The Impact of Historical Events on Politicization:
Comparative Quantitative Evidence from Western Europe, 1973–2002

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Abstract

Although a consensus has grown that historical events—unexpected, punctuated, and collectively experienced moments of political contingency—are important political socialization factors, these expectations are based on case studies that have verified only event effects on political attitudes and only a handful of research settings. To address these limitations, I conduct a quantitative comparative analysis, the first to my knowledge, of event effects on everyday political engagement. Using 68 survey data points from 1973 to 2002, I evaluate the performance of 34 concrete historical events on levels of everyday political discussion in Belgium, France, West Germany, Italy, and the Netherlands from 1973 to 2002. I test hypotheses from dominant generational theories of event effects, which see their influences on political engagement as positive, persistent, and stronger the more intense the political disruption they produced. I also offer an alternative "diachronic" outlook to event effects. This outlook sees the influence of an event in political engagement as eroding over time, positive or negative depending on whether it was divisive or unifying, and stronger the more effective an event was in changing state actions. I found supportive evidence for these expectations by testing the statistical association of cohort exposure to the events I analyze and political talk levels in 72 regressions per country. My findings suggest that events differ between them not in degree, but in their logics of socializing influence, and bring attention to their capacity to affect, beyond attitudes, more general orientations to politics.

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INTRODUCTION

How does experiencing abrupt moments of political contingency—or "historical events"—shape people's political trajectories? Studying this question has long held disciplinary relevance as a vehicle to study the relationship between contingency and social change and the role that history plays as a political socialization factor. And against the backdrop of unexpected populist victories, sudden protest waves, and political-sanitary COVID crises across the world, the study of this interrogation has also acquired renewed substantive saliency in current times.

Although the literature tends to agree in seeing historical events as capable of producing lasting effects in individual-level political attributes, we do not know with sufficient precision how differently and how much they persistently affect foundational political attributes. This gap in the literature is as much a consequence of the substantive focus of previous research on collective memory and issue positions as its analytical and theoretical choices, which have limited the conduction of comparative analyses on how events affect individual-level political characteristics.

In this investigation, I seek to contribute to specific knowledge on how, how frequently, and how much historical contingencies can generate lasting cohort differences in political engagement. By doing so, I intend to make a set of inter-related theoretical, methodological, and substantive contributions.

Theoretically, going beyond the contention that "events matter" as devices for political socialization, I develop an original set of hypotheses on event effects. Departing

from "synchronic" generational theories that see event influences as permanent and generated mainly when an event occurs, my hypotheses take a "diachronic" outlook to event effects. This take posits that the politicizing influences of events update over time in interaction with ongoing political conditions and life cycle maturation processes. Based on this view, I propose that the effect an event has on politicization fade with time, that its strength is associated with its capacity to lastingly change government action, and that its direction of influence is negative or positive depending on whether an event was polarizing or produced a rallying effect.

Analytically, I test these hypotheses by developing a comparative quantitative research framework to investigate event effects. This approach begins by conducting in-depth historiographical research to identify empirical instances of "historical events" in a specific period and polity. For each of these events, it then generates a variable that captures graded cohort influences on politicization under examination. Finally, it evaluates the statistical association of these variables with politicization in a way that controls for model selection uncertainty and cohort factors not related to event experiences. This design allowed me to use thirty years of survey data to evaluate the performance of twentieth-century historical events from Belgium, France, West Germany, Italy, and the Netherlands as predictors of frequency of political talk, an everyday behavioral measure of politicization and an important political conduct in and of itself.[1] This evaluation is based in the analysis of 72 regressions per country.

My results suggest that events are heterogeneous in the magnitude, direction, and robustness with which they persistently influence politicization. These heterogeneities'

[1] Political talk is important for processes of diffusion of political information, the formation of political identities, interpersonal political influence (Snow and Benford 1988; Steinberg 1999), and political participation (McClurg 2006; Klofstad 2010). It has also been related to democratic quality (Habermas 1984).

organization does not support generational hypotheses on event effects and align instead with the diachronic outlook to event effects I introduce in this investigation. My findings show that the robustness of an event's association with political talk across the regressions I analyze is associated with its recency and its capacity to have generated lasting turnarounds in state action. They also suggest that the direction of events' influence on political engagement is associated with divisive or cohesive societal reactions to a historical event.

My investigation is organized into five sections. The first reviews the state of the literature on historical events' role as political socialization factors. The second identifies existing hypotheses on event effects on politicization and introduces a new set of hypotheses on this influence. The third part discusses the research design and analytical strategy I used to test these hypotheses, and the fourth examines the results of my analysis. 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—an analytical outlook I will call "evenemential" (Sewell 1996)—comes from investigations that fall under the rubric of "generational research". The theoretical cornerstone of these works is *The Problem of Generations,* written by Karl Mannheim almost a hundred years ago (Mannheim 1952 [1927]). According to the dominant reading of this classic essay, in this text Mannheim proposes that the eruption of a historical contingency unleashes processes of "generational imprinting" in cohorts who are coming of age when

they occur, making them develop political attributes connected with the specific characteristics of the contingency that is taking place.

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 political situations they experienced when they were young (Zeitlin 1966). In the seventies and eighties, the heyday of generational research, many other investigations began producing similar findings. Most of them came from research that investigated the political legacies of the sixties in the United States. These investigations found that people who entered adulthood in this decade carried distinctive political attitudes, which they associated with the historically turbulent context they came of age in. Two works from this line of inquiry stand out for their comprehensiveness. One is Jennings and Niemi's analysis of longitudinal data on the political attributes of 1965 high school seniors and their parents (Jennings and Niemi 1981). They explored how the political orientations of these young people evolved over time and how this variation diverged from their parents'. The other one is Delli Carpini's examination, using twenty-eight years of National Election Surveys (NES) data, of how the political characteristics of the "sixties generation" differed from the rest of their fellow Americans (Delli Carpini 1986). Both works found that people who came of age during the sixties exhibited particularly distinctive characteristics related to subjects that organized the political debate during the sixties—for example, school desegregation.

Since the seventies, processes of generational imprinting have also been identified for party identification. In this decade, cohort-based analyses began to make use of repeated cross-sectional data (typically, the NES surveys) to show that party adscription was patterned at least as strongly by birth year as by age, which was then the temporal factor most

frequently used to understand people's partisan preferences (Glenn 1972; Abramson 1979). During this time, this body of work did not explicitly relate 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 more vocally cohort variations in this political orientation to differences in the historical occurrences they experienced during young adulthood (Osborne, Sears and Valentino 2011; Bartels and Jackman 2014; Ghitza and Gelman 2016).

Collective memory research has also generated results supporting generational imprinting processes. At the end of the eighties, Harold Schuman and Jacqueline Scott analyzed open-ended surveys that asked people to name the historical events they considered most important. Their investigation found that historical developments experienced during young adulthood were better remembered and more likely to be considered as particularly relevant (Schuman and Scott 1989). Since then, they and 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 key to establishing an agreement in the literature seeing historical events as important long-term devices for political socialization. Against the backdrop of the epistemological presentism that still tends to guide many instances of social research, this is no minor accomplishment. However, there are still important gaps in our knowledge of how experiencing a historical event affects political traits over time.

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. However, 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 people from these cohort segments have a higher sense of political efficacy and give more weight to politics than their parents (Delli Carpini 1986; Jennings and Niemi 1989).

Besides people that came of age during the sixties in the United States, we also don't know much of how historical events affect people in other times and contexts. A large majority of generational investigations oriented to the analysis of political attitudes remain dedicated to the study of the "sixties generation" in the United States (Cutler 1974; Miller 1992; Jennings and Markus1984; Jennings 1996). This lack of diversity in research contexts impedes gauging whether the findings of these investigations can be generalized to other events or if they are idiosyncratic of the historical experiences of the American sixties.

Finally, besides knowing that events "matter" politically for those who were coming of age when one occurred, more precise knowledge on how, how frequently, and how much they do so has yet to be produced. So far, the literature remains focused on verifying whether event effects exist rather than researching heterogeneities of political influence among them.

In light of this context, conducting a comparative investigation on the effects that events have on a foundational political attribute like politicization appears to be a productive direction of research. It should also serve as a motivation to address 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* (for example, relative influence; see Davis 1975; Inglehart 1981). A plausible explanation of the distinctiveness of the "sixties' generation" can be built as much around the historical ruptures it experienced during young adulthood as in connection with the social conditions that prevailed when they came of age—for example, increased affluence, or higher rates of educational attainment level.

Second, generational investigations have been lax in selecting the historical phenomena they use to test imprinting processes. Most voices in the literature—Mannheim included—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 generational literature evaluates imprinting processes in connection with very wide historical periods—like the "Italian fascism" or the "Weimar republic" periods (Barnes 1972; Weil 1987)—or with nebulous contexts 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 relating their analysis 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 John F. Kennedy's murder (Schuman and Corning 2012) to a midterm election in the 1950s (Bartels and Jackman 2014).

Third, generational findings regularly stem from quantitative analyses where the imprinting capacity of an event is measured by a dummy of cohort exposure to an event that separates cohorts that came of age when it occurred from the ones that didn't. This analytical choice is convenient, but it is inevitably premised on assuming that only young adults are historically sensitive. This contention is seldom explicitly discussed, commented or justified. In fact, most generational investigations dedicate little time to theoretically discuss the imprinting processes they focus on. They reference Mannheim's *The Problem of Generations* for more information and rapidly proceed to conduct empirical analysis. However, because Mannheim's essay is not primarily concerned with discussing the socializing influences of historical events (DeMartini 1985), it does not provide a fully developed theory of how generational imprinting processes actually work.

To sum up, in the current state of the literature, producing concrete and inferentially sound knowledge of event effects on politicization requires also giving sound theoretical foundations to particular expectations on how political contingencies affect this behavior, and developing a comparative research design capable of robustly examining them empirically. I undertake the first task in the next section and engage in the second in the one that follows.

THEORIES OF EVENT EFFECTS ON POLITICIZATION

As noted in the preceding section, the relevant literature has not yet produced conclusive results on whether historical events can persistently shape levels of politicization. I begin discussing theoretical outlooks to these influences by signaling two plausible arguments skeptic of them.

