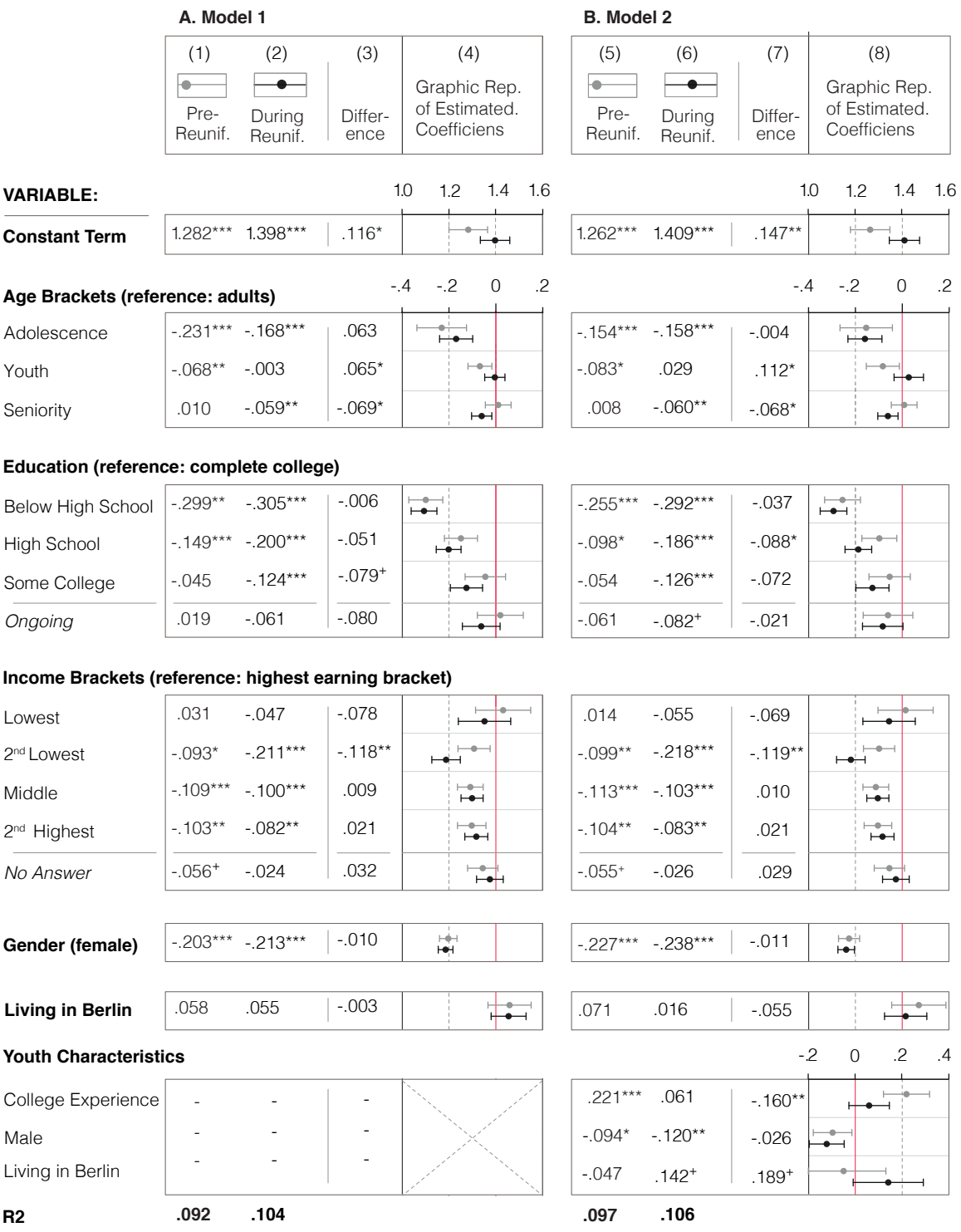
To explore this question Figure 2 presents regression results from Model 1 (columns 1 to 40) and Model 2 (columns 5 to 8). Model 1 presents the results of a baseline model that includes all socio-economic categories as regressors except for *youth characteristics*. These interaction variables are added as regressors in Model 2. For each of these models, columns 1 and 5 show estimated coefficient values for regressors of interest for pre-Reunification, and columns 2 and 6 for the Reunification period.[39] For ease of inspection, columns 4 and 8 report these results graphically. Columns 3 and 7 report the size and statistical significance of differences in the magnitude of estimated coefficients over these periods. Negative differences indicate that disparities between a variable and its reference category increased; positive values indicate it shrank.

*Gaps in* political talk *before Reunification.* Before discussing these variations, I begin by exploring regression results for the pre-Reunification period to identify baseline socio-economic disparities in political talk before the fall of the Berlin Wall (columns 1 and 5). For this reason, I will focus on discussing coefficient results as indicators of talk disparities rather than considering them as straightforward predictors of political talk.

The constant term is significant in Model 1. It sets the baseline value of political talk at 1.282, a little bit above the "sometimes talk about politics" threshold. In Model 2, the value of the intercept is 1.262 and is also significant. Since reference categories are those expected to be most positively associated with political talk, these numbers can be interpreted a priori as maximum baseline values for this variable.

[39] The table does not include results from rural, marital status, and regional controls; they are available upon request.

**FIGURE 2**
Determinants of Political Talk for West Germany
Estimated Coefficients Before and During Reunification




NOTES: + Significant at the .1 level; * Significant at the .05 level; **Significant at the .01 level; ***Significant at the .001 level.
[1] Regional and control categories not included; results available upon request.

Education is the factor most strongly associated with political talk. The talk difference between its most and least talkative category is 0.299. Differences in political talk between college graduates—the most talkative category—and educational attainment brackets for those no longer in school expand the farther away an educational attainment bracket is from higher education completion. People who are still studying talk more about politics than college graduates, but this difference is marginal and not statistically significant. Model 2 exhibits similar results.

In Model 1, age is also significantly but slightly less strongly associated with political talk than with education. Differences in estimated coefficients between the least and the most talkative category—adolescents and adults, respectively—add up to 0.231. Overall, trends in these statistics across age brackets describe a positive, non-linear relationship between political talk and age. Political talk grows from adolescence to adulthood at diminishing rates. Seniors talk marginally less about politics than adults, but this difference is not significant.

In Model 2, talk gaps between adults and younger age categories change. Concerning adolescents, it shrinks to 0.154; with respect to young adults, it increases slightly from 0.068 to 0.083. Both of these differences maintain their statistical significance. The inclusion of *youth characteristics* as regressors also allows identifying talk disparities for them. Relative to those with college experience, young people with college experience talk significantly more about politics (0.221).

For this age group, the gender gap is also substantial and significant, but it goes in the opposite direction than expected. Young women talk more about politics than young men. Talk differences between young Berliners and those living elsewhere also go contrary

to original expectations: young Berliners are associated with less political talk. This difference, however, does not attain statistical significance.

Gender is also strongly associated with political talk. In this case, this association goes in the expected direction. The estimated coefficient for the female indicator shows a significant talk gap of 0.203 between men and women. In Model 2, the size of this gap increases to 0.227.

Income is also significantly but more moderately related to political talk. Trends in estimated coefficients across earning brackets follow similar behaviors in Models 1 and 2. Against expectations, in both models the most talkative category is the bottom-earning bracket, and the least talkative is the middle earning bracket. The talk gap between them is 0.140 in Model 1 and 0.127 in Model 2. Overall, the relationship between income and *political talk* follows a quadratic relationship: middle-earning categories talk less frequently about politics, while the richest and poorest brackets hold higher and statistically similar political talk levels.

Among controls, talk differences between rural and urban, or between regional controls do not exhibit statistical significance. The coefficient for marital status does achieve significance. It shows a visible but small gap (0.076) in political talk between married (more talk) and single people (less talk). Living in Berlin is associated with more political talk, but this coefficient is non significant in both Models 1 and 2.

Finally, in terms of model fit, Model 2 exhibits a slightly larger goodness-of-fit statistic than Model 1.

*Changes in disparities in political talk engagement during Reunification.* After exploring these baseline disparities, I turn my attention to analyzing how they changed during Reunification. Was this eventful time associated with growths or reductions of political talk gaps?

To explore this question, I focus in results for Reunification (columns 2 and 6), and especially in differences in the magnitude of estimated coefficients between this period and the one that preceded it (columns 3 and 7).

The first thing worthwhile noting is the strong and significant increase of the constant term in both models. In Model 1, it jumps 0.115 units—a growth of about 9% compared with its pre-Reunification score. In Model 2, this increase grows up to 0.147, an expansion of 11.6%. This change is a first indication that Reunification might have brought general growths in *political talk*. Gains in goodness-of-fit measures for both Models, on the other hand, also indicate that they gained empirical traction during Reunification.

Age gaps in *political talk also* undergo visible changes.

In Model 1, differences between adults and seniors, which practically did not exist before, grow moderately but significantly. By contrast, talk disparities between adults and younger people shrink. This reduction is significant for young adults, who erase the talk deficit they held with respect to adults. The talk gap between seniors and adolescents also shrinks sharply, but large standard errors prevent this change from attaining significance.

Changes in the association between age and *political talk* are different in Model 2. Talk differences between seniors and adults go unchanged, as do those between adults and adolescents. The talk gap between adults and youth shrinks significantly. The magnitude of this change is twice as large as the one observed in the restricted model. This variation makes the levels of *political talk* of this age bracket statistically comparable to adults'.

In addition, socio-economic disparities in political talk within youth undergo sharp and interesting changes. The deficit in political talk between young people without college and their more educated counterparts shrinks significantly, from 0.221 to 0.061 (-72%). Young Berliners also erase and reverse their talk underperformance with respect to all other young people. During Reunification, they become associated with more political talk than those living elsewhere. This change is the largest observed of any covariate. Finally, although the talk gap between young women and men also increases, it is marginal in magnitude and not statistically significant.

Across education, talk gaps between college graduates and all other educational attainment categories widen. These disparities grow with higher levels of education. For people that left college incomplete, it is statistically significant. The gap between college graduates and continuing students undergoes a similar change that makes it flip direction. People still at school now talk less than those no longer in it. Large standard errors, however, prevent it from achieving significance. In Model 2, disparities in *political talk* between people who completed college and member of the rest of the educational attainment categories increase. Here, the largest gap growth pertains to people that stopped studying after finishing high school. This gap is statistically significant and nearly doubles the talk disparities that this education group had vis-a-vis college graduates.

Regarding income, in Model 1, talk gaps between the highest earning bracket, on the one hand, and the two income categories next to it and also the missing value category, on the other, stay stable during Reunification. By contrast, differences in *political talk* between the highest and the second-lowest earning bracket more than double. This is a statistically significant variation. Differences in *political talk* between the highest- and the lowest-income categories, on the other hand, change direction. During Reunification, the bottom-earning

category becomes associated with less *political talk* than the top one, but differences in talk between the two groups continue being non-significant due to large standard errors. In Model 2, political talk gaps across income brackets follow the same trends.

Concerning *gender,* talk gaps between men and women stay stable before and during Reunification for Models 1 and 2.

Among controls, differences in *political talk* across residential status and regional locations keep being non-significant. Finally, talk gaps between married and single people experience small, non-significant reductions in the two models under analysis.

*Changes in predicted values.* Taking the joint action of all regressors into account, was Reunification a period of comprehensive political activation in West Germany?

Analyzing variations in estimated coefficients can assess this question only partially. While increases in the constant term suggest that political talk increased across the board, in many cases, this jump was offset by observed increases in the disparities of political talk between regression covariates and their reference categories. In consequence, I analyzed how predicted values of this variable (which I will call "PV's") changed before and during Reunification for the full set of 3,024 possible combinations of values of the independent variables of the models. Each of these combinations is related to a unique combination of social characteristics captured by regressors—for example, being a elderly married woman with a college education, living in Berlin, and an income falling into a middle earning bracket. For ease of explanation, I will refer to them as "vectors of regressors" or as the "social characterizations" of the regressions.[40]

[40] My evaluation strategy departs from standard predicted value analyses, which typically compare dependent variable estimates by changing the value of a variable of interest and setting those from the rest at their average, median, or modal levels. When models allow for non-linear effects—like the one I conduct here—this strategy might yield biased results.

I report the results of this analysis for Model 2, which exhibits better goodness-of-fit measures, in Table 1.[41] (For a detailed visualization of how PV's changed across a representative set of regressor characterizations, see Appendix B.) This table shows how PV's changed during Reunification for characterizations that share a common attribute (For ease of exposition, I will refer to these groups of characterizations by the characteristic they have in common: for example, I will talk about "college graduates" when referring to the group of characterizations indicating college completion). For each of these groups, Column 1 presents "gain rate" values, or the proportion of characterizations that increased their predicted values of *political talk* during Reunification. Column 2 shows the average magnitude of these changes, and columns 3 and 4 their maximum and minimum numbers.

Globally, 70.1% of the social characterizations of Model 2 increase their predicted values of *political talk* during Reunification. These increases are, on average, 0.045. Both of these figures indicate that in general, Reunification was a period of heightened political engagement. There are however, some group figures of these statistics that depart from these pooled values.

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The vectors of regressors I analyzed exclude empirically absent combinations of values: Berlin residents living in rural areas, and adolescents with college experience.

[41] I calculated predicted values of political talk using all coefficients into consideration. I also conducted an alternative estimation of these values taking into account only statistically significant coefficients as a robustness check. In general, this estimation exhibits the same general logics of variation than the ones I report. The results for this estimation are available upon request. Results for Model 1, are presented in Appendixes C and D.

**TABLE 1**
Changes in Predicted Values of *Political Talk* Across Combinations of Regressor values
West Germany, Pre-Reunification to Reunification. Model 2

| | Gain Rate | Changes in Predicted Values | | | *N* |
|---|---|---|---|---|---|
| | (1) | (2) Avg. | (3) Max. | (4) Min. | |
| **ALL COMBINATIONS** | ***70.10%*** | **.045** | **.322** | **-.238** | **3024** |
| **Age Brackets** | | | | | |
| Adolescents[1] | *69.04%* | .033 | .185 | -.173 | 504 |
| Young adults | *97.85%* | .154 | .322 | -.058 | 840 |
| Adults | *72.73%* | .042 | .209 | -.170 | 840 |
| Seniors | *40.35%* | -.025 | .142 | -.238 | 840 |
| **Education** | | | | | |
| Some High School | *73.66%* | .059 | .285 | -.187 | 672 |
| High School | *53.86%* | .008 | .234 | -.238 | 672 |
| Some College[1] | *60.71%* | .029 | .250 | -.222 | 504 |
| Complete College[1] | *84.12%* | .101 | .322 | -.150 | 504 |
| *Still Studying* | *79.31%* | .075 | .301 | -.171 | 672 |
| **Earning Brackets** | | | | | |
| Lowest Earning Bracket | *48.80%* | .006 | .224 | -.187 | 504 |
| 2nd Lowest Earning Bracket | *29.16%* | -.043 | .173 | -.238 | 504 |
| Middle Earning Bracket | *84.72%* | .085 | .303 | -.109 | 504 |
| 2nd Highest Bracket | *87.89%* | .096 | .314 | -.098 | 504 |
| Highest Bracket | *80.35%* | .075 | .293 | -.119 | 504 |
| *Missing Bracket* | *89.68%* | .104 | .322 | -.090 | 504 |
| **Gender** | | | | | |
| Men | *72.15%* | .059 | .322 | -.227 | 1512 |
| Women | *68.05%* | .048 | .310 | -.238 | 1512 |
| **Controls** | | | | | |
| Married | *77.84%* | .075 | .322 | -.194 | 1512 |
| Single | *62.36%* | .031 | .278 | -.238 | 1512 |
| Urban Residence | *77.46%* | .074 | .322 | -.187 | 1296 |
| Rural Residence | *68.05%* | .046 | .294 | -.216 | 1296 |
| Berlin Residence | *54.16%* | .010 | .233 | -.238 | 432 |

NOTES:
1 Regessor vectors combining ascription to adolescence and college experience were excluded.