One of these arguments can be made by pointing out that life-cycle events and family and school political socialization processes tend to run largely independently from historical conditions. To the extent that these processes have been repeatedly found to be key factors in shaping political engagement in the long run (Plutzer 2002; Neundorf, Smets and García-Albacete 2013; Smets 2016), one would not expect history to have a direct socializing force in terms of politicization.

A similar position can also be maintained even after recognizing historical forces as political socializers. This argument has also been articulated, sometimes in a surprisingly strong-worded way, by influential voices in the discipline of history. Fernand Braudel, an important figure of the Annales historiographical school, famously characterized events as "surface disturbances, crests of foam that the tides of history carry on their strong backs" (Braudel 1980, 3). Within the literature on political socialization, Inglehart's post-materialist theory, which associates levels of political engagement with levels of social affluence when a generation came of age (Inglehart 1981, 884), echoes this skepticism —albeit in a more subtle manner—by suggesting that the main historical carriers of lasting political influences are socialization *contexts* and not historical *contingencies*.

Generational research, on the other hand, posits that historical events exert lasting political influences for those who were young adults when they occurred, who at the time combined nascent political interest with lack of political experience. These investigations argue that when a historical event occurs, these "impressionable" citizens modify their political attributes in a manner that captures key characteristics of the political environment that the event produced (Mannheim 1952 [1927]; Weil 1987; Griffin 2005). Since historical events are moments of heightened political involvement (Sewell 1996; Wagner-Pacifici 2017), this outlook implies that a historical event increases political engagement for those

who came of age when it occurred relative to other cohorts, and that these increases are larger the more intensely an event disrupted everyday social conditions or manifested in people's everyday experience when they were active.

These expectations are predicated on an outlook to event effects that sees their lasting political influence as a process constructed mainly synchronically. In this view, the cohort differences that events produced develop when an event is active, and once they are generated, they are assumed to continue unchanged and survive the passage of time. So far, this assumption has been given little discussion, but its viability can be associated with findings that personal memories from young adulthood are more vividly and more frequently rehearsed (Rubin 1998) and that memories from this life period tend to be perceived as being more important than others (Schuman and Scott 1989).

Still, it remains unclear how these vivid and personally important memories might be able to continuously shore up politicization levels as time goes by, against the backdrop of changing political conditions and the erosion of political engagement occurring along the life span. Although motivational resources for political engagement tend to remain stable, other resources for political engagement steadily erode once people reach adulthood. Individuals tend to undergo life events, like marriage and parenting, that reduce their investment in public-oriented matters (Kalmijn 2003; Stoker and Jennings 1995). After young adulthood, people's core interaction networks, where the bulk of political interaction occurs, also start to shrink (Wrzus et al. 2013; Marsden 2018). steady decreases in political cognition and environmental sensitivity are added to these changes in senior years (Lau and Redlawsk 2006). These processes suggest that event influences on cohort levels of politicization erode over time, making older events be less capable of shaping cohort levels of politicization.

Identifying event effects as processes of cohort differentiation that fade over time also allows developing an outlook towards event effects less focused on what they synchronically provoked when they were active and more centered in the diachronic consequences they have over time in reducing or increasing the endowment of political engagement resources for people, and of building historical conditions capable of slowing the decay of their influences.

An event's capacity to stay relevant as an organizing political issue, for example, might be able to extend the time span of its politicizing influence regardless of how intense it was when it occurred. An event that was powerful but no longer resonant with issues organizing political issues later on—for example, school desegregation after the sixties—might be less likely to sustain effective political engagement over time than an event that continues organizing contemporary political issues—for example, events that extended their resonance by having generated sweeping changes in state policy outlooks.[2]

A diachronic outlook to event effects on politicization also calls for revising the assumption that events exert only positive influences. Instead, it suggests that an event has positive or negative impacts on politicization depending on whether it was politically polarizing or "divisive," or produced a rallying effect that made it "cohesive." This argument is based on the recognition that the impact that events have on young adults is not only cognitive but also structural.

Events increase political attention and multiply political interactions. When an event is divisive, these changes will ease people's ability to identify the ideological positions of the people they interact with. By doing so, they facilitate detecting ideological homophily. Since this trait is a key characteristic of political interactions, events perform as "subsidies," so to

[2] It is, of course possible, that historically dry events can nonetheless influence other important political attributes over time (Osborne, Sears, and Valentino 2011).

speak, for the generation of new steady political interaction partners (Noelle-Neumann 1993; Mutz 2002; Baldassarri and Bearman 2007). Young adults are particularly well equipped to capitalize on this opportunity. They stand in a structurally fluid biographical time when teenage ties are being substituted by new workplace or university contacts, with whom they begin to build-long lasting adult interaction networks (Bidart and Lavenu 2005). Under these conditions, the subsidy for political interaction ties that events generate provides young adults with an expanded pool of interaction partners. This will give them larger resources to start their adult political involvement, which will make them develop higher levels of political engagement relative to other cohorts that did not experience a political event during their coming of age. On the other hand, if an event is cohesive, we might expect the opposite situation. Relative to normal, uneventful times, the political interaction increases that stem from these types of events will obstruct the detection of political positions, hinder identifying political homophily, and hamper the development of political interaction networks in early adulthood.

Hypotheses.

Overall, the discussion above identifies three different sets of hypotheses on how events might affect political engagement in the long run.

Two hypotheses contend that historical events are unable to exert lasting influences in politicization. One (H1) argues that since levels of political engagement are related primarily to family and life-event socialization processes that are independent of historical developments, we should not expect events to exert persistent influences on politicization. Another (H2) contends that we should not expect events to be associated with political

engagement independently from the relationship between the affluence of a socializing context and political engagement:

> *H1: No historical effects.* Historical events are unable to generate persistent influences in political engagement.
>
> *H2: No evenemential effects.* When the positive relationship between a socializing context's affluence and political engagement is taken into account, historical events do not exert lasting influences on political engagement.

Generational investigations, on the other hand, see historical events as capable of impacting politicization in the long run. They take a synchronic outlook to understand these influences, seeing them as constructed mainly when an event is an ongoing political disruption. This view suggests that events positively impact cohort levels of politicization (H3a), that this impact is time resistant (H3b), and that the magnitude of its influence is associated with the intensity at which an event manifested itself on people's everyday experience when they occurred (H3c):

> *H3a: Positive cohort effects.* An event's occurrence makes the political engagement of cohorts that experienced it closer to young adulthood persistently larger than other cohorts'.
>
> *H3b: Permanent of effects.* The positive effects on politicization that events exert do not decay over time.
>
> *H3c: "Intensity" hypothesis.* The magnitude of the increases in politicization lasting effects that an event produces on political engagement is positively related to the strength at which it manifested as a political disruption when it occurred.

Finally, I offer a diachronic alternative set of hypotheses on event effects on politicization. It sees these capabilities as stemming not directly from what they did when they were active but from how they relate to political conditions once they are no longer present. This "recursive" approach to event effects sees them eroding over time (H4a) but argues that this erosion can be slowed down if an event manages to keep organizing political cleavages in the future—for example, if an event keeps resonating with state actions by having produced lasting sweeping changes in them (H4b). In light of the kind and strength of the structural changes that historical contingencies produce, it also contends that the direction of influence of an event on cohort levels of politicization will be positive or negative depending on whether it was divisive or cohesive (H4c):

> *H4a: Fading effects.* The more recent an event is, the more likely it will impact cohort levels of political engagement.
>
> *H4b: Polarization defines direction of influence.* Politically divisive events are associated with relative increases in politicization for cohorts that came of age when they occurred; cohesive contingencies, on the other hand, are associated with relative decreases.
>
> *H4c: "Effectiveness" determines magnitude of influence.* Historical events that produce sweeping changes in state action are more likely to keep exerting lasting cohort-level effects in political engagement.

RESEARCH DESIGN: DATA, SELECTION OF EVENTS, AND MEASUREMENT OF EVENT CHARACTERISTICS.

I evaluate the hypotheses above by examining the impact that a theoretically built set of twentieth-century historical events from Belgium, France, West, Germany, Italy, and the Netherlands had on the frequency with which people talk about politics from 1973 to 2002.

Data

The data I use comes from the Mannheim Eurobarometer Trend Dataset, which collects questions included at least five times in Eurobarometer surveys from July 1970 to April 2002. One such question is one which I will refer to as "political talk": "when you get together with friends, would you say you discuss political matters frequently (2), occasionally (1) or never (0)?" For Belgium, France, West Germany, Italy, and the Netherlands, the Mannheim Dataset includes *political talk* responses from sixty-eight different survey points over 29 years (1973-2002).[3] Eurobarometers collect information from people aged 15 and up. This coverage allowed me to examine *political talk* responses for people in these countries born as early as 1900 and as late as 1985.[4]

As a preliminary inspection of how *political talk* varies by birth year, Table 1 shows descriptive cohort statistics for this variable. Cohorts from Belgium exhibit the lowest mean value of *political talk* (0.569) and those from West Germany the highest (0.906—just below the "sometimes talking about politics" threshold). Consistent with findings from the literature (Bennet, Fischer and Resnick 1995; Bearman and Parigi 2004), these numbers show that people discuss politics relatively infrequently.

[3] The time span, density, and distribution of the data I analyze are, at minimum, comparable to equivalent figures for recent long-term generational analyses (Caren, Ghoshal and Ribas 2011; Ghitza and Gelman 2014). Response values were renumbered for ease of interpretation.

[4] Appendix A shows the distribution of responses across cohorts. I excluded cohorts born before 1900 from the analysis since they were sampled at ages when being alive is closely associated with education and income, thus violating missing-at-random assumptions.