Differences in PV variability are wide across age. In one extreme, PV's for young adults increase almost always. More than 90% of the characterizations of this group exhibit larger values of *political talk* during Reunification. They also show strong and average PV increases: they are three times as large as the pooled value of this statistic. On the opposite end, PV's for characterizations indicating seniority decrease more frequently than they grow (*gain rate* = 40.35%). On average, they actually decrease their engagement in political talk by 0.025. In between lie vectors of regressors associated to adolescence and adulthood. They exhibit gain rates and average PV changes close to global values.

PV changes also vary strongly across earning categories. For the top three earning brackets and the missing value category, vectors of regressions exhibit gain rates superior to 80% and very strong average PV increases—for the second highest income group, the value of this statistic (0.096) is almost more than twice as large as its global figure. These large and comprehensive positive variations contrast starkly with the ones observed for the two bottom earning groups, whose gain rate falls short of the 50% threshold. For the lowest-earning category, average PV changes near zero. For the second-lowest one, this variation is strongly negative. This lower-middle earning category exhibits the lowest gain rate value of all the groups examined. Less than a third of the regressor instances of this group exhibit PV increases during Reunification.

PV changes across vectors of regressors vary much less when they are grouped across levels of educational attainment. All of these groups increase their PV's more often than not and also exhibit average positive changes. Regressor instances related to incomplete high school, complete college, and ongoing studies show larger gain rates and average

variations than global values.[42] On the other hand, the values of these statistics for middle educational attainment groups are notably lower than the pooled values. For people that stopped studying after completing high school, for example, PV's increased only in 53.86% of the cases, and averaged near-to-zero gains.

Across gender, men exhibit slightly larger gain rates and average change values than women. However, both of these numbers are very similar between these two groups, and do not depart much from the global values of these statistics. This is also the case between urban or rural residents, and instances of regressors related to being married or single (although gain rates and average PV values are somewhat lower in the married group). For regressors' instances related to living in Berlin, average PV changes are also similar to the global mean, but the gain rate is lower (54.16%).

*Difference-in-Differences Analysis*

I now proceed to analyze differences between variations in *political talk* observed for West Germany and those for France in the periods under analysis using results for Model 2.[43] (Appendix C presents detailed results of regressions of *political talk* before and during Reunification for France.)

[42] Among these groups, the one that exhibits the most frequent and largest positive variations is the group related to ongoing studies. This is an arithmetic consequence of the inclusion of vectors of regressors from adolescence in this group. Because these instances are excluded from college-experienced groups of instances, in those cases vectors related to seniority, which exhibit in average lower values of political talk, became more disproportionately important in the calculation of PV gain rates and average values.

[43] Results for Model 1 are available upon request.

*Variations in Estimated Coefficients.* How did West German changes in political talk disparities perform relative to those observed in France?

I discuss this question using Figure 3. Column 1 shows variations in estimated coefficients before and during Reunification for West Germany; column 2 does so for France.[44] Column 3 reports differences between West German and French variations. Negative values indicate larger variations in France, and positive values larger changes in West Germany. Using one-tailed Chow tests, this column also shows if German variations were statistically larger (i.e. decreased disparities) or smaller (i.e. amplified talk gaps) than in France. For ease of inspection, Column 4 also shows variations in estimated coefficients graphically.

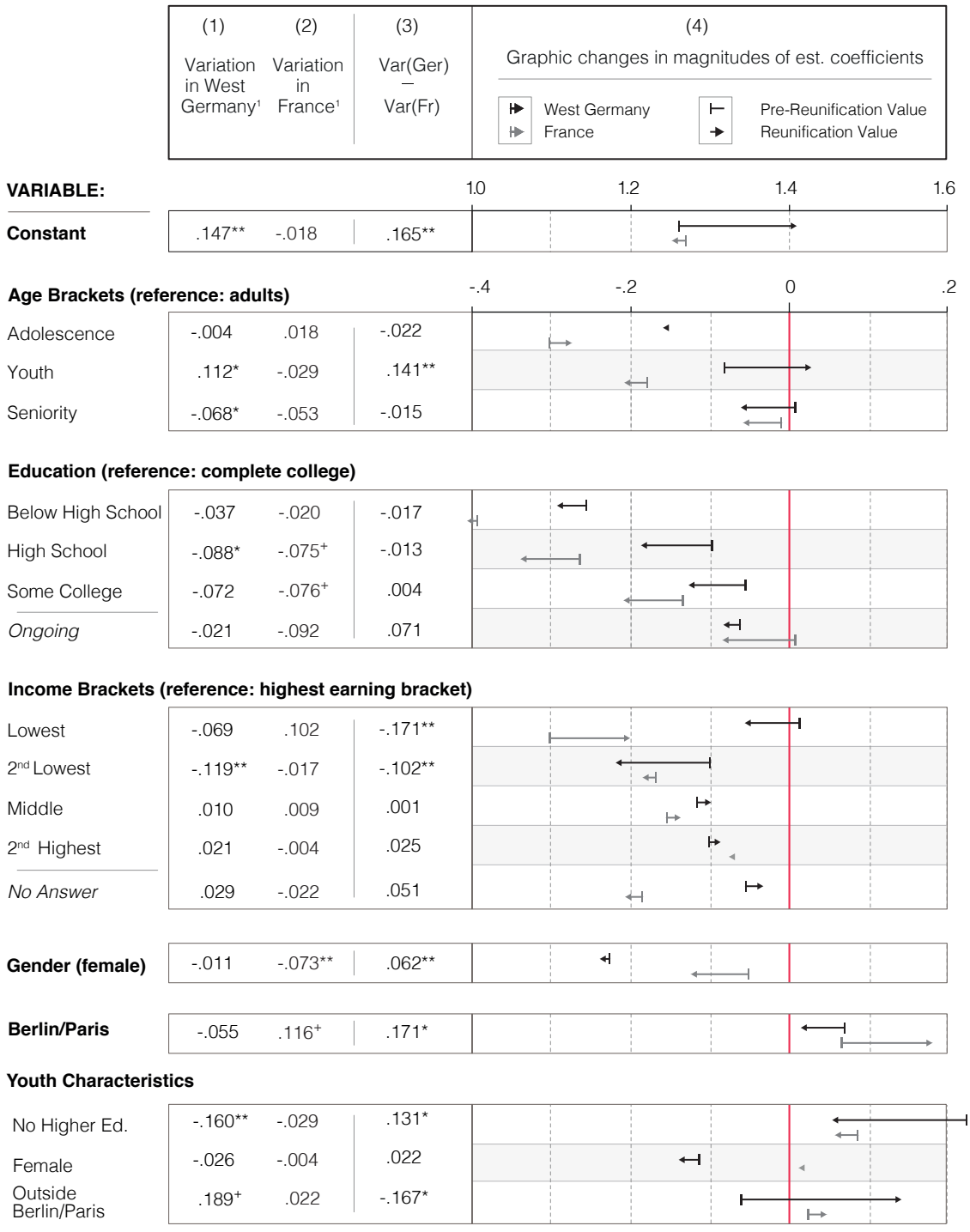
Concerning the constant term, the considerable increase of this estimate in West Germany is statistically larger than the marginal variation it undergoes in France. This difference provides further indication that German Reunification was associated with more extensive political activation.

In a similar vein, the disappearance of the political talk gap between the young and adults in West Germany is strong enough to make it significantly larger than the moderate expansion of the talk gap separating these age brackets in France. By contrast, German changes in talk gaps between adults and other age categories are not statistically different from those observed in France.

With respect to *youth indicators*, variations in disparities in *political talk* between young people with college experience and less educated peers trend in the same direction in both West Germany and France. The talk gap between these groups shrinks. In West Germany, this reduction is strong enough to make it significantly larger than France's. A similar trend

[44] Results for French regressions before and during Reunification are shown in Appendix E.

**FIGURE 3**
Differences in Variations in Estimated Coefficients between West Germany and France Before and during Reunification,Model 2




NOTES: + Significant at the .1 level; * Significant at the .05 level; **Significant at the .01 level; ***Significant at the .001 level.
[1] Regional and control categories not included; results available upon request.

occurs with respect to *political talk* differences between young people living in Berlin or Paris and those who live elsewhere. The change of direction and the strong magnitude of variation in *political talk* differences between youth living in Berlin and their peers living elsewhere in West Germany is statistically different from the analog variation between Parisian and non-Parisian young people, which is small and does not change direction. *Political talk* differences between men and women, on the other hand, trend similarly in direction and magnitude in both countries.

Across income categories, changes in *political talk* disparities between the highest-earning bracket and the two bottom-earning categories are considerably different between West Germany and France. For the bottom-earning bracket, the talk gap increases in West Germany, but it shrinks in France.[45] Both transformations are strong and significantly different from one another. For the second-lowest earning category, the expansion of the talk gap in West Germany is considerably larger than the small one observed in France. For all other income brackets, West German variations in talk disparities are not significantly larger or smaller than the ones seen in France.

Across education, the growth in talk disparities between college graduates and all other educational attainment levels observed in West Germany also occurs in France at similar magnitudes. By contrast, the expansion of the gap in *political talk* between men and women is significantly larger in France than the one observed in West Germany.

Concerning controls, the flip in the direction of talk differences between people living in rural and urban areas from positive to negative in West Germany is statistically larger than analog changes in France, where the residential gap in *political*

[45] This increment is possibly related to the saliency that violent protests in working-class neighborhoods in several large cities acquired in France in 1990.

*talk* shrinks. Growth in talk disparities between Parisians and the rest of French citizens is also statistically larger than *political talk* variations between Berliners and other German citizens in the periods under analysis.[46] Finally, West German changes in *political talk* gaps between married and single people are statistically similar to France's.

*Predicted values.* Finally, how did West German changes in predicted values of political talk compare with those for France?

Using regression results from Model 2, Table 2 reports variations in PV changes (differences in differences, or "diffs") between West Germany and France for socio-economic groupings of regressors' vectors.[47] For each of these groups, Column 1 reports the "positive variation rate", that is, the proportion of regressors' instances whose PV variation was larger in West Germany than in France. Column 2 reports average diffs values. Positive numbers indicate larger PV's in Germany, and negative numbers larger PV's in France. Columns 3 and 4 present maximum and minimum diff values, respectively.

The global rate of positive variation is similar to the gain rate. Globally, in 65.43% of regressors' instances, PV changes are larger in West Germany than in France. On average, variations in political talk in Germany are larger than in France by 0.062, a figure about a third larger than the average positive rate observed in changes within West Germany.

Across age, West German PV variations for young adults are larger than in France in 88% of the cases. The average *diff* value for this group is 0.171, the largest of all grouping of vectors of regressors. For seniors and adolescents, positive variation rates and average *diff* values indicate stronger and more frequent gains in *political talk* in West

[46] This increase might also be related to the burst of working-class demonstrations that occurred in France during 1990, as they were concentrated in the Paris metropolitan area.

[47] Combinations for regional ascriptions were not taken into account to allow comparability between West Germany and France. Coefficients from these variables were taken into account by adding to each regression vector the average magnitude of regional influence. This strategy led to the analysis of 1296 different combinations of regressor vectors between West Germany and France.

**TABLE 2**
Variations in Changes in Predicted Values of *Political Talk*
Across Combinations of Regressors' Values
West Germany vs France, Pre-Reunification to Reunification. Model 2

| | Rate of Pos.Var. | Changes in Predicted Values | | | *N* |
|---|---|---|---|---|---|
| | (1) | (2) Avg. | (3) Max. | (4) Min. | |
| **ALL COMBINATIONS** | ***65.43%*** | **.062** | **.505** | **-.360** | **1296** |
| **Age Brackets** | | | | | |
| Adolescents[1] | *53.70%* | .013 | .342 | -.360 | 216 |
| Young adults | *88.00%* | .171 | .505 | -.197 | 360 |
| Adults | *60.00%* | .030 | .364 | -.338 | 360 |
| Seniors | *55.27%* | .015 | .349 | -.353 | 360 |
| **Education** | | | | | |
| Some High School | *57.29%* | .030 | .417 | -.360 | 288 |
| High School | *58.68%* | .034 | .421 | -.356 | 288 |
| Some College[1] | *68.51%* | .067 | .438 | -.332 | 216 |
| Complete College[1] | *66.66%* | .063 | .434 | -.336 | 216 |
| Still Studying | *77.08%* | .118 | .505 | -.272 | 288 |
| **Earning Brackets** | | | | | |
| Lowest Earning Bracket | *28.24%* | -.075 | .283 | -.360 | 216 |
| 2nd Lowest Earning Bracket | *46.75%* | .006 | .352 | -.291 | 216 |
| Middle Earning Bracket | *75.00%* | .096 | .455 | -.188 | 216 |
| 2nd Highest Bracket | *81.48%* | .120 | .479 | -.164 | 216 |
| Highest Bracket | *74.07%* | .095 | .454 | -.189 | 216 |
| Missing Bracket | *87.03%* | .146 | .505 | -.138 | 216 |
| **Gender** | | | | | |
| Men | *57.56%* | .031 | .443 | -.360 | 648 |
| Women | *73.30%* | .093 | .505 | -.298 | 648 |
| **Controls** | | | | | |
| Married | *83.02%* | .137 | .505 | -.211 | 648 |
| Single | *47.83%* | -.011 | .356 | -.360 | 648 |
| Rural Residence | *85.41%* | .145 | .505 | -.178 | 432 |
| Urban Residence | *69.90%* | .079 | .439 | -.244 | 432 |
| Berlin/Paris Residence | *40.97%* | -.036 | .324 | -.360 | 432 |

NOTES:
1 Regessor vectors combining ascription to adolescence and college experience were excluded.

Germany than in France. Average diffs for vectors of regressors related to adulthood, on the other hand, exhibit a positive variation rate that is similar to the global value of this estimate but a relatively small average *diff* number.

Across income, *diffs* in predicted values of *political* talk exhibit bigger changes. These variations trend in a similar fashion than did equivalent West German differences before and during Reunification. Regressors' instances related to the top 4 earning categories and the missing value group exhibit almost larger *political talk* gains in West Germany around 75% of the time. Their average *diffs* are large and positive: in these groups, mean PV changes in *political talk* are larger in West Germany than in France. For the second-lowest earning brackets, average diffs are still positive, but they near zero. Their rate of positive variation is also low (46.75%). By contrast, this figure for the bottom-earning group is only 28.24%. It is also the only one that exhibits a negative average diff value.

Educational attainment groups exhibit positive variation rates and average diff values that are generally close to the pooled numbers of these statistics. People who are still studying exhibit the largest positive variation rate (77.08), and diff average value (0.118). High school graduates, on the other hand, underperform in these estimates relative to pooled values.

Concerning gender, women exhibit larger positive variation rates and diff averages than global values (73.3% and 0.093, respectively). By contrast, men underperform with respect to global figures. The positive variation rate for this group is 57.56%, and its average diff value is 0.031.

Among groups associated with controls, vectors of regressors indicating rural residence hold the largest positive variation rate. Eighty five percent of them undergo larger PV values in West Germany. On average, these variations are 0.145 units larger than in France. Vectors of regressors related to single and married individuals and people who live in rural areas exhibit diff statistics close to the global value. Finally, compared to PV variations for regressors' instances indicating residing in Paris, those related to living in Berlin undergo larger variations with less frequency (rate of positive variation = 40.97%). This value, however, is notoriously smaller than the global value of this estimate.