TABLE 1
POLITICAL DISCUSSION: COHORT DESCRIPTIVE STATISTICS PER COUNTRY [1,2]

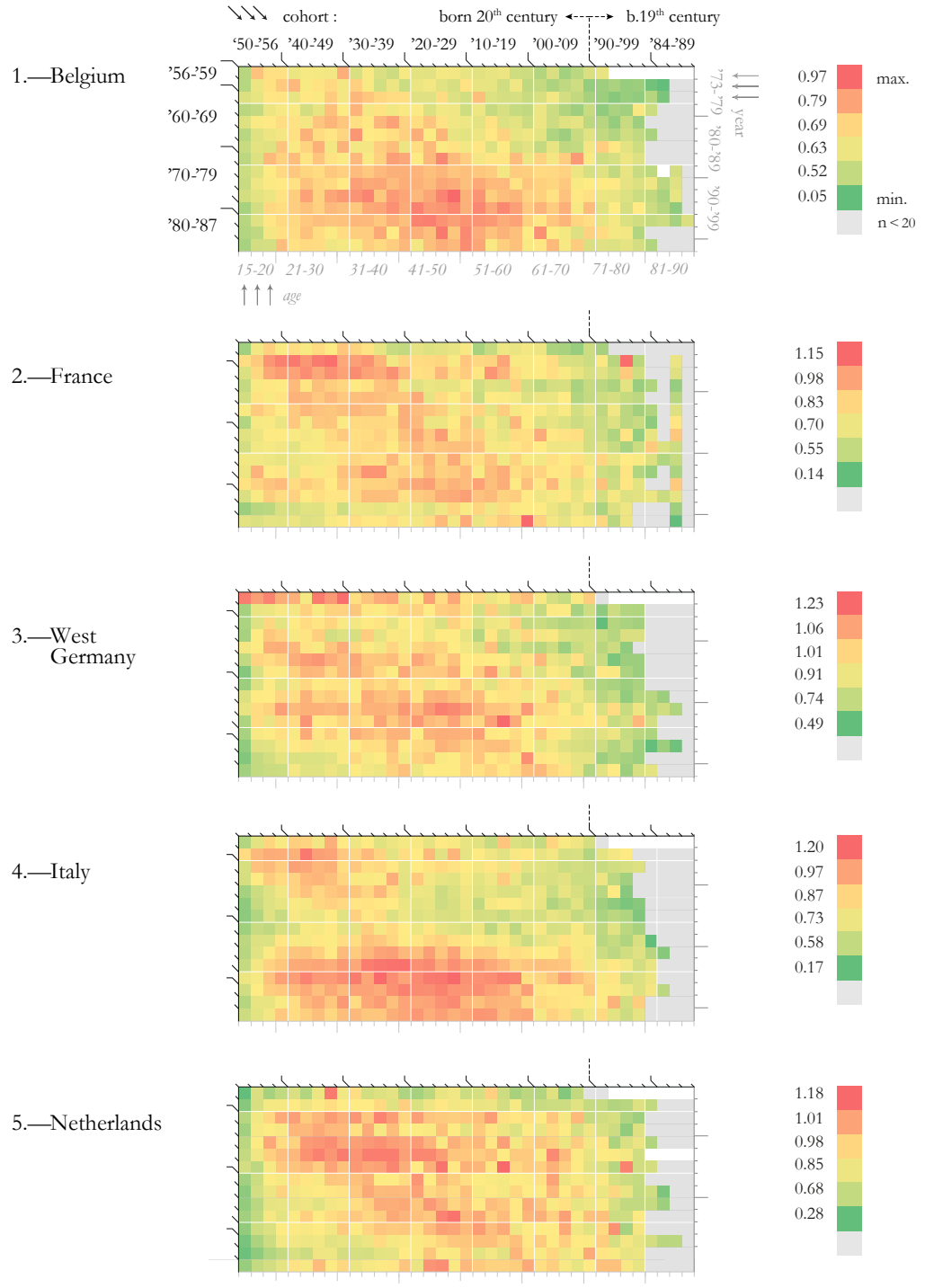
	Cross-Cohort Values			
	(1)	(2)	(3)	(4)
	Mean	Standard Deviation	Minimum	Maximum
1.—Belgium	.569	(.569)	.120	.794
2.—France	.748	(.748)	.350	.951
3.—West Germany	.906	(.906)	.444	1.063
4.—Italy	.772	(.772)	.454	.994
5.—Netherlands	.842	(.842)	.147	1.040

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

But how did cohort levels of political talk evolve across time beyond these static aggregate indicators? Is there any initial evidence of cohort patterns related to differential exposures to historical events?

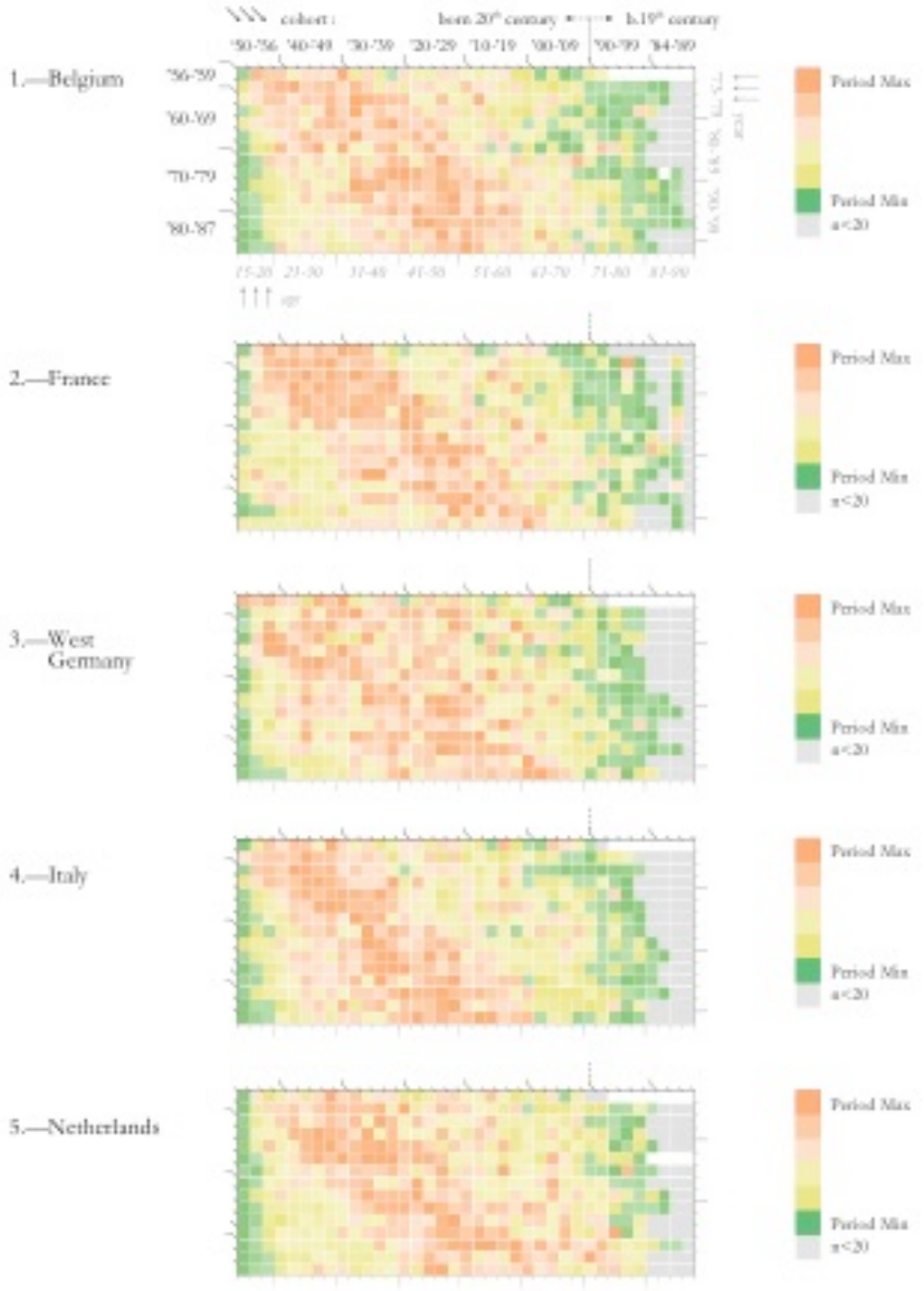
I explore these questions with the help of the heat maps in Figures 1A and 1B, which plot *political talk* values across 2-year age/period cells. Cells from the same biennium are distributed along a single row, and cells across age are distributed across columns; in consequence, cells associated with a specific cohort are diagonally distributed. In Figure 4A, a cell is colored according to its mean *political talk* value relative to all other cells; in Figure 4B, cells are similarly colored according to their talkativeness relative to cells from the same biennium. Cells with higher means are colored with stronger shades of red, and those with lower values are colored with darker tones of green; cells with a small sample of respondents ($n<20$) are shown in gray.

FIGURE 1A
POLITICAL DISCUSSION VALUES ACROSS TIME
COLOR HISTOGRAM, 1973-2002



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

FIGURE 1B
POLITICAL DISCUSSION VALUES ACROSS TIME
COLOR HISTOGRAM BY 2-YEAR PERIOD, 1973-2002



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

Figure 1A shows that *political talk* changes according to environmental conditions. For instance, in Italy, its values shifted from moderate figures during the 1980s to maximum figures at the beginning of the 1990s, when the country was shaken by massive corruption scandals, high profile terrorist acts associated with the mafia, and the collapse of its postwar political system—which is sometimes referred to as the "First Republic" (Gundle and Parker 1996).

Figure 1B, on the other hand, shows that net of historical contexts, *political talk* levels are also organized by cohorts. Cells that exhibit biennial peak values for this variable are diagonally organized, indicating noticeable stability of cohort rankings of political discussion. Cohorts that exhibit high levels of *political talk* when they were young relative to others tend to keep doing so later on. On the other hand, those who came of age exhibiting low *political talk* values remain being relatively silent.[5] Figure 1B also shows that cohorts that are either frequent or infrequent talkers reached adulthood when major political contingencies occurred. In Italy, people that came of age during the collapse of the First Republic keep being frequent political talkers later on. By contrast, cohorts that came of age during the first years of the eighties, when several powerful terrorist attacks occurred in the country (Tota 2003), were infrequent political talkers then and continued being so over time.