## DISCUSSION AND CONCLUSION

In this investigation, I evaluated how the relationships between socio-economic attributes and engagement in political talk changed in West Germany before and during German Reunification. Subsequently, I examined how these changes compared to those observed for France. These analyses generated rich results on how German Reunification created and redistributed political talk engagement across society, and how concentrated or widespread it generated increases in this behavior.

Did Reunification, then, amplify or reduce social gaps in political talk?

I found that in this period overall disparities in political talk across age did not grow, but became substantially reorganized. The talk gap between the most and less talkative age brackets—seniors and adolescents—did not increase significantly. But young adults notoriously increased their engagement in political talk. For West Germany, this change was significant and significantly larger than its French equivalent. This growth completely erased disparities in political talk between members of this age bracket and adults. On the other

hand, talk differences between adults and seniors, which were previously statistically comparable, underwent a statistically significant increase in West Germany.

For the population at large, I also found that political talk disparities across socio-economic factors increased during Reunification. The largest such increase occurred with respect to income. Differences in political talk engagement between the highest income earners and members of the bottom two earning brackets increased notoriously. The growth of this gap in West Germany for the second-lowest income-earning category attained statistical significance. It was also significantly larger than its French counterpart. In the backdrop of the quadratic relationship between income and political talk that existed before Reunification in West Germany, these shifts amplified the full range of income disparities in political talk. Education disparities in this behavior also moved in a similar direction. In West Germany, people who left school before completing college increased their political talk deficit with respect to college graduates. The magnitude and statistical significance of these changes, however, were moderate in intensity and significance. The talk gap between people with incomplete college and those who completed it grew significantly in West Germany. Still, it was not statistically larger than the variation in this gap for France.

For young adults, on the other hand, I found that during Reunification, socio-economic gaps changed in the exact opposite direction. Disparities in political talk between young people with and without college experience were slashed more than half. This change was significant in West Germany, and was also significantly larger than the one observed for France.

Finally, regarding gender, I found inconclusive evidence that Reunification changed disparities in political talk between men and women. The size of this gap remained

practically the same before and during Reunification in West Germany. But compared to the strong increase of this difference in France, this near-zero variation was significantly smaller. For young adulthood, evidence disconfirming increases in gender disparities was more conclusive. Variations in talk differences between young women and young men were not large in West Germany, and were statistically comparable to those observed for France.

But beyond these specific distributional changes, how widespread or concentrated politicization gains were during Reunification?

I found that predicted values of political talk increased in 70% of the combinations of regressors' values during Reunification in West Germany. And in 65% of these combinations, these variations were larger in West Germany than in France.

These figures suggest that Reunification was associated more frequently with gains in political talk than with losses. However, they also show that nearly a third of the social characterizations of the models of my investigation decreased their estimated values of political talk in West Germany. This share was also approximately the number of regressor values that exhibited smaller changes in politicization in West Germany than in France. These instances of negative change were not randomly distributed. On the contrary, they were concentrated in specific social groups. In West Germany, seniors and lower-income earners diminished their political talk engagement more frequently than not, and their political variations where bigger than French ones much less frequently than the rate at which this generally occurred. For vectors of regressors ascribed to the lowest income-earning bracket, bigger German growths in predicted political talk values were actually a small minority.

The findings I summarized above allow understanding better the political effects that German Reunification, one of the most consequential occurrences in postwar European history, produced when it was active. But in addition, due to the historical location and the specificities of German Reunification, my results also provide relevant information to assess broader issues related to how evenemential gains in political engagement distribute. For West Germany, Reunification was an externally produced event that held a homogeneous, unique importance across all meaningful political constituencies, and it resonated with youth experiences from adult and senior Germans related to other contingencies associated with German. In addition, by developing a quantitative indicator of eventfulness, I was able to identify that the eighties were overall a period of low eventfulness that was broken by Reunification in West Germany, but went uninterrupted in France. All of these features combine to make Reunification a particularly well-suited ground to research event effects, as it allows evaluating under conditions that closely approximate a natural quasi-experimental research setting.

My findings suggest that the evenemential literature has overstated historical events' ability to act as comprehensive and equalizing devices for political engagement. Reunification seems to have brought politicization up in a generalized, but not fully comprehensive way. And for several social groups—seniors and people on lower incomes, the Reunification period was strongly associated with decreases in political engagement. These results do not provide supportive evidence for Hypothesis 1, which regards events as generators of a full-fledged political activation, Hypothesis 2, which sees them producing increases in politicization of a similar magnitude across the board, or Hypothesis 3, which considers them at least capable of delivering comprehensive gains in politicization.

On the other hand, my findings also indicate that generational investigations might be overestimating how differently events affect young adults and people from other ages. My results did not support Hypothesis 4, which sees evenemential effects on politicization as exclusive of youth. People at this life stage certainly were those who increased their political engagement levels the most during Reunification. But they were not the only ones that did so: adults and adolescents also increased their engagement in political talk. More broadly, my findings suggest that age might not be the only social dimension sensitive to history. In my analysis, variations in estimated coefficients and predicted values were at least as big across income than across age.

My investigation did not find supportive evidence for processes of generational formation either. Hypothesis 5, which established that due to these processes, socioeconomic disparities in political engagement within youth would grow, was strongly disconfirmed by my findings.

With respect to Hypothesis 6, which I derived from an original perception-based outlook to evenemential influence, my investigation disconfirmed comprehensive increases in politicization, and growths in gender gaps in political engagement. The rest of the propositions of this hypothesis, however, were supported by my findings. During Reunification, increases in politicization across the life span followed a young adult gradient. They peaked for people that were coming of age, receded for seniors and adolescents, and exhibited the lowest (negative) values for seniors. In this period, in line with the expectations of this hypothesis, socioeconomic gaps in political talk expanded for the population at large, but shrank for young adults.

These evaluations carry broader implications for event-based research.

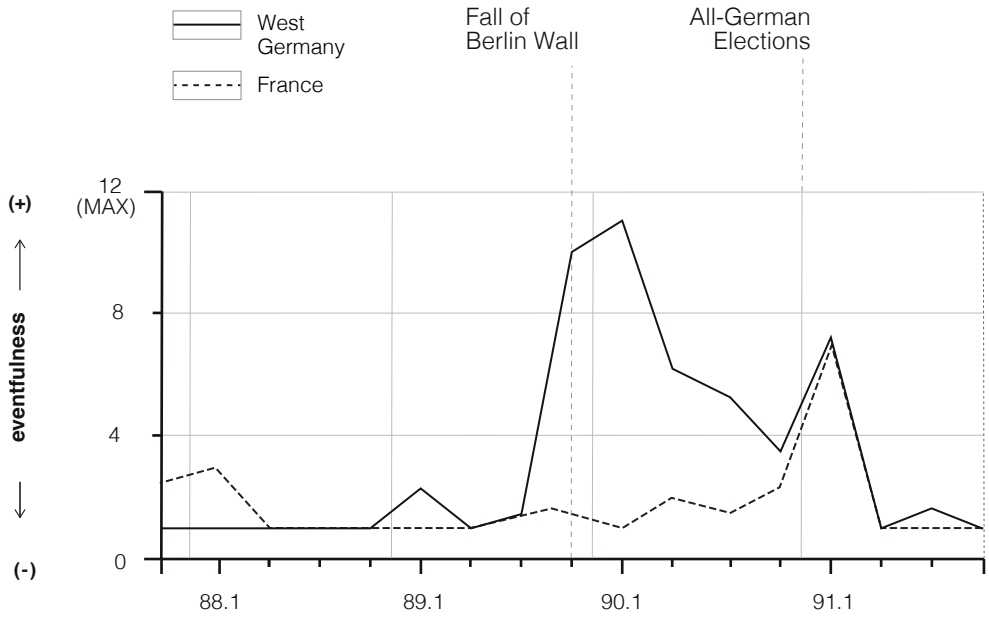
The first of them concerns our understanding of how event effects come about. Available models of evenemential influence tend to associate the causal properties of events to their facet as macro-historical facts rather than to their character as micro-historical experiences. My findings suggest that the individual-level political influences that events create should be considered as a product of the interaction of both of these features. I hope to have contributed to moving the literature forward in this direction by introducing a perception-based model of historical sensitivity that was instrumental in generating alternative explanations of how historical events generate and distribute politicization gains. This model sees evenemential influences in politicization as nuanced by levels of political interest, and amplified by the elasticity of political interest that people hold and their relative use of political environmental information. The distributional hypotheses I derived from this simple model exhibit a better empirical traction than alternative hypotheses based on top-down "imprinting-based" processes of evenemential influence. Evaluating these hypotheses for other historical contexts and conducting theoretical work aiming to refine them in future research will be instrumental in assessing more robustly their generalizability, and in extending its capacity to understand if events might actually cause a decrease in political engagement for specific social groups—namely, seniors and people of lower socioeconomic status.

Finally, my findings also have several relevant implications for how we conceive that events impact the societies where they emerge. They suggest that the way historical events tend to be depicted as moments that bring societies closer to a state of full and equal political participation should be taken with a grain of salt. Rather, they suggest that historical contingencies impinge on society through a dual distributive logic. They amplify socioeconomic inequalities in political engagement while reducing them for young adults,

assigning to events an intriguing Janus-face role in bringing societies closer to a state of equality in political participation. Looking at their own present, historical events seem to perform a regressive role, widening pre-existing inequalities in political engagement between resourceful and disadvantaged people. But when they stare at the future, by drastically diminishing politicization disparities for the newly enfranchised, events might act as progressive devices, seeding the possibility for of a more equally involved citizenry. The degree to which this potential might come to fruition depends, of course, if the distributional changes that events seem to generate for young adults are maintained over time. The extent to which this is the case also constitutes an important direction for further research.

APPENDIX A

Quarterly values of *Eventuflness*[1]
France and Germany, 1988-1991




[1]Source: Newseelky Covers for *Der Spiegel* and *Le Nouvel Observateur*.

APPENDIX B

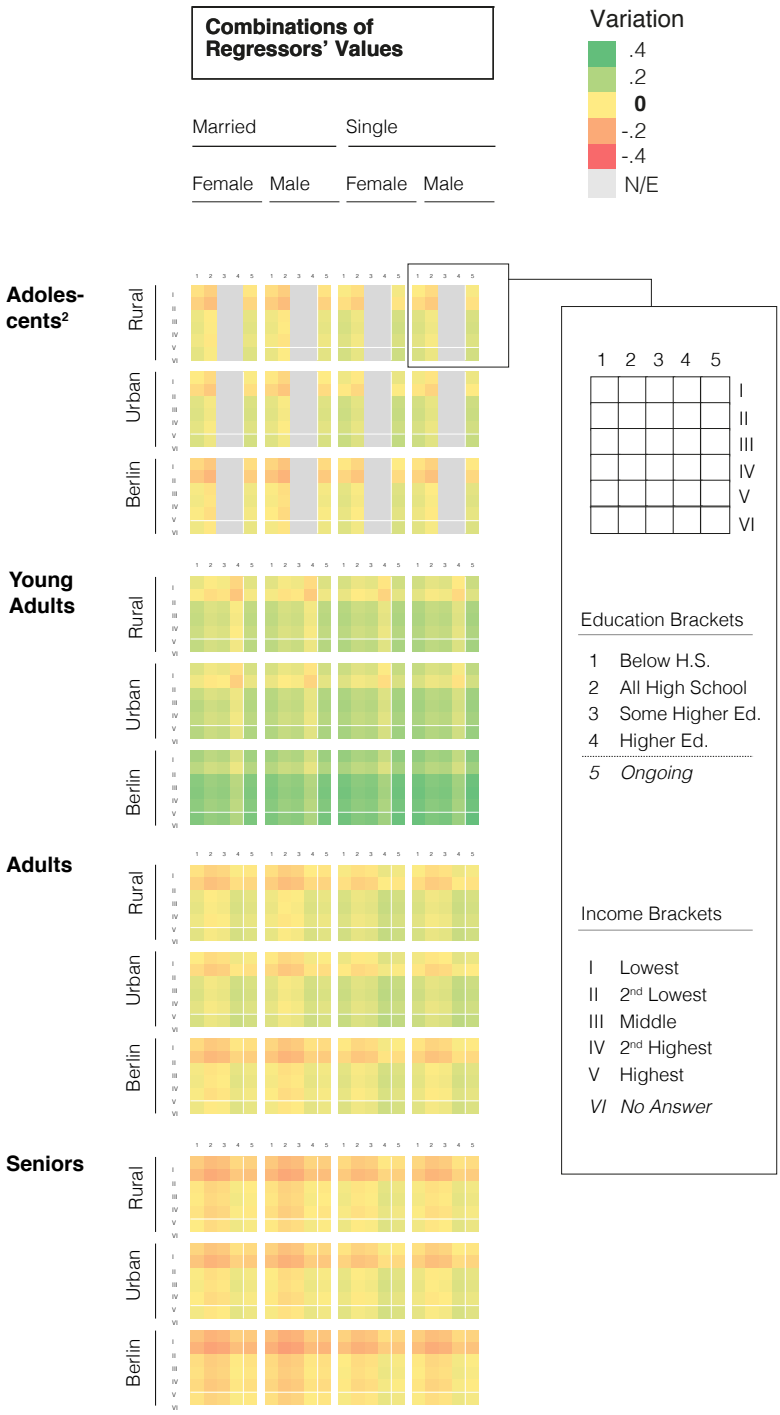
*Introduction*

Appendix B is a heat map that graphs changes in predicted values of *political talk* (or PV's) before and during reunification using all estimated coefficients.[48] Each heat cell represents changes in the predicted value of *political talk* for a specific combination or "instance" of regressor values. Greener cells refer to larger positive changes, and redder cells to steeper decreases; changes near zero are colored in yellow. Color cells are organized into four different blocks, each of which refers to a particular age bracket ascription. Each of these blocks is composed of a 4 x 3 grid of heat wave charts. Columns in this grid are different combinations of gender/marital status ascriptions, and rows indicate rural, urban, or Berlin residence. Each of this heat wave charts is made of a 5 x 6 group of heat cells. In them, columns represent different education brackets, and rows different earning categories.

[48] For simplification, the contribution of regional controls was set constant at a weighted average of the estimated coefficients for each regional category.

**APPENDIX B**

Heat Map of Changes in Predicted Values of *Political Talk* for Combinations of Regressor Values West Germany, before and during Reunification. Model 2




NOTES

[1] Simulations using all coefficients, setting the marginal contribution of regional controls using a weighted average of their estimated coefficients.

[2] Gray areas are empirically impossible combinations of variables (i.e. adolescents with college experience)

# APPENDIX C

## Changes in Predicted Values of *Political Talk* Across Combinations of Regressor Values
## Model 1

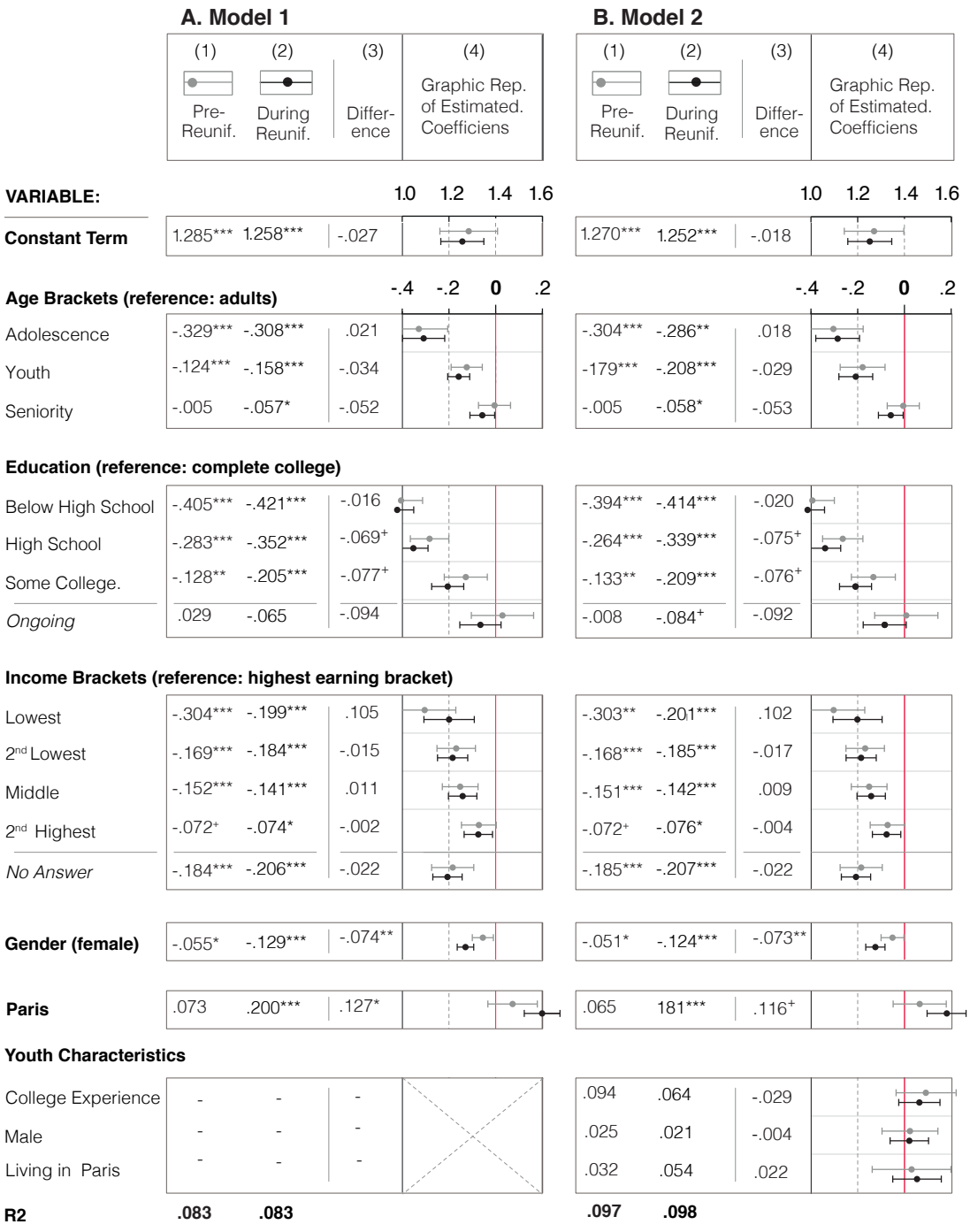
| | A. Changes in West Germany | | | | B. Variation Differenes between West Germany and France | | | |
|---|---|---|---|---|---|---|---|---|
| | Gain Rate | Changes in Pred. Values | | | Gain Rate | Changes in Pred. Values | | |
| | (1) | (2) Avg. | (3) Max. | (4) Min. | (1) | (2) Avg. | (3) Max. | (4) Min. |
| **ALL COMBINATIONS** | ***64.3%*** | **.033** | **.247** | **-.233** | ***79.4%*** | **.097** | **.379** | **-.225** |
| **Age Brackets** | | | | | | | | |
| Adolescents[1] | *85.7%* | .083 | .239 | .083 | *83.8%* | .114 | .322 | -.154 |
| Young adults | *86.2%* | .087 | .247 | -.099 | *91.9%* | .165 | .379 | -.109 |
| Adults | *63.7%* | .022 | .182 | -.164 | *73.9%* | .066 | .280 | -.208 |
| Seniors | *28.1%* | -.047 | .113 | -.257 | *69.7%* | .049 | .263 | -.257 |
| **Education** | | | | | | | | |
| Some High School | *81.3%* | .074 | .241 | -.159 | *79.9%* | .099 | .371 | -.213 |
| High School | *63.5%* | .029 | .196 | -.204 | *81.6%* | .107 | .379 | -.205 |
| Some College[1] | *43.5%* | -.015 | .168 | -.232 | *76.4%* | .083 | .359 | -.225 |
| Complete College[1] | *78.0%* | .064 | .247 | .153 | *76.9%* | .085 | .361 | .223 |
| *Still Studying* | *50.9%* | .000 | .167 | .233 | *80.9%* | .103 | .375 | .209 |
| **Earning Brackets** | | | | | | | | |
| Lowest Earning | *39.3%* | -.025 | .137 | -.193 | *27.8%* | -.051 | .142 | -.225 |
| 2nd Lowest Earning | *19.8%* | -.065 | .097 | -.233 | *62.0%* | .029 | .222 | -.145 |
| Middle Earning | *79.8%* | .062 | .224 | -.106 | *94.4%* | .130 | .323 | -.044 |
| 2nd Highest | *83.3%* | .074 | .236 | -.094 | *97.7%* | .155 | .348 | -.019 |
| Highest | *75.0%* | .131 | .215 | -.115 | *94.4%* | .132 | .325 | -.042 |
| *Missing Bracket* | *85.1%* | .085 | .247 | -.083 | *100.0%* | .186 | .379 | .012 |
| **Gender** | | | | | | | | |
| Men | *65.5%* | .036 | .247 | -.223 | *72.7%* | .065 | .315 | -.225 |
| Women | *62.0%* | .026 | .237 | -.233 | *86.1%* | .129 | .379 | -.161 |
| **Controls** | | | | | | | | |
| Married | *56.5%* | .012 | .210 | -.233 | *75.0%* | .078 | .342 | -.225 |
| Single | *70.9%* | .049 | .247 | -.196 | *83.8%* | .115 | .379 | -.188 |
| Rural Residence | *58.3%* | .017 | .220 | -.233 | *81.3%* | .097 | .315 | -.159 |
| Urban Residence | *68.9%* | .044 | .247 | -.206 | *92.1%* | .161 | .379 | -.095 |
| Berlin Residence | *64.4%* | .032 | .209 | -.202 | *64.8%* | .031 | .379 | -.225 |

NOTES:

1 Regessor vectors combining ascription to adolescence and college experience were excluded.

APPENDIX D

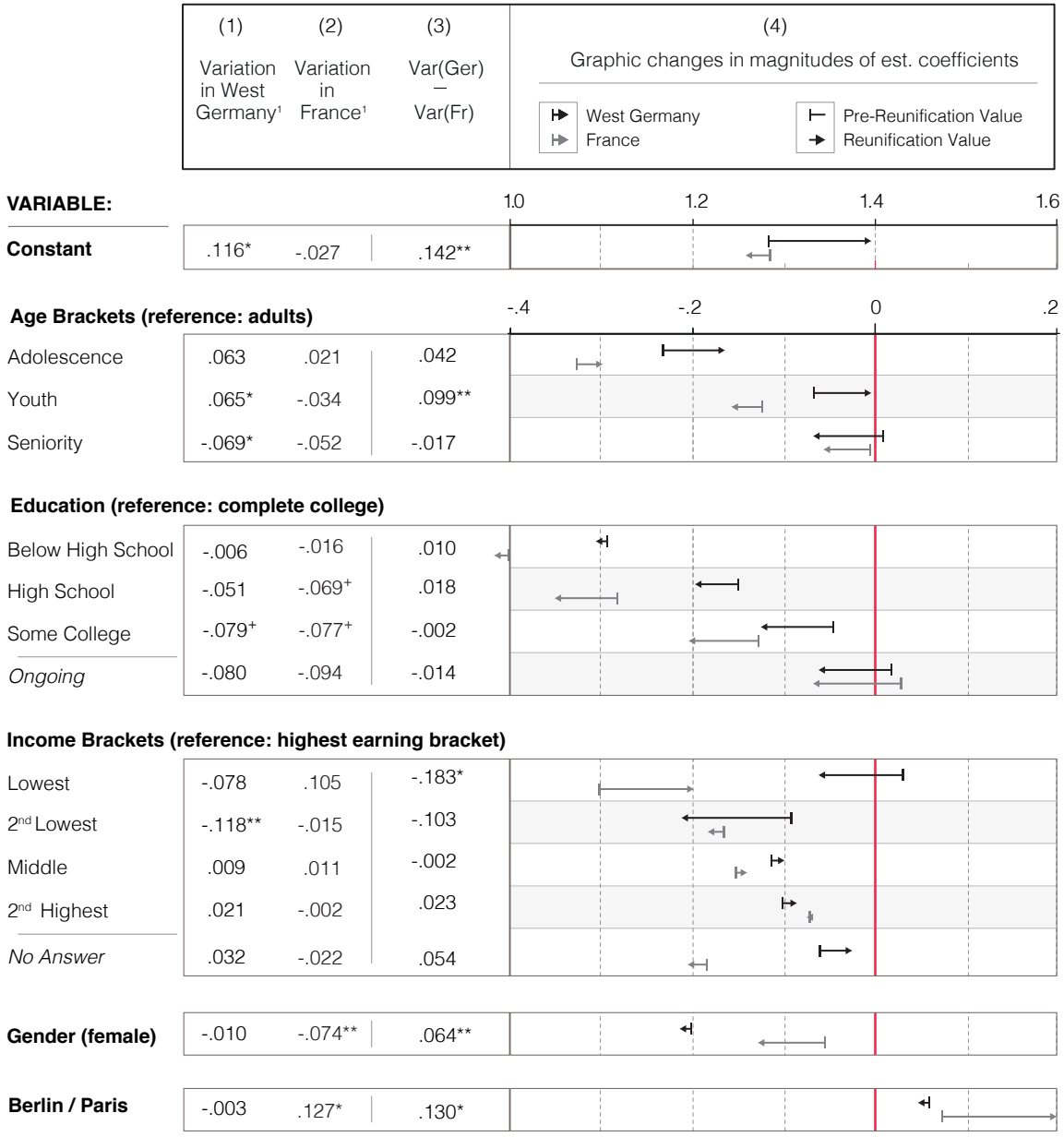
Determinants of Political Talk for France
Estimated Coefficients Before and During Reunification




NOTES: + Significant at the .1 level; * Significant at the .05 level; **Significant at the .01 level; ***Significant at the .001 level.
[1] Regional and control categories not included; results available upon request.

## APPENDIX E

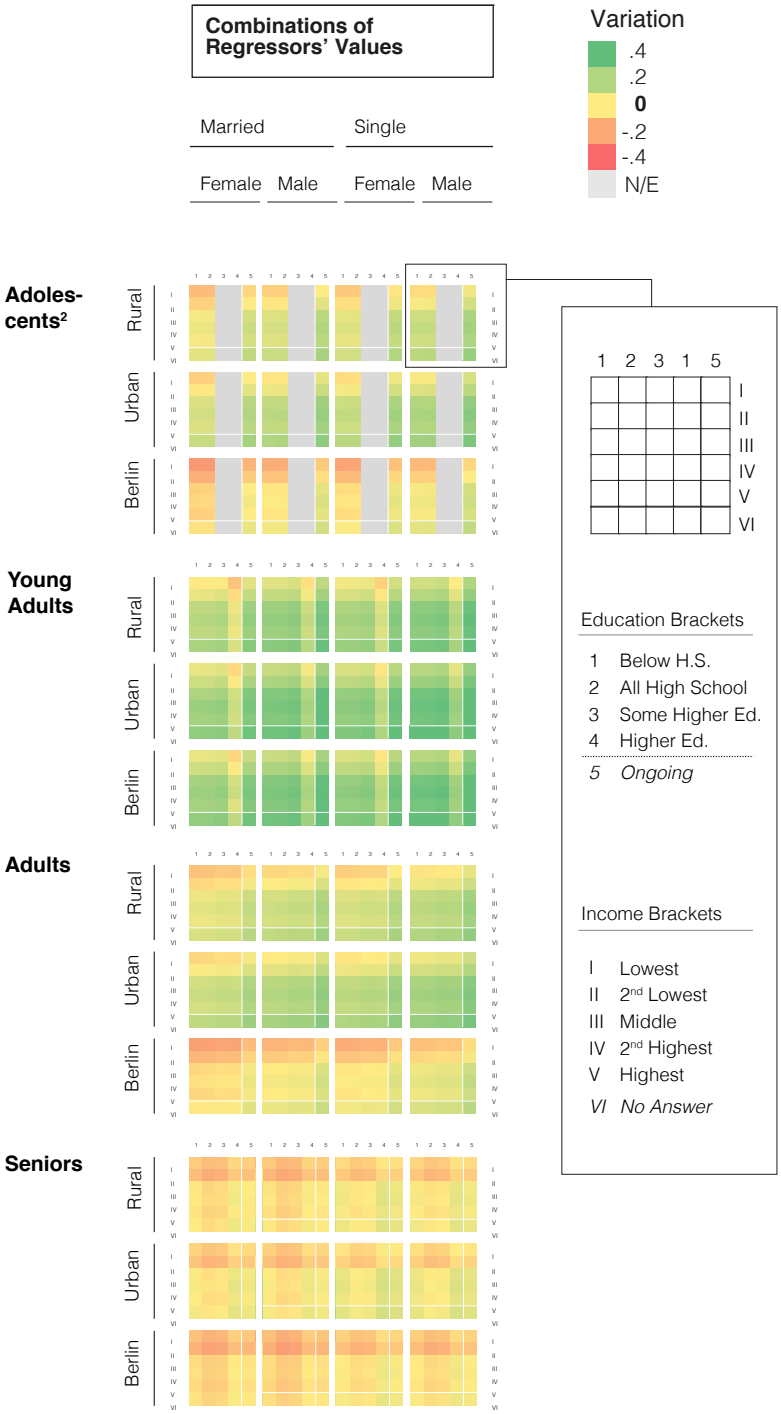
### Differences in Variations in Estimated Coefficients between West Germany and France Before and during Reunification, Model 1




NOTES: + Significant at the .1 level; * Significant at the .05 level; **Significant at the .01 level; ***Significant at the .001 level.
[1] Regional and control categories not included; results available upon request.

**APPENDIX F**

Heat Map of Differences in Changes in predicted. values of *political talk* for combinations of regressors' values.
West Germany with respect to France, before and during Reunification. Model 2



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# Taking the Victory of Jair Bolsonaro Personally: Historical Events as Bridges Between Social Domains

## INTRODUCTION

In the living room of a downtown São Paulo apartment, the conversations in a party of left-wing sympathizers following in the company of beers and peanuts the results of the 2018 presidential election in Brazil halted a few minutes past 10 PM on October 7. At that moment, a small TV began showing the images of a press conference held by the Federal Electoral Court (TSE), announcing that a run-off election between Fernando Haddad and Jair Bolsonaro would be organized on October 27 to decide the final winner of the election. They both were the most contrasting, polarizing, and voted candidates of a convulsed electoral process that had kept stirring the stormy political waters of a country that by that time had been immersed in a deep five-year-long political crisis.[49]

Runner-up Fernando Haddad was a former academic, a one-term São Paulo mayor, and a former Education minister during the administration of Inácio "Lula" da Silva, a two-

[49] Standard accounts of this crisis mark its beginning in the summer of 2013, when massive left-wing demonstrations, commonly known as the *Passe Livre* movement, took the streets to protest against corruption and economic austerity measures. It then continued in 2014 with the start of the *Lava Jato* process, a massive and highly mediatized judicial inquiry into systemic corruption practices in the Brazilian government. A year after, the investigations started to imprison first-rank political and business personalities. 2015 also saw another wave of massive protests. However, this time they were staged by right-wing organizations against state corruption and the permanence of PT member Dilma Rousseff in the Brazilian presidency. In 2016, Rousseff was finally ousted by the Brazilian congress after a controversial and highly politicized impeachment process. Two years later, as part of the *Lava Jato* process and in a highly debated decision, former president Luiz Inácio "Lula" da Silva was also indicted and shortly after imprisoned. Recent as all these episodes are, analytic accounts of them remain scarce and necessarily preliminary. Patto Sá Motta 2018 presents an overview of Brazil's political crises until 2016; Brum 2019 gives a journalistic recollection of political developments in 2018.

term president and the most dominant figure of twenty-first-century Brazilian politics. In summer 2017, Lula became the presidential candidate of the embattled Workers' Party (PT) for the seventh time in an attempt to remain electorally comptitive. The party's once-dominant position began to erode after 2014, when the country entered a steep economic crisis, and crumbled a year later when the country also began to be shaken by massive corruption scandals. In 2016, its fourteen-year long tenure in government dramatically came to an end when president Dilma Rousseff, a PT member, was impeached and removed from the presidency. Throughout 2018, Lula led the polls even after being controversially imprisoned in April in connection with a corruption scandal. His candidacy, however, was finally declared invalid by a Supreme Court decision on August 31, merely one month before the election. Haddad entered the race as the PT's last-minute candidate on September 11.