Overall, these trends provide initial supporting evidence that historical events can modify cohort levels of *political talk*. Thus, they motivate the conduction of an explicit examination of how experiencing moments of historical contingency are associated with cohort levels of political talk, and how these associations relate to the hypotheses I identified in the previous section.

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

I conduct this analysis by identifying the set of historical contingencies that the cohorts included in my data experienced in the countries I analyzed, generating cohort variables of historical exposure to each of them, and evaluating the performance of these variables of *political talk*.

Identification of Events and Event-Level Characteristics

I conducted an in-depth historiographical revision of the twentieth-century history for the countries under analysis, first, to identify which historical occurrences to include in my investigation, and second, to gauge several characteristics relevant to the hypotheses I seek to test.

My investigation was oriented at detecting historical instances connected to the notion of "historical event" recently advanced by historical sociology as an abrupt, widespread, and collectively experienced political contingency (Sewell 1996; Wagner-Pacifici 2017). I identified an occurrence forming part of these instances if it was described as an unforeseen political contingency that abruptly provoked major discontinuities in the political environment of a country or if it provoked sudden shifts in people's everyday relationship with politics. Overall, I was able to identify such instances in 43 contiguous "eventful" years between 1918, when the first cohort under analysis came of age, and April 2002, when the last Eurobarometer survey from my data was conducted.

Some of these periods include political contingencies that originated in social mobilizations (e.g., the May '68 protests in France [Bavard 2008]). In addition to these occurrences, which have been the type of events more frequently investigated by previous research, the periods I identified also feature contingencies associated with electoral processes (for example, the 1948 General Election campaign in Italy [Novelli 2008]);

government crises (e.g., the 1978 breakup of the Egmont Pact in Belgium [Brassine and Mabille 1978]); 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 [Barrez 1998]); and contingent periods of diverse origins, such as the political emergencies provoked by Germany's failure to keep up with war reparation payments in 1923 (Maier 1975).

Ten of these eventful periods occurred in Belgium, 8 in France, 13 in West Germany, 7 in Italy, and 5 in the Netherlands. Figure 2 shows the temporal location of these eventful years and a name referring to the events that occurred in them.[6] For ease of exposition, I will refer to these evenemential periods as "events"; to refer to a specific period, I will use the name of the political contingency(es) that occurred in it. Appendix B provides a selected bibliography for the contingencies events that occurred in each of these periods.

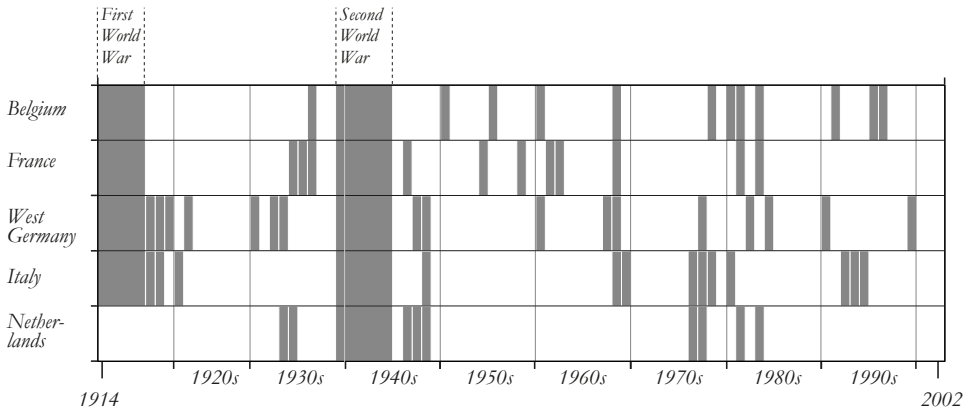
I also used my historiographical research to identify synchronic characteristics related to how intensely an event manifested in everyday reality when it occurred, and to diachronic characteristics related to its recency and the changes, if any, it exerted on state action. (For specific figures for these estimates for each of the events I analyze, refer to Appendix B).

As measurements of an event's intensity, I generated indicators of their duration, narrative diversity, political disruptiveness, and experiential strength.

[6] Several events spanned multiple years. In these cases, the year of an event's occurrence was assigned to the one that included the largest part of the event's life as a major political contingency. The year of occurrence of the 1991 "Black Sunday" in Belgium, for example, was assigned to 1992 because the election was held in November, and most of its political effects took place in that year (Mabille and Brassine 1992).

FIGURE 2
VARIABLES OF HISTORICAL EXPOSURE TO EVENTS[1]

Temporal Location



Period Event Instances

Belgium

1.— '36. First Black Sunday, Summer strikes.
2.— '50. Abdication of Leopold III.
3.— '55. Collard Law protests.
4.— '60. Intervention in Congo; Winter strikes.
5.— '68. Leuven University Split crises.
6.— '78. Egmont Pact breakdown.
7.— '80-'81. Events from the *Redresesement* period: Federalization crises ('80); Political-Economic crises ('81); First Peace March ('81).
8.— '83. Second Peace March.
9.— '92. Second Black Sunday and Martens downfall.
10.— '95-'96. Agusta-Dassault Corruption Affair ('95); Marc Dutroux Affair & White March ('96).

France

1.— '34.-'36. Events from the Popular Front development period: Anti-Parliamentary Riot ('34) Unitary Rally ('35); Popular Front Victory & Summer strikes ('36).
2.— '47. Tripartite Government Fall and Strikes, Establishment of Gaullist Front.
3.— '54. Dien Bien Phu & Indochina Retreat.
4.— '58. First Algiers Putsch and Establishment of Fifth Republic.
5.— '61-'62. Algerian crises: Referendum on Algerian Independence, OAS Terrorism (First Wave); Second Algiers Putsch; Repression of Algerian- and Left-Wing Protests ('61); Évian Agreements, OAS Terrorism (Second Wave), De Gaulle Murder Attempt ('62).
6.— '68. Spring '68 Events.
7.— '81. Socialist Electoral Victories.
8.— '83. *Tournant de la Rigueur:* economic reform policies.

[1] See Appendix B for selected bibliography and further information on the events

FIGURE 2 (CONT.)
VARIABLES OF HISTORICAL EXPOSURE TO EVENTS

Event Instances

West Germany

1.— '18-'20. Events from the German Revolution period: Capitulation, January Strikes, and Second Reich Downfall Events ('18)[2]; January Uprising, Freikorps Campaigns, Dissolution of Councils & Weimar Constitutive Assembly ('19); Kapp Putsch and Ruhr Uprising ('20).
2.— '23. War Reparation Crises.[3]
3.— '30. Federal Election Results.
4.— '32-'33. Events from the Weimar Republic downfall period: National Concentration Cabinet crises ('32); Reichstag Fire and Enabling Acts ('33).
5.— '48-'49. Events from the Federal Republic Establishment period: End of Blockade & Economic Reintegration ('48); Establishment of Federal Republic ('49).
6.— '61-'62. Berlin Partition ('61); Spiegel Affair ('62).
7.— '67-'68. Summer and Emergency Law Protests.
8.— '72. RAF Terrorist Acts; *Ostpolitik* Political Crisis.
9.— '77. RAF Terrorism: Stammheim Offensive.
10.—'81. Euromissile Mobilizations, Corruption Scandals.
11.—'83. Events from the Wende Period; Flick Commission; Nuclear Action Week; Helmut Schmidt Downfall.
12.—'90. Reunification.
13.—'00. CDU Financing Scandal.

Italy

1.— '18-'20. Events from the *Biennio Rosso* period: Strikes, Fiume Occupation Crises, General Election Results, ('18-'19); Social Violence Events ('20).[4]
2.— '22. Legalitarian Strike and March on Rome.
3.— '48. General Election, Strikes.
4.— '68-'70. Events from the *Maggio Strisciante* period: *Sessantotto* Protests and Labor Strikes ('68); *Autunno Caldo* ('69); Piazza Fontana Attack & Aftermath ('70).
5.— '76-'78. Events from the *Anni di Piombo* period: Lockheed Scandal, General Election ('76); Lockhed Commission; '77 Movement & Terrorist Acts ('77); Aldo Moro Murder ('78).
6.— '80. Bologna Station Attack; Donat-Cattin Scandal.
7.— '92-'94. Events from the 'First Republic' Dissolution period: *Mani Pulite* Inquiry; Falcone & Borsellino Murders, Mafia Terrorism ('92); Cusani Trial; Amato Resignation, Technocratic Gov't ('93); *Discesa in Campo,* Berlusconi's electoral victory and resignation ('94).

Netherlands

1.— '33-'34. *Die Zeven Provinciën* Mutiny ('33); Jordaan Riot ('34).
2.— '46-'48. Indonesian Crises: Linggajatti Agreements & Conscription Protests ('46); First Indonesian Police Action ('47); Second Indonesian Police Action ('48).
3.— '76-'77. Lockheed Affair ('76); Glimmen Train Hostage Crisis, Gov't Formation Crisis ('77)
4.— '81. Euromissile Dissensus.
5.— '83. *Keerpunt* and Second Peace March.

[2] Capitulation; Kiehl Mutiny; Reich Downfall (Wilhelm II abdication, Workers' Councils, December Coups).
[3] Ruhr Occupation, Passive Resistence, and Cuno Strikes; Bavaria State Commission; Dissolution of Communist Governments in Saxony and Thuringia, and Hamburg Uprising, Küstrin Coup, Beer Hall Coup.
[4] Ancona Mutiny; Factory Occupations; Fascist Squads and Palazzo Accursio Massacre; Siege of Fiume.