Jair Bolsonaro, on the other hand, was a retired military official and a lackluster seven-term congressman. He was long known for his right-wing extremism and his crude and vocal hatred of the PT and all things left. Once considered a curious but inconsequential aberration of Brazil's political system, his political star began to grow hand in hand with the anti-PT atmosphere that began to take over Brazil in 2015, when the country went into recession and news of massive acts of political corruption began to dominate the headlines. Once Lula was forced to suspend his campaign, Bolsonaro took the pole position of the race, particularly after he was stabbed in a rally one month before the election. But he was also a deeply polarizing figure. By the end of the campaign, the election had in practice become a referendum on his character and the politics he represented. In the week before the election, more than six hundred thousand people took Brazil's streets to express their opposition or their support for his presidential bid.

That Bolsonaro and Haddad would face-off in a second electoral round was long and resignedly expected by most left-wing sympathizers, including the people I was watching the Court's message with. What was largely unanticipated, however, was how good the confirmation of a run-off election would end up being felt after the first official results of the election that day had given Bolsonaro a shocking 48% of the votes cast, a vote share that was between 6 and 15 points higher than poll results two days before the election, and merely 2 points shy of an absolute majority that would make him president-elect that very night.[50]

My party companions reacted to the Court's message with sighs of relief almost instantly. They had averted watching Bolsonaro become president-elect that evening. But this joyful moment of decompression did not last long. Moments afterward, a new silence descended into the living room. This time, the soundless void felt more mournful than suspenseful. I attributed this quality to the fact that the results election had also relegated Haddad to a testimonial role in the run-off election, and shattered the political reality that the people in that living room had grown with.

The results of the election had wiped out the left-wing tilt of Brazilian politics. After electorally growing almost uninterruptedly since 1994, the PT and the left as a whole had lost a third of the vote share they had received in the preceding presidential election and became the minority for the first time in the century. This sobering defeat put an end, after a long period of agony, to a fourteen-year long period of left-wing governments. In the eyes of left-wing sympathizers, this period carried an unequivocally positive balance. Over this time, Brazil had experienced strong and near uninterrupted economic growth, a reduction of

[50] The next day, the final results of the election ended up giving Bolsonaro 46.6% of the votes cast, a figure more than 50% higher than Haddad's vote share (28.5%). In the city of São Paulo, both numbers were smaller, but the relative distance between them similar: Bolsonaro received 44.6% and Haddad 19.1% of the votes cast, respectively.

economic inequality, an expansion of public services, and the political empowerment and social recognition of women and minorities (Salama 2010). In the evening of October 7, this period received its coup de grace no more and no less than by the sweeping electoral victory of a crude and unabashedly open homophobe, gun-supporter, climate-change denier, gender equality attacker, and apologist of torture and dictatorship.[51] That night, the country seemed to embark on the risky political adventure, unwitnessed in any liberal democracy since the 1930's, of allowing a man with overtly fascist tendencies to arrive to power by electoral means.

Whatever my party companions were thinking about in the quietness that came after the Court's message, it didn't take long before a minimal sense of normalcy started to form. Some people resumed checking their cell phones, trying to get the most recently updated numbers of the vote count. Others occasionally threw isolated comments about the election with no apparent addressee. The most optimistic of them still maintained, each time less enthusiastically, that once the counting process reached the Northeast, a PT bastion, the election results would become much less depressing.

But this forced serenity was interrupted when Ifigênia, the host of the party, walked in direction to a balcony at the end of the living room and began roaring from that spot the signature cry out of the massive anti-Bolsonaro protests that had taken place a week before:

*"ELE NÃOOOOO!"* [NOT HIIIIIM!]

*"ELE NUUUNCA!"* [NEVER HIIIIIM!]

[51] For an overview of Jair Bolsonaro's political positions, see Dieguez 2016.

I walked to the balcony to see what these cries were about. When I got there, I realized Ifigênia's words were not the only ones being poured into the streets. Her voice was one among many shouting the same words from other balconies and other apartment towers.

The choir lasted for about fifteen minutes and then faded into the night. But by the time it disappeared, it revealed that a political breaking point had occurred that day. The void that this spectacle of vertical protest left in the air soon became impregnated with questions on how the coming days would become politically tainted. And more broadly, it also demanded to know how it is that historical events—major political disruptions collectively experienced as such—produce politicization bursts right after they emerge.

In the days that followed Election Day, I explored these questions by conducting ethnographic fieldwork in public areas and in moments of socialization between left-leaning members of personal interaction networks in the first four days after October 7. I used this fieldwork to observe how politics changed everyday experiences in the city, and evaluate how these changes related to different explanations of how events generate effects as collectively shared moments of political rupture—an influence that following William Sewell, I will call "evenemential".

My observations suggest that the electoral results of October 7 generated a political environment best described as one of "engagement without action". Politics increasingly started to occupy people's minds, developed sentiments of vulnerability, and became the dominant topic of conversation in personal interactions. But at the same time, political action almost disappeared in public spaces.[52]

[52] I understand "political action" as a minimally purposeful social behavior motivated or justified by accounts related to the field of politics. Alternatively, I understand "interaction" as that

This environment fits poorly with the two outlooks to event effects that currently exist in the literature. Rationalist theories would have expected Bolsonaro's victory to be associated with less political action and involvement. However, what I observed in my fieldwork was an absence of political action and an abrupt increase in political interest and engagement. Current cultural theories of evenemential influence, on the other hand, would have expected that any political change attributable to Bolsonaro's victory would have originated from shifts of meaning related to the realm of politics. What I observed departed from this "politico-cultural" expectation. The political modifications I witnessed were not made by changes in people's political identities as left-wing sympathizers, but by shifts in their personal self-understandings. In other words, the political changes I observed did not come from attempts to make political sense of Bolsonaro's victory; they proceeded, quite literally, from "taking it personal".

I found that this pattern could be better grasped by a "connective" take on evenemential influence, which I introduce in this investigation. This perspective sees a political event as a bundle of ruptures in political reality,[53] some of which are capable of bridging between politics and other domains of social experience that are more frequently applied, or "instantiated", in people's everyday. These bridging ruptures provide a path through which political stories might begin to infiltrate into these domains and lead to the production of political action.

My investigation is divided into five parts. The first section discusses rational and politico-cultural outlooks to evenemential politicization bursts, and introduces a third,

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part of human behavior primarily aimed at the act of communication, and not directly related to the attainment of a specific goal.

[53] I make use of the term "reality" in the way that ethnomethodology studies and investigations on the sociology of knowledge have understood it as the set of stories, accounts and social structures that are naturalized and "seen but not noticed" in peoples everyday experiences (Garfinkel 1964: 226; Berger and Luckman 1966).

connective approach to understanding it. I then proceed to discuss the methodological strategy I used to explore how the victory of Bolsonaro altered the relationship between politics and everyday life in São Paulo. In the third part, I describe the political shifts I observed in the aftermath of Bolsonaro's victory, and the fourth provides an analytical account of these shifts. The fifth section concludes by recapping the investigation's main findings and discussing its implications.

## EXPLAINING POLITICIZATION BURSTS: RATIONAL, POLITICO-CULTURAL AND CONNECTIVE APPROACHES

There are currently two theoretical frameworks available to understand how Bolsonaro's victory in particular, and historical events in general, might be able to politically charge the everyday in the societies where they emerge. One takes a rationalistic approach to evenemential influence and has been developed mainly in the literature on collective action. The other takes a politico-cultural approach to these influences and has been most clearly articulated by William Sewell, writing from the subfield of historical sociology. Both of these theories are directed at understanding how events generate behavioral effects, but they also hold implicit but distinctive views on important questions that are prerequisites for this behavioral shifts: how, in the words of Robin Wagner Pacifici (2017: 1358), events "force their way into subjects field of attention," and how this attention turns experienced time into political time.

The rationalist approach to evenemential influence sees historical events as large-scale political billboards. These contingencies act as neutral purveyors of information to political actors with fixed political attributes on the cost of participating in collective action, or on whether a specific threshold of collective action justifying engagement in it has been

met. (Francisco 2004; Hess and Martin 2006; Beissinger 2013; Shultziner 2018). This outlook contends that people are capable of attending events without much effort, as the model of political action that guides it presupposes an actor continuously (and exclusively) monitoring her social context from a political standpoint (Downs 1957; Olson 1965). An empirical illustration of how this outlook sees events provoking increases in politicization would be when someone discontent with economic austerity policies decides to join a demonstration after knowing of massive protests against them.

William Sewell articulates an alternative theory of evenemential influence in *Historical Events as Transformations of Structures* (Sewell 1996). In this seminal essay, he contends that events produce shifts in political meanings that change people's understandings of politics in a way that tends to generate a higher level of political engagement. Sewell exemplifies this process by examining how the Storming of the Bastille modified the meaning of the terms "people" and "revolution" from July 12 to July 23, 1789, in ways that favored participation in the massive wave of political action that we know now as the French Revolution.[54] His examination suggests that events politicize people's experiences by expanding the frequency and time with which politics is instantiated as a specific social experience domain. In the context that Sewell describes, a case in point would be one when someone decided to take arms against the Bourbons after reimagining her political identity from "regal subject" to "member of a sovereign people."

Different as they are, these perspectives share in common assuming that there is a strict correspondence in social domains between motivation and action: in their view, political behavior stems necessarily from political motivations instantiated in political chunks

[54] The dates indicate the moment when the Storming of the Bastille began to be imagined as a possible act and the day when this episode ended up being sanctioned as a legitimate episode by the French National Assembly.

of experience. But this assumption leaves unrecognized other paths through which an event might be able to generate political shifts. Keeping the French summer of 1789 as a context, it would have trouble identifying and making political sense, for example, of someone who became a revolutionary to avenge the death of a loved one at the hands of monarchist soldiers.

However trivial this example might seem, it allows distinguishing an alternative, previously unidentified form of evenemential politicization that I call "connective." Here, the political ruptures that events generate end up politicizing perceptions and actions from domains of social experience that are not properly political. I briefly discuss this connective path with the help of Harrison White's discussions on the relationship between social domains and identities, social contingency, and social action.

White describes individuals as bundles of social identities, each of which constitutes specific and differentiated platforms of social perception and action (White 2008, 1-10). These identities aim to control social uncertainty by generating behavioral and interpretive regularities associated with specific "domains," realms of social experience constituted by specific social networks and stories that give meaning to the relationships expressed in them.[55] In this framework, a person can, for example, act as "heterosexual" in the domain of romantic or sexual relationships, "bad dancer" in the domain of a techno party, and "leftist" in the domain of politics, a specific dimension of social experience related to the social field inhabited by politicians, activists, or policy-makers interacting around protests, legislative reforms, or electoral campaigns (Bourdieu 1991). Each of these identities drives social action when the domains they pertain to instantiate into people's experience.

[55] The word that White uses to refer to these spaces is "netdom". For ease of exposition, I will refer to them as "domains."

To control uncertainty, an identity—and by implication, the actions and perceptions it produces—shifts in reaction to changes in the social networks or stories of its underlying domain. Normally, most of these changes tend to be marginal. But in unsettled times like the ones stirred by an event, they can also change abruptly. Of course, these changes can be entirely circumscribed within the domain where an identity operates. But domains sometimes share stories and networks—think, for example, of a person that is both a close friend and a colleague from work, or how being a mother might become an essential story in family and labor domains. This opens the possibility that identities and behaviors in one domain might change in connection to shifts in other domains. For this investigation, this connective possibility suggests that another way an event might have politicization shifts is by incepting political stories or changing the constitution of networks in social domains outside politics.

The fieldwork I conducted in São Paulo was informed and catered to exploring how well this connective path and its rationalist and politico-cultural counterparts would help understand the shifts that the elections provoked. In the following section, I describe how I conducted this fieldwork.

## METHODS

Researching the consequences that a historical event produces right when it emerges is a difficult endeavor. Like tornadoes, events are short-lasting and impossible to predict. These features make it very difficult to evaluate their effects in real time using standard quantitative or qualitative research techniques. Consequently, evenemential influence has typically been investigated using retrospective testimonies, surveys with temporal spacings that make them

poorly fit to investigate immediate event effects, or accounts produced by members of dominant social strata that might be biased.[56]

In 2018, I had a rare opportunity to study event effects observationally by being in São Paulo in October that year, and by being familiar and having first-hand knowledge of the city. In the spring of 2016, I visited São Paulo as part of an architectural design project, and in the summer, I did research there for an architecture exhibition. The next year, I went back to São Paulo to work on the implementation and design of a social mapping project. These experiences allowed me to acquire ground experience with the city, and to develop a steady network of personal contacts that served as a major platform to conduct fieldwork one week before and one week after the first round of the Brazilian presidential election, and one week before and after the run-off election on October 28. Because this investigation seeks to study the immediate effects of a historical event, it is based on the fieldwork I conducted in the four days after Bolsonaro's triumph, between October 8th and 11th.

Over this period, I gathered and analyzed observations to explore the following questions: How and through which social domains people made sense of the election and the political changes it unleashed? What were the news and political information that people attended and reacted most strongly to? When and how did people behave politically, and what accounts did they mobilize to do it? How did they incorporate political information as motivations or justifications for the behavior they produced?

My fieldwork was organized in full-day long journeys that started in the morning and ended late at night. Similar to the study that Brubakers and his colleagues made on

[56] This is the case, for example, of Sewell's sources to reconstruct the effects of the Bastille. They come from written records of speeches from National Assembly members, and from articles from *Les révolutions de Paris,* a journal that was influential in Parisian revolutionary circles over the summer of 1789, and whose direct was the scant 37% of the population that is estimated that knew how to read at that moment in France (See Markoff 1986 and Gough 1988).

nationalism in the Romanian city of Cluj-Napoca, I focused on observing the effect that the Bolsonaro's triumph had on everyday life situations (Brubakers et al. 2006).[57] I did so by observing interactions in public spaces and in moments of socialization between personal contacts.

My fieldwork in public spaces focused in downtown São Paulo and in Avenida Paulista. They both are zones with an intense public life, and of symbolic importance in the city. Downtown São Paulo is a collection of streets, pedestrian areas, parks, and towers of offices and apartments that blends middle-class residents with white-collar workers and a large homeless population. Avenida Paulista, on the other hand, is São Paulo's most emblematic thoroughfare. It borders Bela Vista, a rapidly gentrifying middle-income area, and Jardins, a high-rent enclave in the city's central region. It is historically the city's flagship business district and its most diverse and densely used public interaction space.