I measured an event's duration by counting the months spanning between its "beginning"—the major political disruption that originated an event—and its "end"—the moment when the political incertitude it produced was put under control. For instance, the duration of the Spring '68 events in France was two months: May and June '68.[7]

I also generated a measure of an event's narrative "dimensionality" by counting the number of separate sequences of political disruptions that an event generated. The French '68, for example, features three such sequences: the student protests of May and June; the wildcat strikes (and later on, union-backed) and negotiations between the government and unions that led to the signature of Grenelle labor agreements on May 27; and the political crisis provoked by the erosion of president Charles De Gaulle's control of the political agenda and the increasingly visible polarization regarding his permanence as president through May and June, which came abruptly to an end after his landslide electoral victory at the end of that month.[8]

An additional indicator of an event's intensity concerns its political disruptiveness. I counted the number of governments that fell in the eventful periods I investigated. The French '68 carries one such fall: the resignation of Georges Pompidou as Prime Minister in

[7] On May 2, left-wing students from the Nanterre campus of the University of Paris, located at the outskirts of the city, relocated their protests to the Latin Quarter in downtown Paris. Their actions led to the unprecedented closure of the University cloister and a violent eviction of protesters from university premises, which soon degenerated into large-scale street confrontations between police and students. This contentious period ended in June. The last week of that month, de Gaulle obtained a sweeping electoral victory that politically neutralized the massive protests and strikes that had taken place since May (Pavard 2018; Vigna and Vigreux 2008).

Appendix B shows duration estimates for the events under analysis and describes the historical occurrences associated with their beginnings and ends. While these starting and closing moments are to a degree arbitrary, they are connected to the period in which an event, in the words of Robin Wagner Pacifici (2017: 1358),"forced their way into subjects' field of attention."

[8] The minimum number of disruptive sequences of an event was one. The maximum is seven, pertaining to the French Algerian Crises of 1961 and 1962—see Appendix B.

reaction to the strains that the May and June protest episodes provoked in his relationship with president De Gaulle.[9]

As a fourth and last measure of how strongly an event manifested in people's experiences, I generated an indicator distinguishing various degrees of experiential intensity: a "mild" one indicating disruptions constrained mainly to the formal domain of politics; a "moderate" one for contingencies that impinged directly in everyday experiences either in the form of economic crises or latent violence, as in the case of large waves of terrorist acts; and a "high" one for disruptions associated with a more generalized collapse of the rule of law.[10]

As a last measure of an event's strength, I also categorized events according to the "type" of political disruption they were most importantly associated with: insurrections, terrorist acts, protests and strikes, corruption and malpractice scandals, political crises (including decolonization), elections, and periods of contingency related to more than one type of events. Most events fall into this latter category.

I also identified two characteristics relevant to the diachronic set of hypotheses I introduced. One was the recency of an event—measured by the year at which it happened—, and the other was an indicator of how relevant it remained after its occurrence, which I

[9] The formation of a new government following a regular election or one headed by the prime minister and coalition that governed previously was not coded as a government fall. Twenty-two events did not provoke government changes. The period related to the development of the Popular Front in France (1934-1936) witnessed the fall of six governments, the maximum number in the event set (Jackson 1985; see Appendix B).

[10] An example of a "mild" event is the breakup of the Egmont pact (Brassine and Mabille 1978). It unexpectedly put an end to a political agreement on the federalization of Belgium but had few tangible implications in citizens' everyday lives. Events of "moderate" experiential intensity include, for example, the Autumn '77 terrorist acts conducted by the RAF in West Germany (Wunschick 1997). The events from the *Biennio Rosso* period, on the other hand, are an example of contingencies whose experiential intensity was coded as "high" (Maier 1975).

measured by evaluating whether it had provoked comprehensive turnarounds in state structures or in the orientations of its policies.[11]

METHODS

General Analytical Strategy

I study event effects on politicization, first, by analyzing how variables of cohort exposure to the set of historical events I identified above perform as regressors of *political talk* between 1973 and 2002, and second, by evaluating how this performance varied across events sharing different characteristics.[12]

The age-period-cohort (APC) identification problem is often voiced as a concern for the conduction of cross-sectional approaches to study temporal social dynamics. This concern stems from the fact that separate effects of age, period, and cohort influences cannot be reliably calculated because these three temporal concepts are perfectly collinear (Mason, Mason and Poole 1973). However, this problem is operative only if age, period, and cohort effects are assumed to be linear, monotonic (consistently growing or decreasing in value), and independent. These assumptions have not been theoretically defended and are at odds with key empirical findings and theoretical postulates from the literature.[13] Hence,

---

[11] An example of the first type of event is the German Revolution of 1918 (Ryder 2008); of the second, the 1936 Popular Front, which generated lasting labor reforms (Jackson 1985); and of the third, the Federalization crises of 1980, which finally reorganized the Belgian state along linguistic lines (Brassine 1980). I excluded the First Algiers Putsch and the Nazi Power takeover in Germany ('32-'33) from being understood as influential events due to the processes of collective memory suppression that have been documented for them (Harbi and Stora 2004, Lüdtke 1993; Kansteiner 2004).

[12] Since events are political disruptions bounded to country-specific contexts, I conducted separate analysis for each country. This also allowed controlling for country-specific idiosyncrasies in political communicative practices.

[13] Assuming linearity in age goes against systematic findings of an inverted-u relationship between age and political engagement. In addition, the absence of a progressive or regressive trend in the temporal distribution of the events I analyze also makes the cohort "effects" unlikely to be linearly organized. From a

similar to other recent studies on historical socialization (Bartels and Jackman 2014: 8), I do not see the APC identification problem as an inferential threat to my investigation.

An inferential issue that is more relevant for my cohort relates to potential omitted variable bias at the cohort level, which is pervasive in generational research studies. To assess this issue, I included measurements of cohort-level factors other than evenemential exposure as regressors of *political talk* in my analysis.

Another relevant inferential issue is related to model selection uncertainty, which warns against deriving conclusions from a single regression due to ignorance of the "true" causal model (Young 2009). This warning is particularly relevant for investigations like mine, which has few precedents in the literature. Being, to my knowledge, the first quantitative comparative analysis of event effects, there are no previous results with which to compare my results. Against this backdrop, my investigation addresses model selection uncertainty by evaluating how events performed as predictors of *political talk* in 72 different regressions models for each country under analysis. The analysis of my results is based on cross-regressions parameters that measure the magnitude, direction of influence, and significance—or "robustness" of their performance as regressors of *political talk* across this regression set.

Dependent Variable and Model Specification

The dependent variable is *political talk,* an ordinal covariate with three different values: never (0), occasionally (1), and frequently (2).[14]

broader epistemological standpoint, presentist social research is actually implicitly predicated on the premise that a period "affects" different people in different ways.

[14] Although this question focuses only on political talk with friendships, research indicates a significant part of political talk occurs among friends (Huckfeldt and Sprague 1995).

Sociological research has typically examined ordinal variables like this one using ordered logistic models. However, since my investigation analyzes multiple regressions, using these models is less recommendable because estimates from ordered logistic regressions cannot be compared across models due to unobserved heterogeneity (Mood 2010; Allison 2009).[15] As an alternative, recent methodological pieces have suggested using linear models. Their estimated coefficients are unbiased and consistent, and their substantive results in terms of average estimated effects, have been found to be nearly identical to those from logistic models (Breen, Karlson, and Holm 2018, 49-50; Mood 2010, 78). Following these recommendations, I use linear models to regress *political talk*. To control for heteroskedasticity, I used robust standard errors to calculate models' estimates. (As a robustness check I also conducted analysis on the statistical significance of regressors using ordered logistic regressions. The results of this analysis are reported in Table 4 and are similar to the ones I center my analysis on).

Key Independent Variables

The key independent variables capture cohort-level differentials in historical sensitivity to each of the events I analyze. I will refer to them as "event variables." I also generated variables of cohort exposure to the First and Second World Wars. Although not properly events due to their long duration, their importance for twentieth-century European history provided a compelling reason to include them in the analysis.[16]

[15] These pieces develop their argument for logistic models, but the logic of their discussion applies equally to ordered logistic models (Mood 2010, 79).

[16] The Netherlands did not participate in the First World War. I did not assign variables of exposure to World War I for Germany and Italy either because 1918 was also associated with the German Revolution and the dissolution of the Liberal political system, respectively.

The literature currently models variables of historical exposure dichotomously, giving a value of 1 to cohorts belonging to the "generation" that came of age when an event happened and a zero value to cohorts that did not. This construction captures the distinctiveness of young adulthood as a period of heightened historical sensitivity, but it is insensitive to the gradual, not abrupt, way in which this attribute diminishes in value across cohorts from younger and older ages (Galaz-García 2020).

I take into account the graded form of this decrease by modeling an event variable as a bell-shaped curve skewed towards cohorts that came of age when the event happened using the following exponential formula:

$$sensitivity_{i(k),j(k)} = \begin{cases} 100 & if\ coh^{*}_{j(k)} = coh_{i(k)}\ ; \\ 100*\exp[-6.0a*(coh^{*}_{j(k)} - coh_{i(k)})]^2 & if\ coh^{*}_{j(k)} < coh_{i(k)}\ ; \\ 100*\exp[-0.3a*(coh^{*}_{j(k)} - coh_{i(k)})]^2 & if\ coh^{*}_{j(k)} > coh_{i(k)}\ , \end{cases} \quad (1)$$

where $sensitivity_{i(k),j(k)}$ indicates the degree of historical sensitivity of individual *i* to the *j*-th event in country *k*, $coh^{*}_{j(k)}$ is the birth year of the cohort aged 20 when the *j*-th event happened, and $cohort_{i(k)}$ is the birth year of individual *i*.[17]

With this formula, cohorts that experienced a political contingency during young adulthood receive values in the vicinity of 100. They decrease rapidly and then more slowly until reaching near-zero levels for the oldest and youngest living cohort at the moment when an event erupted and for people not yet born at that time. To preserve causal precedence,

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

the values for each of these variables of event exposure were set to zero for responses from surveys conducted before the occurrence of an event.

The *a* coefficient defines the width of the bell. I analyzed the performance of events as regressors of *political talk* using four different widths: a=0.004, a=0.006, a=0.008, and a=0.010) The amplitude of the cohort segment with values larger than 90 in each of these bell widths was 11, 8, 7, and 6 years, respectively.

Figure 3 shows the shape of these variables across cohorts using the '68 French Spring as an example. This event variable peaks at 100 for people born in 1948, who were aged 20 when it occurred. Cohorts that experienced Spring '68 around this age exhibit values that are only marginally smaller to 100. The values for the variable decrease at a relatively fast pace and then at a slower speed for the youngest and oldest cohorts.

Controls

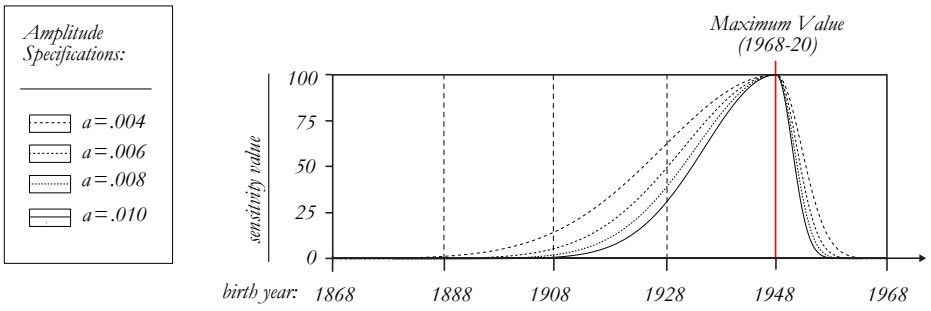
Cohort-level controls. I include as regressors measurements of cohort-level factors that have been discussed as influential for politicization: the size of the population of a cohort (Ryder 1965: 845-846) and the relative affluence of the period when it came of age (Inglehart 1981). There are no readily-available data series to measure the size or the level of educational attainment for the 80 cohorts of the five countries I analyze. I constructed this series using a variety of historical statistical sources that allowed me to produce minimally consistent measures of cohort size and educational attainment for these cohorts. Appendix C discusses how these data series were constructed.[18]

[18] 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.*

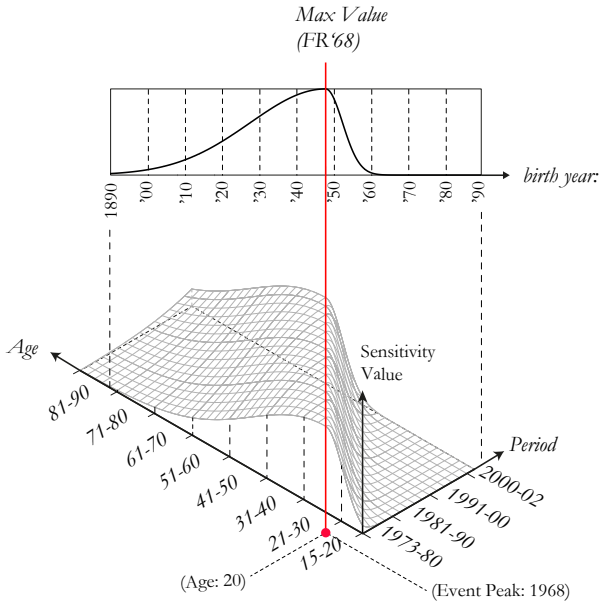
FIGURE 3
EVENT SENSITIVITY VARIABLES[1]

Example: Spring 1968, France

1.— Functional Forms of Sensitivity Variables



2.— Values across Time/Age Space
a = 0.004



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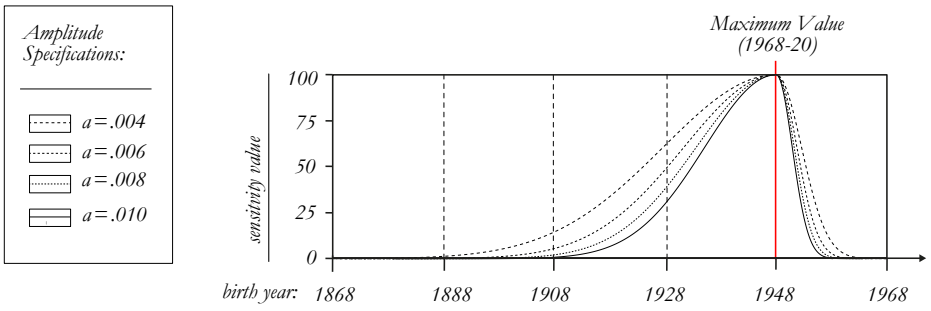
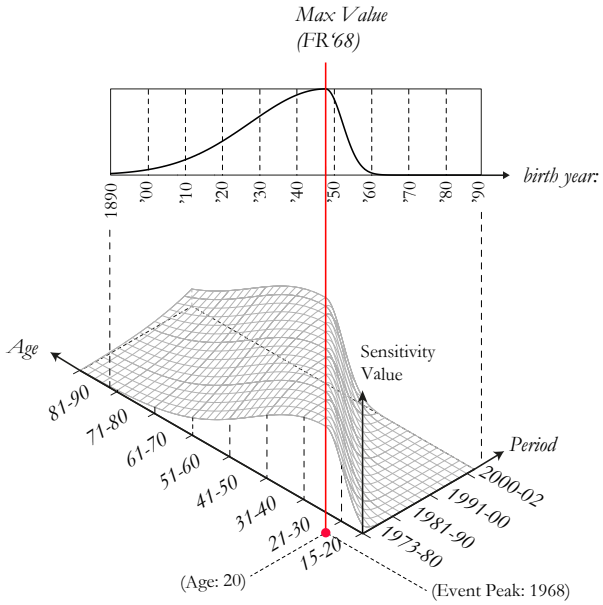


2.— Values across Time/Age Space
a = 0.004



I measured *cohort size* as the number of people (in thousands) aged between 15 and 20 when a cohort was aged 18. As an indicator of the affluence of a cohort's socialization environment, I used its *educational attainment* level, measured as the proportion of university students or graduates among people aged between 15 and 25 when a cohort was 18 years old.

In addition, to control for other cohort-level factors that could potentially affect *political talk,* I also included dummy variables indicating a respondent's adscription to one of twenty 5-year *cohort categories*. The reference category was the first cohort bracket included in the analysis.

Period-Level Controls. To test the impact of elections on political talk (Sears and Valentino 1997), I included a dummy variable indicating general *election* years. I also included yearly GDP per capita growth rates to test the strength of association between *political* discussion and *economic performance.*

Individual-Level Controls. I included variables indicating *age*, *income, education*, *female* identification, and *marital status* as individual-level controls. These factors have been previously associated with political discussion (Marsden 1987; Moore 1990; Huckfeldt and Sprague 1995; Bearman and Parigi 2004). Following standard findings from the literature, I included age using a quadratic specification. I measured education through categorical dummies indicating maximum educational attainment: incomplete high school, complete high school, incomplete college, incomplete college, and ongoing studies.[19] I included

[19] Eurobarometers do not provide direct information on respondents' maximum educational attainment; I use the age at which a respondent left school as an indicator to generate indicators of attainment related to 4 categories that ranged from not having finished junior high school (1) to having completed college studies (4), plus a fifth indicating continuing education. I included *income* through five categorical dummies associated with monotonically increasing income brackets, measured in 2002 real value local currencies, and a sixth indicating non-response.

income through five categorical dummies associated with monotonically increasing earing brackets, measured in 2002 real value local currencies, and another one indicating non-response. The reference categories for these variables were people with incomplete higher education and the lowest income bracket.

I also included *urban* and *metropolitan* residence indicators[20] and dummies indicating the region of residence of a respondent—for each country, the reference category was its most industrialized region. Except for *income,* for which I generated a non-response indicator, no variable exhibited evidence of violating missingness-at-random assumptions.

Set of Regressions under Analysis and Regressors' Parameters of Performance

To control for model selection uncertainty, I evaluate the performance of event variables as regressors of *political talk* across 72 regressions per country. Each included an invariable vector containing all period, event exposure, and individual- level controls, and a specific permutation of event variables of a particular width (a=0.004, 0.006, 0.008, and 0.0010), specifications for *cohort educational attainment* (absence, linear, or quadratic specifications), *cohort size* (absence, linear, or quadratic specification), and *cohort dummies* (presence or absence).

My analysis evaluates regressors' magnitude, direction, and significance—or "robustness"—of influence across these models by examining several cross-regression parameters of performance.

Following Young and Holsteen (2017), I analyzed the magnitude and direction of influence of an independent variable by calculating the mean value and standard deviation of

[20] The metropolitan residence indicator distinguishes inhabitants of cities with an urban core exceeding a million inhabitants (Paris, West Berlin, Rome, and Amsterdam) from the rest of the population.

its estimated coefficients. I used these parameters to gauge the strength of association between an event and *political talk* and evaluate if it was "robustly" unidirectional—that is, if the distribution of these values was negative or positive at standard significance levels.

I evaluated robustness of influence by calculating covariates' significance rates (SRs), or the proportion of regressions at which their estimated coefficients were significant at standard levels of confidence. Using this value, and drawing from Ragin's discussions on causal sufficiency tests (2000), I also developed a statistic, which I call the "robust significance" estimate, that calculates the probability that a variable would be significant in 75% or more of possible regression specifications using the following z-value test:

$$z_{sig} = \frac{(RS - p) - (1/[2(n))}{[p(1-p))/n]^2} \qquad (2)$$

where *RS* refers to the rate of significance, *n* refers to the number of models in the regression set (72 for my analysis), and *p* refers to the benchmark rate of significance, which, following Ragin (2000), was set at $p = 0.75$.[21] The values of this test range from 0 to 1, and its logic of interpretation is the same as a standard *p*-value. I will refer to variables exhibiting test values below 0.1 as robustly significant predictors of political talk.

FINDINGS

[21] *Cohort size* and *cohort educational attainment* were included in their linear or quadratic form in less than thirty regressions. Accordingly, robust significance tests for them were conducted using negative binomial probability tests.

FINDINGS

I begin by exploring summary results for control variables. Table 2 shows the direction and range of variation of estimated effects[22] for individual- and period-level controls that exhibited robust significance and directionality.