I also engaged in participant observation of moments of socialization between my contacts, their friends, and friends of their friends in coffee shops, restaurants, bars, parties, living rooms, kitchens, and workspaces. Most of the people I interacted with were in their thirties, white (in the Brazilian use of the term as "non black"), and upper-middle class. They resided in the central area of São Paulo, and save for a few exceptions, were social scientists or creative professionals. They were also left-wing sympathizers, but not members of any political party or organization.

My fieldwork in these settings came with the challenge of identifying and observing a sufficient number of analytically relevant situations in a very short period. My ability to do so

[57] I understand "everyday life" as the thread of concrete social experience that enters and gets out from specific social domains as people wake up, leave their house, work, eat, get out from work, relax, sleep and wake up again.

was unavoidably limited by the irrepeatability of time, my incapacity to be in different places at once, and the fluidity of social situations characteristic of a major metropolis.

I addressed these challenges by making my fieldwork flexible. I readjusted it throughout the day (and the days) in response to the fluid conditions of the country's political environment, the changing schedules of my contacts, and the analytic interest of the situations I ran into. As a result, the analytically relevant situations I observed did not come at a steady, regular pace. Sometimes I ran into them when they were already ongoing or fading, or at the moment when I had to be somewhere else. In some periods, a lot of time passed in-between them. In others, they stacked one after the other in ways that made it very hard to assess each one with sufficient time. These characteristics made the daily collection of situations I observed so diverse that at moments they looked as if they were not comparatively, but constitutively different from one another. For this reason, rather than analyzing each of these daily observations as representations of temporal stages of a causal maturation process, I examined them as an aggregate compilation of everyday situations.

It goes without saying that what I observed in my fieldwork cannot speak to how the victory of Bolsonaro on October 7 impacted São Paulo city dwellers—lest even Brazilians—at a general level. I cannot directly discuss how unique or generalizable the shifts I observed were. However, when possible, I also complemented my observations with data on social media usage and news consumption suggestive of how my observations might have been resonant with other people's political experiences after the election. I believe that the observational strategy I conduct in this investigation, despite its methodological limitations, holds, for theory-building, strengths over the methods with which evenemential influence has been traditionally explored.

## ENGAGEMENT WITHOUT ACTION: POLITICS AND THE EVERYDAY IN THE AFTERMATH OF JAIR BOLSONARO'S VICTORY

The fieldwork I conducted suggests that, politically speaking, the first four days after Bolsonaro's victory on October 7 can be characterized as a period of "engagement without action". As an introduction on what this represented, I provide first an account of what I observed on October 10th, the day that provided the most analytically rich set of observations during my fieldwork. After that, I discuss the main political features that characterized the everyday situations I observed along this period.

### *Inaction, Self-Censorship, and Exaltation: Wednesday, October 10.*

What we know now as downtown São Paulo is a mash-up of public squares, boulevards, occasional historic buildings, modernist masterpieces and non-descript skyscrapers lumped together in the undulating area that contained the whole of the city at the end of the nineteenth century, when the city was a provincial capital about to start becoming Brazil's economic engine—and a hundred years later, the center of a 24-million people metropolitan area. This area has undergone repeated cycles of abandonment and reoccupation that have turned it into a complex and multilayered area bringing together white-collar workers, buyers and sellers of low-cost commodities, middle-class residents, a small but visible population of creative workers, and a large population of homeless citizens. Three days after Election Day, on October 10, my fieldwork began where São João Avenue turns into a pedestrian walkway descending into the ancient riverbed that is now the Anhangabaú Park. On Monday and Tuesday, my fieldwork began earlier, at 9 AM. At that morning time, I had been hoping to see protest preparations or spontaneous acts of open political discussion at the moment

when downtown sidewalks become flooded with people rushing to their work. Considering how polarized the country was, and with the memory of the protest chants thrown into the city out of Ifigênia's apartment, I thought these situations would be relatively easy to watch. However, I was not able to witness any. As time went by and started to make Election Day a less current experience, I thought that the probability of observing these situations was low enough to justify starting my fieldwork that day an hour later, around 10 AM.

My observation journey began walking through a grid of streets that were then undergoing their daily transformation from sleeping shelters for homeless people to bustling shopping walkways. Like the days before, the only visible political elements present in these areas were in the newspaper headlines that hung in magazine stalls. The covers of all three of Brazil's reference newspapers, for example, were dedicated to news related to the electoral results of the October 7 election. Center-right *Folha de São Paulo,* referring to the finger-pointing that was occurring at the PSDB—Brazil's traditional right-wing party—after its electoral implosion, informed that "Alckmin insinuates that Doria betrayed him, and the tension within the PSDB increases." *O Globo,* also from the center-right, published that "Jacques Wagner negotiates with FH, Ciro and Marina," alluding to the talks that a PT politician was having with major political figures across the political spectrum to build a united front against Bolsonaro in the run-off election. Finally, *Estado de São Paulo,* a right-wing journal, published a story that went ahead of the electoral news cycle and jumped to report how the impending presidency of Bolsonaro would look like: "Four generals coordinate Bolsonaro's government program."[58]

[58] All translations are my own. Geraldo Alckmin was the PSDB's presidential candidate in the 2018 elections. Doria, then mayor of São Paulo, was the party's nominee for the governorship of the state of São Paulo. FH refers to Fernando Henrique Cardoso, a former president and the most important historical figure of the PSDB. Ciro Gomes was the candidate of the *Partido Democrático Trabalhista* (PDT), a center-left party. He was the Haddad's main competitor within the left. Marina

I kept walking towards the esplanade in front of the São Bento convent. Once I got there, I sat in a chair in a resting area overlooking the severe, Gotham-city-like façade of the convent. Some of my sitting neighbors were taking sunbaths. Others were playing chess, taking a break from shopping, or waiting for shoppers to leave downtown to take back street spaces. None of them, however, was engaging in any form of political behavior. After twenty minutes, I walked two streets eastwards into the Antonio Prado square, a small and leafy bourgeois enclave that acts as the access point to the São Paulo stock exchange and many historic headquarters of Brazil's flagship banking institutions. I entered a Portuguese bakery meticulously decorated in the bland style of smart design, ordered an egg tart at the counter, and spent a good half hour slowly eating it and taking a look at the crowd that surrounded me. The word "Bolsonaro" frequently crossed the air, but I could not identify any thorough conversation about him or the election results. This buzz was, however, the strongest kind of political act I observed that morning.

About half an hour after noon I began walking westwards to have lunch with Paulo, a graphic designer in his mid-thirties. I had not seen him since the day before the election, when he and I tried to attend a Haddad rally on Saturday. On my way to meet him, I observed the first purposive political behavior of the day. In front of the Municipal Theater, an overcharged Belle Époque building, a middle-aged man with a PT cap on his head was shouting rallying cries against Bolsonaro under the utmost indifference of the passersby. A man and a woman younger than him were also there. They were almost barricaded behind a small plastic table with materials from Haddad's campaign, waiting without much illusion for people to get close by. Nobody approached them before I decided to do so a few minutes

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Silva was a centrist environmentalist that had experienced several bursts of national popularity after her tenure as Environment minister during Lula's second administration. In 2018, she was the candidate of the centrist REDE party.

after. I asked them how many people they had talked to since they had set up the table. "Not many", the young man mechanically replied without much interest in pursuing conversation. I then asked if I could take a sheet of Haddad stickers. He said it was fine without much excitement. I took one and left to meet Paulo for lunch.

We sat on the terrace of a restaurant overlooking the Mário de Andrade library and the park surrounding it. I took the stickers out and without much thinking put one on my t-shirt and another on Paulo's. We asked for the menu and started catching up.

Our conversation revolved mainly around the elections. We talked about their shocking nature, how hard it was not to be thinking about who had decided to support Bolsonaro, how surprising it had been not to have seen reactions to the election in the streets, and our fear that Bolsonaro's victory heralded the return of conservatism into the city. Our conversation was agile, but it was eventually obstructed by a string of monosyllabic responses from Paulo, who started to throw increasingly frequent gazes at other restaurant-goers.

"Is there anything happening?" I asked.

"Do you see how they're looking at us?"

I took a look around. As far as I could see, nobody was watching us. "It all looks normal to me", I replied. We kept talking normally but at a lower tone. The rest of the meal went by without disturbances, although Paulo kept expressing a certain discomfort. Every once in a while, he turned his head again to take a look to what was going on at the restaurant.

After having lunch, we took a walk around the streets I had walked by earlier in the day. Like all other weekdays, that afternoon was full of people coming and going around the

shop fronts and entrances of office towers. As we were walking, Paulo suddenly threw the same interrogation he had made in the restaurant:

"Do you see how they're looking at us?"

I still could not see what he was seeing. In the crowd, I found it even harder to figure out what exactly he was referring to. Soon after, however, it became crystal clear when he took the Haddad sticker out of his t-shirt and threw in the first trash bin he ran into. His action made evident that the sticker had made Paulo feel politically exposed.

We then entered a small bodega to buy a bottle of cachaça. When I paid, a woman sandwiched between an old office desk and stacked beer boxes noticed the Haddad sticker I still had on my t-shirt. "I also like what that guy's saying," she said to me with a complicit smile. "It's great to hear that", I replied. "It's nice to see not everyone in this city voted for Bolsonaro". I asked her if I could leave the rest of the stickers I carried with her so other people could take them. She agreed, and I thanked her. Once back in the street, Paulo and I said good-bye a little bit after.

I spent the rest of the afternoon walking and doing errands. I picked up a pair of sneakers from a shoemaker store. While waiting, I asked the business owner if he had not felt things differently after the election. "If you say so," he circumspectly replied. I then walked a few more steps and entered a pharmacy ebullient with tired white-collar workers that had just finished their workday. When I finally made it to the cash register, a black woman began talking to me as she passed the things I had bought over the barcode scanner. "So you're not being fooled either, huh?" she said while looking at my sticker. "That Bolsonaro guy is just cheap spectacle. I think people will regret supporting him very soon". I told her I just wished that regret would happen before the run-off election. I wished her luck and went back to the street.

On the way back to my apartment, I received a text message from Paulo. Attached to it was a cellphone screenshot of Grindr, a popular application for gay encounters. The image had been taken a few minutes before. It showed a dialogue box that hid the grid of user pictures that usually popped out when someone opened the app. The box was succinctly titled "Warning." A brief but telling message followed suit: "After the recent election, members of the Grindr community expressed concerns [about the] increase in violence. Take the necessary steps to remain safe this week."

In the evening, I took a car ride service to have dinner at Paulo's in the neighborhood of Sumaré, close to Avenida Paulista. My driver turned out to be a full-frontal Bolsonaro supporter (or "bolsonarista"). He spent a good chunk of time proudly explaining my travel companion, a Peruvian tourist, how the "Myth," as Bolsonaro was sometimes known among his supporters, would finally get rid of all the thieves in the government and kill those in the streets. I tried to have a conversation and temper his excitement by reminding him of the notable achievements that Brazil had undergone during PT administrations and the peril of engaging in summary executions. My intervention was unsuccessful to the point of being interrupted by the driver. He repeated almost literally Bolsonaro's talking points, boastfully saying that the only thing he cared about was seeing streets "clean from thugs." He also said he was happy to know with certainty that after Bolsonaro became president, "rapists would be castrated, and thieves resisting police shot."

The rest of the ride went by silently.

It took me about half an hour more to get to Paulo's place. After letting me in, he rapidly went back to the kitchen. He was finishing cooking in the company of Rafael, a visual artist, and João, a fashion designer. They were gay and in their thirties. I had met both the Saturday before the election, hours before the polling booths started to open. That night,

stories about the election had sparked only intermittent comments—expectedly so, as it was understandable that nobody wanted to talk about an issue everyone in the party probably considered depressing. Most—if not all—times, these small bursts of political attention were channeled into their specific "group styles" of talking (Eliasoph and Lichterman 2003). Every once in a while, however, someone referred to the topic speculating on the political positions of friends in common or throwing some lackadaisical comments about how rapidly Brazil had become seduced by the extreme right.

Five days later, at Paulo's, Rafael, João, and the host engaged with the election in very different ways. They literally could not stop talking about it, and did it in a heated and overexcited way full of screams, interruptions, and overlapped talk. They repeated, again and again, stories of violence and harassment related to the election. They meticulously dissected, for example, the story of a bolsonarista attack that had ended with a swastika carved out in the belly of a leftist woman in the Southern city of Porto Alegre. Rafael also shared how an acquaintance had been subjected to homophobic slurs downtown in the middle of the day. João, on the other hand, told us about a tense argument the day before in the Copãozinho, a popular restaurant downtown, between a leftist client and a bolsonarista server.

My dinner companions appeared completely taken by these stories, avidly perusing them a first, second and a third time in a manner that even they came to recognize as exhausting. "That's enough, guys," Rafael said at one moment as if he had been brought back to reality by an exorcism. "Can we just try to stay sane for a while and stop talking about the elections for at least 5 minutes?" Everyone nodded in agreement, and silence finally interrupted the screams and hasty words. A few moments later, however, the truce was broken by an interrogation thrown by João as we were finishing eating: "How much time do you think will pass before they also start shouting at you in the street?"

*Taking Politics Personal: Political Evacuation and Experiential Proximity*

Many of the situations I observed (or did not) on that Wednesday illustrate several important characteristics of how politics manifested itself (or not) in everyday situations in São Paulo in the aftermath of the presidential election of October 7.

The most straightforwardly visible was, not without irony, the absence of political action in public spaces. In the first four weekdays before the election, I witnessed three political acts staged by non-activists. A week later, the dreary stall of PT militants where I picked up Haddad stickers became the only open political act I observed in the first four days after the election. Judging by what I saw on the streets, public life in São Paulo appeared utterly numb to the political watershed that Bolsonaro's victory had provoked. Brazilian reference journals—*Folha*, *O Globo*, and *Estado de São Paulo*—had equally mild reactions. In my fieldwork period, less than half of their headlines presented the election results as extraordinary. This trend was even stronger in tabloids. There, the elections were a cover topic only twice.[59]

In light of these non-observations, the term "evaporation" that Nina Eliasoph (1998) uses to describe how political apathy develops in the everyday life of middle-class U.S.

[59] Examples of headlines that made emphasis in the extraordinary nature of the election were, for example: "the stock exchange soars after the conservative wave in the new legislature" (*Estado de São Paulo*, October 9); "Bolsonaro and Haddad commit to respect the Constitution of 1988" (*O Globo*, October 9), or the October 10 headline news of *Folha de São Paulo*). Headlines that presented the election as normal covered issues like government formation or run-off election polls—for example, "Bolsonaro and Guedes recruit executives for their administration" (*Folha de São Paulo*, October 9); or "Datafolha: Bolsonaro has 58% and Haddad 42%" (*O Globo*, October 11). In the tabloid press, most headlines during my fieldwork covered crimes, soccer, or practical professional advice. For instance, "The falsifier of cheap clothing strikes again" (*Súper Notícia*, October 11), or "Insured people have until October 21 to ask for delayed payments from Plano Real" (*Agora São Paulo*, October 10).

citizens seems like an adequate term to describe the political atmosphere that prevailed in São Paulo after October 7. There were, however, some important differences.

In São Paulo, political action's disappearance did not go hand in hand with general apathy. On the contrary, in the time of my fieldwork the election entered more frequently into public conversations and more strongly in moments of interaction between personal contacts.

In public spaces, this increase was particularly notable in arm's length transactional interactions. In the first four weekdays before the election, I engaged in 17 such interactions, and none of them addressed politics. One week after, as my interactions with the women in the pharmacy and the bodega show, political talk started to "leak", so to speak, even into these instrumental interactions. From the 26 transactional interactions I counted having then, 5 of them ended up carrying political content. In one instance—my car ride to Paulo's—this kind of interaction gave way to a lengthy political conversation.[60] In absolute terms, these politicized situations were still a minority of the transactional interactions I engaged in during my fieldwork. But at the same time, they represented a substantial increase relative to the absence of politicized transactional interactions I observed in the week before the election.

The election and Bolsonaro were frequently talked about in other types of public interactions, but they appear to have led to brittle discussions. In coffee shops, restaurants and bars, I overheard these subjects, but they appeared in fickle and short-lasting snippets of conversation rather than in long deliberative conversations. In the morning of October 8,

[60] There were interesting interactive differences between bolsonaristas and leftists in these types of interactions. The interventions of bolsonaristas tended to be longer. They were also more prone to lead to substantive deliberations and were less likely to demand political homophily. Leftists' interventions were shorter. They were exclusively catered to express solidarity, and seemed to appear only after people were certain of having a minimal ideological affinity with another person.

for example, I heard a magazine vendor caustically telling an acquaintance "it would fun" to finally see Bolsonaro "kill the fags" ("*matar viado*"), making obvious reference to the intense, open and widely known homophobia of the winner of the election. His colleague reacted by nodding in agreement. A few moments after, they effortlessly switched to talk about soccer matches.

In personal interactions, the change was more qualitative than quantitative. The number of times the elections became a conversation topic was already high before October 7 and continued to be so in the week after. What was new was how preponderant this topic became after the election. In the week before the election, I witnessed only one conversation where the election became a focal talk point. In the week after, it was the protagonist—sometimes the only one—in all but one conversation.

On October 9, while hanging out in a museum coffee shop, Clarice toggled back and forth with two friends about maternity issues, moving out from São Paulo, the future of state patronage for the arts under Bolsonaro, and friendship and family distancings provoked by political disagreements and the results of the election.[61]

The following day, as I described at the beginning of this section, I observed how both little and big stories and gossips about post-electoral violence and harassment took command of the dinner between Paulo, Rafael, and João.

On October 11, I went to a get-together in the apartment of Francisco and his partner with Paulo and two female friends in the neighborhood of Pinheiros.[62] The conversation meandered around inside jokes, stories of sexual conquests, and wholly

[61] Clarice was an architect and a recent mother in her thirties. Her friends were a heterosexual couple working in the film and advertising industry.

[62] Francisco was a makeup artist of around forty years. His partner was an artist of about the same age. The older woman was a retiree and a neighbor of Francisco. The younger woman was black, in her late thirties, and worked as a manager in an association promoting the use of solar energy.

unexpected and abrupt turns towards the election. One of them began when Francisco stormed into the living room, interrupted a juicy love story that one of his friends was telling us, and anxiously shouted how in June homophobes assaulted a friend in Higienópolis, a high-income neighborhood not far from downtown. His interruption led without any resistance to a political detour that went uninterrupted for more than an hour. This shift made visible with striking clarity the amount of interest and concern that all the people in that living room had about the state of Brazilian politics behind the façade of political disinterest they were trying to perform, evidently without success. At some other moment, the woman whose love story was interrupted gave an impressively well-informed assessment of Haddad's tenure as mayor of São Paulo, and an equally well-thought discussion of the structural violence that black Brazilians face in their lives.

Finally, the next morning—at the very end of my fieldwork—I had breakfast with Emilia, an expatriate architect, and her partner Franco, an engineer. Considering that I had not seen Emilia in many years and had not met Franco yet, I expected our breakfast would contain a good chunk of personal catching up. But to my surprise, the election was pretty much the only theme we talked about in the two hours that we were together.

Overall, these situations indicate that the evaporation of political action that took place after October 7 was accompanied by a political "condensation" in conversations between personal contacts. Politics had been evacuated, so to speak, from action to interaction.

From an interactive standpoint, this switch provoked not only changes in the frequency with which my contacts talked about the elections in their daily lives. It also changed how they talked about them. Politics stopped being "far from home." Overnight, it began to be felt very close—sometimes too much so— to their tangible daily experiences.

Sometimes my contacts were the ones that brought the election to their direct daily experiences by engaging in speculations about the political leanings of the people they saw or interacted with. For example, Pedro, an architect in his mid-thirties, told me that the day after the election, he could not stop thinking who among the passersby he saw in the streets had decided to support Bolsonaro. He became so flustered by these speculations that he chose to spend the rest of the day alone after lunch. Similarly, Jorge, a film student in his early twenties, mentioned that in the days immediately after the election, he decided to stop small talking to a bus driver he had befriended for fear of confirming he had voted for Bolsonaro.[63]

The many episodes of mental and even physical discomfort attributed to the election are additional evidence of the proximity with which politics began to be felt after October 7. For example, Pedro described how time slowed down the Monday after the election, as he repeatedly failed to shut down thoughts about it over the day. Bernarda, an artist in her early thirties, told me in a telephone conversation in mid-October that she had been unable to sleep more than three hours since Election Day because she had been having constant nightmares about Bolsonaro's triumph —in fact, electoral nightmares after October 7 appear to have been sufficiently widespread to merit writing op-eds on the matter; see Zuker and Zeytounlian 2018. Lina, a middle-aged public official, told me she woke up with intense back pains the morning after the election, which she attributed to stress from the election results. She also mentioned having had trouble sleeping, which most of my contacts also reported having experienced.

Another evidence of how close the elections began to be felt after Bolsonaro's triumph concerned the type of news, stories, and conversations that were most attended

[63] Jorge shared this information after I asked him on the matter in the last week of October 2018.

about it. Information on public policies or the electoral campaign received very little attention. By contrast, people engaged constantly in meetings and conversations with stories of harassment and violence.

Some of these stories came by word of mouth. I came to know of an altercation between a bolsonarista and a left-wing sympathizer in a downtown restaurant through Rafael, who learned about it through texting with friends. Another example is a harassment episode I heard from a black woman in her early thirties as we were descending an elevator. She told me that a few days after October 11, a friend was jeered with racist slurs at the square in front of the building where she lived by a group of young men driving a car with Bolsonaro stickers.

My contacts also became familiar with other harassment stories through social media accounts that began compiling instances of intimidation or violence. One that was particularly popular was an Instagram account tellingly named *elenãovainosmatar*—literally, "he [Bolsonaro, it goes without saying] will not kill us." It was created on October 9 and published stories of acts of violence and intolerance (especially against the LGBTQ community) that occurred after the election.[64]

Finally, another source of information on acts of physical and symbolic violence was social media and online news outlets, which regularly published extreme stories of violence and harassment in the first post-election week. One of them was the swastika incident I was told about by Paulo, Rafael, and João. It became a conversation topic in all but one personal interaction situation during my fieldwork. The incident occurred on October 9, a few hours before I joined Paulo and his friends for dinner. The victim was a young woman who was

[64] Other examples of these repositories were *vitimasdaintolerancia.org*, an open Google maps titled *Violência Política no Brasil*, and *mapadaviolencia.org*. Until May 2019, the first two were still active as webpages.

attacked by Bolsonaro supporters in a public transit bus after they confronted her over the *Ele Não* stickers she carried on her purse. Together with a striking image of the incident, the news of the attack was initially published on the Facebook page of a local reporter and began to be vertiginously disseminated by major online media outlets immediately after (*El Pais* 2018). Another news that received significant attention was an op-ed in *El Pais*, a major online journal, on October 11. It discussed how the election had unleashed a wave of psychological aggressions against left-wing sympathizers and people carrying social identities associated with it. To support its argument, it included a psychoanalyst's description of a verbal aggression one of her patients had suffered in the days contiguous to the election:

> What's up, fucking little faggot [*viadinho de merda*]? Have you looked at the polls? Enjoy walking hand in hand [with a man] until the 28th. When the Myth takes office, all this faggot behavior [*putaria*] is gonna stop and you will be hammered until becoming a man (Brum 2018).

In this news, and in the stories that I heard talked about in my fieldwork, the protagonists were not soldiers, party bosses, nor militants. They were not politicians, activists, or legislators either. Bolsonaro himself was only a contextual character. Instead, they were ordinary citizens that appeared to follow Claude von Clausewitz famous aphorism of continuing "politics by other means." They certainly had not done so by waging war, but by executing politically motivated acts in their free time. By doing so, these actors were ambivalent about the social domain they pertained to. They had a foot in the domain of politics and another in the world of the everyday. The basis of their actions was their adscription as Bolsonaro supporters, but the setting where they executed them was connected to their personal experience. They acted in their spare time, and in the sidewalks and parks my contacts and many other people regularly used.

Last but not least, one final evidence of how much the election results were perceived as a personal experience was the widespread development of sentiments of fear and worry, and the readjustment of social behavior that emerged out of these feelings. For example, in an overt act of self-censorship, the fear of retaliation from passersby made Paulo stop performing one of the softest kinds of political action possible: wearing a sticker on a t-shirt. Similar concerns informed Francisco's demand, wrapped in the form of a gentle insistence, that I should use a taxi to go back home for fear something might happen even if I was staying in a few blocks away from his house. But the most concise articulation of these feelings I witnessed came while chitchatting in an elevator on October 26, when a black woman in her thirties shared how the days after October 7 felt to her: "like a gun pointed at your head".

In sum, the first four days after October 7 were characterized, first, by an evaporation of political action, and second, by a densification of political interaction. Jointly, these processes describe an environment of political evacuation where political engagement was transferred from action to interaction. Third and finally, those days were also marked by the development of sentiments of malaise, exposure, and vulnerability.

Although more research needs to be conducted on whether these modifications might be representative at an aggregate level, available information on electoral media coverage, social media activity, and consumption of online news suggests that the features I observed might at least not be exceptional.

In consonance with the lack of public political acts I observed, news from both traditional and online media, for example, did not report instances of social protest (or celebration, for that matter) in the first week after the election. This lack of reporting was in stark contrast with the intense coverage these outlets gave to protests and demonstrations in

the week prior to the election. This shift suggests that few instances of collective action occurred during my fieldwork period.

To have a minimum grasp of whether the election was perceived as a personally proximate shock, I identified and counted the number of retweets of news published in *Folha de São Paulo* and *El Pais* on the relationship between the elections and mental health—a news genre that is (or was) seldom seen in major journals. I counted 346 tweets with that made direct reference to stories written on the matter during my fieldwork at most three days after being released. This number was more than three times higher than its equivalent figure the week before the election. Among this news, the one that was most widely circulated was the op-ed that included the psychoanalyst's testimony I quoted some paragraphs above. Links to this occurred 331 times on Twitter the first three days after its publication. This figure is comparable to the number of retweets that run-off election polls conducted by *Folha* and *O Globo* received that day.

Finally, there is evidence that Brazilians massively consumed news on violence during my fieldwork. The Instagram account *elenaovainosmatar* already had 66,000 followers twenty-four hours after it was created. By Wednesday, this number was 88,000 (Mena 2018). The swastika incident in Porto Alegre also received a high degree of attention. On Facebook, it was shared 10,000 times the first hours after its first publication, and it was tweeted about more than 329,000 times the day it occurred (Sala de Democracia Digital 2018a). The pop-up message on Grindr also suggests the scale at which unsafety became a widespread feeling in some communities. For a broader left-wing public, there is evidence that instances of violence were at least massively attended. Available calculations, for example, suggest that in the evening of the election 3,200 tweets on political violence per minute were published. By October 11, 2.7 million tweets on this subject had been poured into this platform (*Ibid*). The

number of tweets on the matter in those three days alone was 300% larger than those published during the *thirty* days prior to the election (Sala de Democracia Digital 2018b). On October 11, tweets on violence amounted to the combined number of messages in this platform about campaign issues related to public safety, corruption, economy, health, and education (Sala de Democracia Digital 2018a).

## TAKING IT PERSONAL: POLITICIZATION OUTSIDE POLITICS

Earlier in this investigation, I presented three theoretical approaches to understand how historical events shape the relationship between politics and everyday life when they occur. How well does each adjust to the political shifts I observed occurring after Bolsonaro's victory?

Because a steep electoral defeat and an impending extreme-right government diminished the expected benefits of dedicating time and attention to politics, a rationalist approach to evenemential influence would have expected decreases in left-wing collective action and political engagement after October 7. Since this approach also assumes that changes in political attributes are not at play when an event occurs, it would also expect that the election results had not provoked major shifts in how people related to politics.

These expectations did not fit what I observed in São Paulo during my fieldwork. There, a decrease in political action was accompanied by substantial increases in political engagement and broad cognitive and perceptual changes in the way people related to politics.

The politico-cultural approach to evenemential influence taken by historical sociology, on the other hand, adjusted to my observations only partially well. October 7

generated cultural changes indeed, and some of these changes were fully contained within the domain of politics. But these changes received the least attention and provoked the fewest reactions. I heard very few conversations striving to make a diagnosis of what had made someone like Bolsonaro win an election in the world's fourth-largest democracy, or discuss future political action strategies even with a looming run-off election. Most of the talks I witnessed reacted to the immediate effects that the election produced on streets, parks, and squares. From a properly political standpoint, these consequences were of lesser importance than the policy and governance changes that the election results announced, but they were also the most relevant for my contacts' personal experiences. These conversations did not occur in formal acts of public deliberation between left-wing militants either. On the contrary, they developed in times of leisure between friends that met with the nominal objective of just having a good time. For the people whose interactions I observed—which, like many other Brazilians, lacked pre-determined moments of political instantiation in their everyday routines—the domain of politics continued being weakly instantiated into social experience.

However, the inapplicability of politics as an experiential lens did not prevent my contacts from undergoing profound political behavioral and perceptual changes. But these changes were not triggered by cultural changes within the domain of politics. They were provoked by how Bolsonaro's victory infiltrated political stories into the "personal domain"—that is, the domain of social experience related to individuals' self-understandings as distinctive, temporally continuous, physically concrete, and biographically consistent social agents. In this way, the results of the election seemed to be generating political reactions in a connective way.

Understanding this process of connection begins by identifying how some political ruptures provoked by Bolsonaro's commanding victory were able to act as bridges between the political and the personal domain.

My observations suggest that this process began with how the election crushed long and widely held assumptions about the size of popular support for the left and right. Overnight and without the right to appeal, the left had to relinquish its majority condition and give it to a right-wing block dominated by its most virulently anti-leftist strand.[65] And since in a liberal democracy political electors are also restaurant-goers, joggers, neighbors, and coworkers, this sudden change in what the "political majority" meant after October 7 created an opening through which this event began to impact identities and network perceptions from domains outside of politics. The election results became embodied not only in the figures of Haddad and Bolsonaro or in the bodies of professional politicians, but in those from individuals seen by my contacts in their everyday life, and some of whom were responsible for the acts of aggression that many of my contacts vertiginously talked about *ad nauseam*.

But where did this intense spike in the attention to political violence come from? Did it just merely reflect factual increases in the numbers of these unacceptable, worrisome acts?

Future investigations should thoroughly evaluate this proposition. However, using available data, I was unable to find much evidence supporting that the growth of violent acts was as steep as increases in conversations about them. All of my contacts, for example, knew of violent acts, but none witnessed any directly. They knew them by word of mouth or through social networks and online media. Some of the key stories of violence that my

[65] I do not mean that this specific breaking point was enough to generate the post-electoral reactions I observed. Had the PT lost the election narrowly, or with equal force against the moderate PSDB, its historic rival, reactions to the election probably might not have been as strong and abrupt as the ones I observed.

contacts shared had actually occurred long before the election—for example, the assault that Francisco's friend suffered in June. Others were later found to be false. A few days after our dinner, João told Paulo that he had found out that the restaurant altercation he had told us about at dinner had not actually happened. And another one, no more and no less than the swastika incident in Porto Alegre, turned out to have executed by its very alleged victim (see Gazeta do Povo 2018). Available quantitative data does not support that violence skyrocketed after Bolsonaro won the first round of the presidential election. Between September and October, for example, reports of acts of political or hate-induced violence increased by 31% in the state of São Paulo (Estarque and Faria 2018).[66] Even if all the reports of October were related to incidents that had happened during the period of my fieldwork, increases in reports of political violence in this period would have still been ten times as small as equivalent growths in tweets on political violence.