TABLE 2
SUMMARY OF META-ANALYTIC RESULTS FOR INDIVIDUAL-LEVEL AND PERIOD CONTROLS

	Variation of effects (if significant)[1]							
	Individual-level controls						Period Controls	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Countries	Educ.[3]	Income[4]	Age	Female	Rural	Married	Election	ΔGDP
1.— Belgium	.446	.183	.392	.195	.027	N/S	.016	.059
Direction[2]:	(+)	(+)	(+)	(–)	(–)	.	(+)	(+)
2.— France	.461	.256	.218	.147	.049	.027	.020	.076
Direction:	(+)	(+)	(quad.)	(–)	(–)	(–)	(+)	(+)
3.— West Germany	.306	.083	N/S	.208	.026	.040	N/S	.088
Direction:	(+)	(+/–)	.	(–)	(–)	(+)	.	(–)
4.— Italy	.420	.173	.406	.300	.028	.027	.013	N/S
Direction:	(+)	(+)	(quad.)	(–)	(+)	(–)	(+)	.
5.— Netherlands	.389	.302	.143	.057	.012	.037	.047	.049
Direction:	(+)	(+/–)	(quad.)	(–)	(–)	(+)	(+)	(–)

[1] Difference between the largest and the smallest statistically significant average estimated coefficient. N/S: Robust Significance Indicator not significant at standard confidence levels

[2] (–): monotonically negative relationship; (+): monotonically positive relationship; (–/+): relationship includes positive and negative variations; (quadr): quadratic relationship.

[3] Reference category: lowest-earning bracket.

[4] Reference category: incomplete high school.

Overall, the performance of these covariates as predictors of *political talk* is consistent with previous literature. *Education* brackets are the single most important predictors of *political talk*. They are positively related to this behavior: higher levels of educational attainment are associated with higher levels of *political talk*.

[22] Estimated coefficients and rates of significance for each categorical controls are reported in Appendixes D1, D2, and D3.

Income also exhibits a generally positive association with *political talk,* although there are several exceptions to this trend. In West Germany and the Netherlands, middle-income brackets are associated with less political talk than the lowest-earning bracket. The relationship between *political talk* and age, on the other hand, follows an inverted-u relationship: political discussion increases until reaching a peak in mature adulthood, and it then decreases.[23] Being identified as a woman, on the other hand, is negatively associated with political discussion in all the countries under analysis. The magnitude of this relationship changes notoriously: in Italy, for example, it is almost six times larger than in the Netherlands. Residence in *rural* areas is also a negative predictor of political talk, but the strength of this association is small.[24]

The influence of *marriage* on *political talk,* on the other hand, varies by country. Its effect is not significant in Belgium, negative in Italy and France, and positive in West Germany and the Netherlands.

With respect to period controls, the association between *political talk* and an *election* year is also positive, but its magnitude is also small. The relationship between *economic performance* and *political talk*, on the other hand, is positive but small in Belgium and France, not significant in Italy, and small and negative in Germany and the Netherlands.

I now proceed to discuss how cohort-level controls performed as predictors of *political talk*. Table 3 shows average estimated coefficients and significance rates for *cohort education* in their linear and square specifications (column 1 and columns 2 and 3, respectively). Columns 4 to 6 present equivalent parameters for *cohort size*.

[23] In Belgium, the inflection point of this quadratic curve occurs at a late age, which makes the relationship between age and *political discussion* increase monotonically between 15 up to 80 years.

[24] Italy is an exception: living in a rural area is positively related to *political talk*.

TABLE 3
META-ANALYTIC RESULTS FOR COHORT CONTROLS[1]

	Cohort Education Attainment			Cohort Size		
	Lin. Spec.	Quad. Specification		Linear Spec.	Quadratic Specification	
	(1)	(2)	(3)	(4)	(5)	(6)
Countries	Linear Term	Linear Term	Square Term	Linear Term	Linear Term	Square Term
1.— Belgium						
Est. Coeff.[1]	-0.121	1.140**	-2.151*	-6.07e-4	1.93e-4	2.85e-7
Sig. Rate[2]	*0.375*	*0.708*	*0.625*	*0.625*	*0.292*	*0.292*
2.— France						
Est. Coeff.	-0.913***	-0.385	-0.905	-1.27e-5	-2.17e-4	2.99e-8
Sig. Rate	*1.000***	*0.291*	*0.500*	*0.291*	*0.250*	*0.292*
3.— West Germany						
Est. Coeff.	-0.562*	-0.883***	0.680	-7.26e-7	6.31e-6	-8.72e-10
Sig. Rate	*0.750*	*1.000***	*0.041*	*0.083*	*0.167*	*0.167*
4.— Italy						
Est. Coeff.	-0.162	1.952***	-4.319***	-7.16e-5	-7.53e-5	1.33e-10
Sig. Rate	*0.583*	*1.000***	*1.000***	*0.833*	*0.167*	*0.208*
5.— Netherlands						
Est. Coeff.	-0.864***	-0.298*	-2.500***	1.55e-4*	-2.21e-4	1.82e-7
Sig. Rate	*1.000***	*0.500*	*0.916***	*0.708*	*0.208*	*0.208*

Significance: [+]0.1 level; * 0.05 level; ** 0.01 level; 0.001 level.
[1] Significance levels show if the distribution of values were unidirectional at stadard levels of confidence.
[2] Significance levels from Robut Significance Indicator: probability that of a variable being significant in at least 75% of models given observed sigificance rate.

Coefficients for *cohort size* were marginal in magnitude and very rarely significant. Estimated coefficients for cohort levels of educational attainment performed better as predictors of *political talk*. In Italy, terms from the quadratic specification of *cohort educational attainment* were robust in magnitude and significance, as were linear specifications in the Netherlands and France.[25] In Belgium, the magnitude of coefficients is robust in magnitude, but their SRs are not significant. Germany is the only country where *cohort education* performed poorly as a predictor of political talk. Only the linear term of the quadratic

[25] In the Netherlands, the terms for the quadratic specification were also significant, but the linear term was significant only in half of the regressions under analysis. This suggests that a relationship between cohort education and political talk might more likely be linear.

specification exhibits estimated coefficients that are robust in both magnitude and significance.

The direction of the association between cohort educational attainment and *political talk,* however, goes in the opposite direction of postmaterialist expectations: people from cohorts with larger shares of college graduates tend to talk *less,* not more, about politics. The only exception is Italy. In this country, the quadratic relationship between *cohort education* and *political talk* produces mostly increases across cohort levels of educational attainment.

Indicators of adscription to 5-year cohort categories tended to be non-significant, with exceptions of one cohort bracket for Belgium (b. 1906-1910) and West Germany (b. 1921-1925), and four contiguous dummies in Italy (b. 1941-1960; see Appendix D3).[26]

After this brief discussion of results for controls, I now proceed to discuss results for event variables.[27]

On average, events were significant regressors of *political talk* in little more than half of the regressions under analysis (SR= 0.605). A large standard deviation (0.326), however, indicates wide departures across events from this figure. Similarly, the average value of estimated coefficients for events is 0.000143, with a standard deviation almost seven times larger (0.000978). Overall, directionality of influence is largely evenly distributed across events. Twenty-three exhibit positive mean estimated coefficients; those from the remaining twenty are negative. These figures reveal wide heterogeneities in the robustness, direction, and magnitude of influence with which events are associated with *political talk*.

Against this backdrop, how many events—if any—were capable of performing robustly as regressors of political discussion*?*

[26] Significant birth year indicators are 1906-1910 for Belgium; 1921-1925 for West Germany; and the four indicators between 1946 and 1960 for Italy.

[27] For event findings, my discussion excludes results for the First and Second World Wars variables.

Table 4 shows my analysis' results for individual events. For each variable of event exposure, it shows the magnitude and statistical significance of average estimated coefficients (column 1) and its significance rate for OLS models(column 2), and as a robustness check, for ordered logistic models (column 3).

Due to space concerns, I will focus the discussion of these results on the events that were robustly significant predictors of *political talk*. My analysis identified sixteen such evnts.[28]

Three robust events are from Belgium: the break up of the Egmont federalization pact of 1978 and the political crisis it triggered; the political contingencies that occurred between 1980 and 1981, which included government crises provoked by state reform proposals towards federalization, a steep economic downturn, and the organization of a massive march against nuclear missiles; and the results of the December 1991 snap election, which saw a steep increase in the vote share of the extreme right, and ended the nearly uninterrupted 11-year old tenure of demochristian Wilfried Martens as prime minister.

France has two robustly significant events: the victory of François Mitterrand in the French presidential elections of 1981 and the unprecedented absolute parliamentary majority that the left obtained in the legislative elections of that year; and the 180-degree turn in the economic policy of the Mitterrand government of 1983, which implemented sweeping neoliberal austerity reforms.

[28] In the four models of the regression set without cohort controls, 32 events exhibit average p-values below standard significance levels. This set of events shrinks to 8 once across the full space of regressions I analyzed. This notable drop suggests the existence of an omitted variable bias for event-based research without control variables at the cohort level.