Did the sharp spike in talk of violence, alternatively, correspond to equally substantial increases in media coverage of these incidents?

Preliminary data in this regard does not support this contention either. During my fieldwork, the number of pieces of information on violence in *Folha* and *El Pais*, two leading Brazilian news outlets, was only superior by 8% relative to the last 39 days of the election campaign.[67]

These patterns suggest that the attention that politically related aggressions received during my fieldwork was not simply reacting to factual instances of violence or how frequently the press reported them. Instead, it appeared to be associated with attempts to make sense—or "control"—the sudden, impossible to ignore realization that it was more

[66] Ten percent of these reports were related to physical instances of violence. The rest were referred to as acts of verbal harassment.

[67] These numbers cover news or op-eds on everyday political violence. They exclude news on the murder attempt against Bolsonaro, which occurred at the beginning of September.

probable than not that someone would have preferred voting for a raw, outspoken extreme-right-winger.[68] This attempt at control resignified the persons my contacts shared their everyday spaces with. They passed from being anonymous and undifferentiated entities to becoming characters defined by political interests and motivations as political rivals, bolsonarista supporters, and likely executors of his intolerant agenda.[69]

The morphing of anonymous people into active bolsonaristas, in turn, provided a path through which political narratives became infiltrated into constitutive stories of personal domains of experience. This transformation began to politicize everyday social perception, first, by expanding the range of social situations that people explained explicitly through politics—a process I call "attributional politicization"; second, by increasing the number of situations characterized as pertaining to the realm of politics—which I refer to as "classificatory politicization"; and third, by increasing the expected frequency at which the actions of other people were expected to be driven by political objectives—which I name "motivational politicization."

Attributional politicization happened when social situations that were previously legible without being tied to politics began to be accounted for in relation to actors or stories from this field. It occurred, for example, when Francisco tied the homophobic aggression

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[68] In this regard, it was telling that the woman that carved out a swastika in her belly to accuse bolsonaristas of active political extremism decided to do so immediately after the results of the election, and not, for example, in the eve of it or the vicinity of the run-off election later in the month.

[69] A blatant example of this resignification in media was an op-ed published in the Brazilian edition of The Intercept a little bit after the end of my fieldwork period, on October 16. The op-ed was titled "There are bolsonaristas wanting a Crystal Night", making a not so subtle association between the aftermath of the Brazilian election and the political environment in Germany on November 1938, when the Nazi government prompted a wave of lynching instances against the Jewish community (Magalhães 2018). Three days later, an op-ed in *Folha de São Paulo* also made the same comparison (Rossi 2018).

My investigation cannot adjudicate whether news like the one described above was a cause or consequence of deep feelings of vulnerability that I saw being developed in Brazil. For the analytic purposes of my investigation, it is sufficient to understand this relationship as one of mutual relationship.

his friend suffered in June to the election. In my experience in Brazil and elsewhere, similar inexcusable acts are usually portrayed as emerging from socio-cultural dynamics stemming from processes of much longer duration than an election. But Francisco explained his friend's experience as if it had been an effect of the wave of violence he saw being provoked by Bolsonaro's victory. This explanatory choice was all the more striking as this incident happened in June, four months before the election, and when Lula was still comfortably leading the polls.[70]

Classificatory politicization occurred when new social situations became included in social situations ascribed to politics. An example of this happened when the woman in the elevator told me the racial slurs her friend had received in front of her apartment building. She understood her friend's experience as directly linked to the political context that Brazil was undergoing at that time, and to social identities that were unequivocally political. In her account, her friend's aggressors had not acted as young, men, probably white, or probably wealthy (or any combination thereof). The racist act they had made belonged fundamentally to the field of politics because they have done so as sympathizers of Jair Bolsonaro.

Finally, motivational attribution occurred when people re-estimated the capacity of the domain of politics to take control of social experience upwards. For example, by being fearful of being harassed for carrying a Haddad sticker after lunch, Paulo assumed that downtown in the middle of the day, it was very likely that somebody would bring forward his condition as a Bolsonaro supporter and confront him.

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[70] Here also the perceptual dynamics about materialized instances of violence seem to have little correlation with electoral dynamics. According to available information, violent deaths related to homophobia, one of the hate crimes less susceptible to underreporting, increased gradually from 2006 through 2017. In 2018, in comparison, the number of these acts decreased (Grupo Gay da Bahia 2019). Of course, the validity of this comparison rests on the assumption that violent murders are somewhat indicative of less extreme hate crimes. This assumption should be evaluated in more detail.

Together, these perceptual shifts made the election results seem and feel closer than they objectively were.

Many persons began to feel fundamentally vulnerable for political reasons. The election results began to be felt not as a political phenomenon, but as something that was ineludibly personal. It was from the feelings of personal vulnerability that emerged from that vantage point that both avoidance of political action and hyper-engagement in the terrain of interaction rose. They were not independent of one another, but two sides of the same attempts at stabilizing behaviorally and cognitively the contingency that the results of the election had provoked.

## CONCLUSION

Motivated by the increased attention that the relationship between contingency and social action has received in social theory (Berger and Luckmann 1966; Swidler 1983; Padgett and Ansell 1993; Sewell 1996; White 2006; Wagner Pacifici 2000) and by the emergence of a stream of major political ruptures and turning points around the world, works seeking to understand historical events and their tangible political effects have slowly but steadily increased in recent times. I join this body of work with this investigation. I sought to better understand the immediate political effects that an event produces when it occurs by studying the immediate changes that the victory of Jair Bolsonaro in the first round of the 2018 Brazilian presidential election provoked in everyday life situations. I did this by providing an analytical description of the political disruptions I observed it producing in public areas and in intimate interaction networks, and I used these observations to evaluate existing theories of evenemential influence and to generate new approaches to understand how historical

events produce a "politicization burst": an abrupt and generalized increase in behavior motivated or justified by political accounts.

My research diversified the repertoire of historical contingencies that have been studied from an evenemential perspective. So far, the relevant literature has mainly studied the effects that social contingencies related to social protests have had in social constituencies favored by them. My work complements these investigations with a research centered on members of political positions that were defeated in an electoral outcome.

My research also departs from previous studies in how it researched the relationship between events and politicization. I had the rare opportunity to conduct ethnographic fieldwork in real time on the everyday political consequences of Jair Bolsonaro's electoral victory. I conducted my fieldwork in public places and in moments of socialization between left-leaning close friends. Whenever possible, I complemented these observations with aggregate information indicative that the political shifts I observed might also have been experienced by other people.

In interactions in public spaces, I observed a near complete absence of political action. Had my research been focused only on these situations, I would have probably concluded that Bolsonaro's victory had occurred without having any major consequences. On the other hand, in moments of intimate socialization between personal contacts, I saw that his triumph became a dominant and heated topic of conversation. If my fieldwork had been catered solely on this type of situations, I would probably have concluded that the electoral results of October 7 had generated a wave of concerning and notoriously visible political action in the streets of São Paulo.

However, by conducting fieldwork on both types of settings, I was able to see that the near complete absence of public political manifestations did not come hand in hand with

political apathy. On the contrary, it was paired with an impressive increase of political engagement that was channelized to the production of political investment in personal interactions. The immediate aftermath of October 7 was characterized as much by an evaporation of political action, as by an increase in the frequency of politicized exchanges in personal interaction networks.

My fieldwork suggests that this intriguing combination of engagement without action developed as a consequence of a chain of semantic, perceptual, and identity readjustments that occurred to make sense—or "control" —the electoral results of October 7.

This process originated in the abrupt way in which the election results snatched the Brazilian left of its traditional majority position, reassigning it to a right-wing block dominated by its most extreme strand. This change unleashed cognitive decompressions that required making new political stories and frames of reference to make sense of the new political environment this change had established, and identifying how this reality would affect people's experiences. These cognitive demands became manifest in the vertiginous consumption and strong personal reactions to news and stories of violence and harassment against leftists and people with social identities associated with the left, which provided a novel, compelling, and readily available frame to understand the political reality that was emerging after the election. This frame of reference also changed the meanings ascribed to the people my contacts encountered as they traversed the city. Before the election these persons ware barely experienced as tangible presences, but they were now felt as concrete politicized and politicizing presences.

The metamorphosis of everyday people from extras to concrete characters driven by bolsonarista motivations triggered in turn drastic but unexpected changes in the way people related to politics. As a perceptual domain, politics remained poorly instantiated, but other

domains of social experience started to become politicized. Politics was used more often as a way to explain specific social situations; more social moments began to be classified as political; and politics was imagined as a more frequent driver of people's actions. These changes started to make the environments my contacts inhabited be perceived as unsafe, and also changed the way in how my contacts imagined themselves within them. Their personal identities became tainted with a feeling of vulnerability that transformed the victory of Bolsonaro from being a political contingency to becoming a personal emergency. This transformation became the locus of the behavioral rearrangements I observed in my fieldwork: on the one hand, acts of self-censorship of political action, and on the other, a steep increase in political talk.

This cycle of semantic, perceptual and behavioral shifts challenges existing theoretical outlooks to evenemential influence.

Against the expectations of rationalist theories, the victory of Bolsonaro on October 7 provoked a retraction of political action that went together with an increase, not a reduction, of political engagement. It also seemed to have created a muddier rather than a clearer environment for political decision making.

The political shifts that October 7 produced did not align well will politico-cultural theories of evenemential influence either. The ruptures "for politics" generated by the election produced few reactions and received scant attention in comparison to "bridging ruptures" that connected the results of the election with non-political domains of social experience. These extra-political changes were the ones that ended modifying political behavior. My contacts *did* politics without experientially *being* in politics. They reacted to Bolsonaro's victory not by "taking it politically", but by "taking it personally".

These shifts suggest that the political shifts I observed after October 7 followed more closely the connective path of evenemential influences I presented in the theoretical section of my investigation. It had not politicized by expanding the application of politics as a frame of social experience. It had done so by infiltrating political stories and schemas into more frequently applied social domains and generating behavioral and perceptual political modifications out of these infiltrated domains.

In this investigation, my main objective has been describing, discussing, and producing an analytical account of the micro-sociological mechanisms through which the victory of Jair Bolsonaro on October 7 reconfigured the relationship between politics and everyday life right after it landed in the political reality of Brazil. By doing so, I sought to shed light on the issue, poorly explored in the literature, of how historical contingencies produce sudden politicization bursts in the societies where they emerge. I hope to have contributed to this objective by identifying and analyzing infiltration processes through which political stories gained a footing on extra-political domains and by introducing a connective framework to understand evenemential influence. But from a broader perspective, the observations and the theoretical discussions of my investigation also bring attention to new propositions that open new avenues of theoretical and empirical research to keep refining our understanding of historical events as entities with a saying in processes of political socialization and activation. I conclude this investigation by briefly introducing several in the paragraphs below.

*Events are bridges between social domains.* In the seminal essay that reactivated sociological attention to historical events as causally loaded entities, William Sewell characterized historical events as "dislocations and transformative rearticulations" of social structures (Sewell 1996, 861). If we were to distill the theoretical force of this

characterization further, my research suggests that an event's everyday influence might be less directed to the dislocations it produces than, interestingly, the connections they build between politics and more frequently instantiated domains of social experience. This process grants an event ability to produce political modifications without necessarily having to build the cognitive scaffolds needed to expand the application of politics as a direct lens of social experience. It is probable that the efficiency of this path of influence might play an important role in the construction of the intriguing capacity that historical events have to generate widespread political disruptions in "settled times" where politics is instantiated very infrequently as an effective domain of experience (Bennet, Fischer and Resnick 1995; see also Bearman and Parigi 2004 and Wyatt, Katz and Kim 2000), and where many people lack the cognitive scaffolding needed to recognize, process or react to political information when it is defined as properly "political" (Fiske 1983). At the same time, precisely because it circumvents strengthening political frames of experience, the engagement bursts that this "extra-political" form of politicization produces frequently fades rapidly over time.

*Different bridging abilities might explain heterogeneities in evenemential influence.* In a connective framework, analyzing the immediate, everyday impact of an event requires identifying which among the multiple ruptures in political reality produced by it acts as a bridge between politics and other social domains. Subsequently, it calls for identifying the type of political information these ruptures might infiltrate onto these domains, and then analyzing how these pieces of information reconfigure meanings and perceptions of networks in these domains. These tasks offer an organized framework to analyze everyday evenemential influences. And perhaps more importantly, they also generate useful discussions to start tackling the poorly understood questions of how different effects might have heterogeneous effects in the societies where they emerge.

For example, a factor impinging on the strength with which an event manifests itself in the everyday might be related to how many bridging acts it might be capable of performing. The more connections an event might generate between politics and other social domains, the more opportunities it might have to create stronger, more prolonged political disruptions for more people. The infiltrations I observed in my fieldwork were linked to the personal domain, but that does not exclude the possibility that other events might have the ability to bridge to labor or other domains.[71]

The capacity of a political contingency to exert more substantial effects might also be related to how easily it resonates with networks and stories of the domains it would be in principle able to bridge. After a continuous and deeply polarizing five-year-long political crisis, being a left-wing sympathizer in São Paulo had become by the end of 2018 an ascription sufficiently tied to personal self-understandings to act as the entry point through which Bolsonaro's victory gained access into personal domains of experience. If this had not been the case, it would have been likely that his triumph might have had a different impact than the one I observed.

Since the stories that shape social domains often change across different groups, one can expect that the connective influence of a historical event might also differ across them. Thus, studying the semantic points of connection through which events begin processes of infiltration also has implications for understanding social heterogeneities in evenemential sensitivity—an issue that remains understudied in the literature.

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71 For example, connections to labor domains might be important for contingencies related to strike waves, and those to domestic domains might have had a role in the way a sudden convertibility crisis in Argentina in 2001 led to banging pots and pans demonstrations by housewives that soon provoked a protest wave which led to the resignation of President Fernando de la Rúa in Argentina in 2001 (Cafassi 2002).

*The current media environments make historical events more likely to happen.* Steadily and forcefully transmitting a disruption is just as important as the structural ability to do so for the generation of evenemential influences. In this backdrop, media structures are fundamental in fostering or obstructing resources for the steady diffusion of whatever political ruptures an event might be able to transport onto other domains. In São Paulo after October 7, online media was fundamental for understanding Bolsonaro's victory as a collectively shared shock and in generating strong personal reactions to it. They disseminated continuous streams of political information that could be repeatedly retrieved and accessed anytime, at no cost, and without excluding others from also having access to them. Had Bolsonaro's victory occurred at a time where information was still disseminated through discrete, irrepeatable blocks —like TV shows—or through costly and non-shareable commodities—like newspapers—, it would probably have been less likely to be capable of generating intense abrupt personal changes. Thus, relative to the postwar period, the times that we currently inhabit might facilitate the emergence of historical events as its underlying media structure facilitates the generation of stronger and continuous infiltration processes.

My hope is that these final propositions might foster future discussions capable of yielding in our understanding of what historical contingencies do in a time where this research agenda has dramatically switched from being a theoretically motivated agenda to a substantively pressing line of research in light of the multiple political disruptions that keep accumulating in recent political experience.

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