TABLE 4
META-ANALYTIC RESULTS FOR EVENT VARIABLES[1]

Variables	Ordinary Least Squares Model (1) Avg. Est. Coef.[3]	Ordinary Least Squares Model (2) *Signif. Rate*[4]	Ordered Logistic Model[2] (3) Signif. Rate[4]
Belgium			
1.— '18. End of First World War	-.0012	*.500*	.500
2.— '36. Black Sunday; Summer Strikes	-.0013***	*.806*	.819
3.— '39-'45. Second World War.	.0000	*.028*	.028
4.— '50. Abdication of Léopold III.	-.0005	*.417*	.625
5.— '55. Collard Law Protests.	-.0001	*.236*	.194
6.— '60. Intervention in Congo; Winter Strikes.	.0009	*.431*	.431
7.— '68. Leuven University Split Crises	.0005	*.514*	.556
8.— '78. Egmont Pact Breakdown	-.0010*	*.972***	.986***
9.— '80-'81. *Redressement* Events	.0012***	*1.000***	1.000***
10.—'83. Second Peace March	-.0004*	*.597*	.444
11.—'92. 2nd Black Sunday	.0007***	*1.000***	1.000***
12.—'95-'96. Agusta-Dassault/Dutroux Affairs	-.0002	*.097*	.083
France			
1.— '18. End of First World War	-.0004	*.278*	.333
2.— '34-'36. Popular Front formation events	-.0006	*.417*	.444
3.— '39-'45. Second World War	.0011	*.333*	.319
4.— '47. Unity Gov't Fall, November Strikes	.0000	*.167*	.167
5.— '54. Dien Bien Phu and aftermath	-.0010+	*.472*	.472
6.— '58. Algiers Putsch, Fifth Republic Est.	.0001	*.125*	.125
7.— '61-'62. Algerian Retreat Crises	.0000	*.236*	.250
8.— '68. Spring '68	.0004	*.417*	.375
9.— '81. Socialist Electoral Victories	.0008*	*.875**	.792
10.—'83. *Tournant de la Rigueur*	-.0017***	*1.000****	1.000***
West Germany			
1.— '18-'20. German Revolution Events	.0017***	*.903*	1.000
2.— '23. Reparation Crises	-.0004	*.153*	.222
3.— '30. Results of 1930 Federal Election	.0014	*.500*	.500
4.— '32-'33. Weimar Downfall Events	-.0008	*.292*	.292
5.— '39-'45. Second World War	.0024	*.569*	.597
6.— '48. Establishment of Federal Republic	.0003	*.250*	.250
7.— '61-'62. Berlin Crisis/Der Spiegel Affair.	.0007+	*.583*	.750
8.— '67-'68. APO Protests	-.0005	*.417*	.417
9.— '72. RAF Terrorism, *Ostpolitik* Crises	.0007+	*.444*	.458
10.—'77. RAF Terrorism: Red Autumn	-.0006***	*1.000****	1.000***
11.—'81. Peace Protests, Corruption Scandals	.0020***	*1.000****	1.000***
12.—'83. *Die Wende* Events.	-.0008***	*1.000****	1.000***
13.—'90. Reunification.	.0007***	*1.000****	1.000***
14.—'00. CDU Financing Scandal	-.0005	*.500*	.500

TABLE 4 (CONT.)
META-ANALYTIC RESULTS FOR EVENT VARIABLES[1]

	Ordinary Least Squares Model		Ordered Logistic Model[2]
	(1)	(2)	(3)
	Avg. Est. Coef.[3]	*Signif. Rate*[4]	Signif. Rate[4]
Italy			
1.— '18-'20. *Biennio Rosso* Events.	.0020**	*.500*	.375
2.— '22. Legalitarian Strike/March on Rome . . .	-.0009	*.500*	.500
3.— '39-'45. Second World War	.0007	*.194*	.181
4.— '48. General Election, Strikes.	-.0004	*.194*	.167
5.— '68-'70. *Sessantotto* and *Autunno Caldo*	.0006*	*.556*	.556
6.— '76-'78. *Anni di Piombo* Events	.0005	*.931***	.917***
7.— '80. Bologna Station Attack.	-.0014*	*1.000***	1.000***
8.— '92-'94. Dissolution of First Rep. Events . . .	.0024***	*1.000***	1.000***
Netherlands			
1.— '33-'34. *DZP* Mutiny/ Jordaan Riot	.0001	*.056*	.056
2.— '39-'45. Second World War	.0002	*.028*	.028
3.— '46-'48. Indonesian Crises	.0005***	*.444*	.444
4.— '76-'77. Lockheed Affair / Glimmen Crisis .	.0012***	*1.000***	1.000***
5.— '81. Euromissile Dissensus	.0009***	*1.000***	1.000***
6.— '83. *Keerpunt* and Second Peace March	-.0012***	*1.000***	1.000***

Significance: +0.1 level; * 0.05 level; ** 0.01 level; 0.001 level.
[1]Excludes war variables.
[2] Coefficients excluded from report due to their incomparability between models.
[3] Significance levels show if the distribution of values were unidirectional at stadard levels of confidence.
[4] Significance levels from Robut Significance Indicator: probability that of a variable being significant in at least 75% of models given observed sigificance rate.

In West Germany, events that are robust predictors of *political talk* include the German Revolution and the consolidation of the Weimar Republic (1918-1920); the terrorist attack campaign conducted by the RAF in autumn '77, which led to the killing of the chairman of the German Business Association (BDA), the hijacking of an airplane, and the collective suicide of the leadership of the terrorist organization; the first wave of massive anti-nuclear protests and the corruption scandals (Flick and soon after Neue Heimat) that shook left- and right-wing political organizations in 1981; the collection of scandals, political crises, economic reform policies and anti-nuclear protests that occurred in 1983, a time that

was then often referred to as *Die Wende*—the "turnaround"—; and finally, the fall of the Berlin Wall and the German Reunification process of 1989-1990.

Robust Italian events include those from the *anni di piombo* (or "lead years") period, which included the 1977 parliamentary inquiry commission into Lockheed kickbacks to Italian politicians, the wave of social contestation that occurred that wave, and the period of terrorism that peaked in the kidnap and murder of former Prime Minister Aldo Moro in 1978; the terrorist attack against the Bologna train station in 1980; and the political murders, terrorist attacks, corruption scandals and political crises associated with the dissolution of the Italian postwar political system between 1992 and 1994.

The remaining robust events are from the Netherlands: the Dutch branch of the Lockheed corruption scandal of 1976 and the terrorist acts staged by Moluccan separatists in the Low Countries between that year and the next; the political polarization caused by NATO's decision to deploy nuclear missiles in Europe—commonly known as "Euromissiles" —and the staging of the first massive anti-nuclear protest in 1981; and the second anti-nuclear protest of 1983 and the implementation of neoliberal austerity policies by newly appointed prime minister Ruud Lubbers in 1983.

From these events, all but one (the events of the "lead years" in Italy between 1976 and 1978) are unidirectional predictors at standard levels of confidence. The range of their effects on *political talk* goes from 0.07—similar to the impact of yearly growth per capita—to 0.24—a figure comparable to the effects of income.

Nine political contingencies are associated with positive cohort increases in *political talk*. In ascending order of strength of influence, they include the Black Sunday in Belgium ('92); German Reunification ('90); the socialist electoral victories in France ('81); the 1981 Euromissile dissensus in the Netherlands; the *redressement* events of Belgium ('80-'81); the

Dutch Lockheed and Moluccan terrorism crises of 1976-1977; the German Revolution events of 1918-1920; the German peace protest and corruption scandals of 1981; and the dissolution of the Italian Second Republic in 1992-1994.

Six events, on the other hand, are negative predictors of *political talk*. They are, from minimum to maximum effects, the RAF terrorist acts ('77) and the *Wende* events of Germany ('83); the breakup of the Egmont Pact in Belgium ('78); the Peace March and the introduction of neoliberal policies in the Netherlands in 1983; the Bologna train station attack (Italy '80); and Mitterrand's neoliberal turn in 1983. Excepting the events from the Belgian *Redressement* period, all the events associated with neoliberal turnarounds at the beginning of the eighties form part of this set.

These results show that several political contingencies can robustly affect *political talk*, but they still give an incomplete picture of general trends across events in robustness and direction of influence. I examine these results with the help of Table 4, which shows cross-regression statistics for groups sharing characteristics related to their intensity and prospective influence. Columns 1 and 2 show the average significance rate for each of these groups, as well as the correlation between group adscription as well as its significance at standard levels. Columns 3 and 4 show equivalent figures for average estimated coefficients.

How related are measures of an event's intensity to its robustness and direction of influence?

Table 5 shows that significance rates are only weakly related to the duration of an event. The correlation between these variables (0.204) is non-significant and small in magnitude. With respect to average coefficients, the duration of an event follows a stronger, positive, and significant correlation. This suggests that longer events tend to be positively associated with *political talk*.

TABLE 5
VARIABLES OF EVENEMENTIAL EXPOSURE: META-ANALYTICAL PARAMETERS BY GROUP[1]

		Rate of Significance		Average Est. Coefficient.	
	(1)	(2)	(3)	(4)	(5)
Attributes	*n*	Mean	Corr.[2]	Mean	Corr.[2]
Characteristics related to an event's synchronic strength					
A.—Duration (months)	43	.605	.204	.00014	.381*
B.—*Experiential intensity*					
Moderate Intensity	21	.650	.137	.00008	-.064
Higher: terrorist acts, economic crises	15	.572	-.073	.00008	-.046
Highest: generalized political violence	7	.538	.571	.00047	.147
C.—*Dimensionality* [3]					
One	10	.720	.198	.00021	.038
Two	13	.503	-.207	-.00032	-.315*
Between 3 and 5	17	.624	.049	.00026	.101
Six or more	3	.546	-.049	.00124	.311*
D.—*Political disruptiveness*					
No government fall	13	.579	-.052	.00020	.039
One government fall	16	.606	.005	.00004	-.081
Two ore more gov't falls	14	.625	.046	.00020	.045
Characteristics related to recency and political influence					
E— Recency (years; base: 2002)	43	.605	.510***	.00014	-.001
F .—*Effectiveness*					
Produced lasting political turnarounds	8	.831	.336*	.00014	.095
Did not	35	.553	-.336*	.00015	-.095
By main type of disruption					
G.—Insurrections	2	.514	-.027	.00089	.148
Terrorist acts	2	1.000	.274+	-.00098	-.240+
Protests and strikes	8	.461	-.128	.00053	-.056
Corruption and malpractice scandals	2	.299	-.161	-.00033	-.105
Political crises	11	.548	-.017	-.00035	-.286*
Elections	4	.642	.075	.00063	.135
Multiple	14	.720	.307*	.00056	.243+
Wars[4]	7	.276	-.347*	.00040	.092

[1] Excludes **w**ar variables.
[2] p-value testing hypotheses of no correlation. * Significant at the 0.01 level; ** Significant at the 0.05 level; ****Significant at the 0.001 level.
[3] Number of narratively independent sequences of disruptive political occurrences.
[4] World Wars I and II.

My results show little relationship between significance rates and experiential intensity. Mild events hold higher average SRs and average estimated coefficients than events of moderate and high experiential intensity.

With respect to narrative dimensionality, significance rate follows, if anything, a negative relationship: the most narratively simple events tend to be significant predictors of *political talk* more systematically. Regarding average estimated coefficients, events with two narrative sequences and the most narratively complex events—those with six sequences or more— exhibit significant negative and positive correlations. Overall, however, no clear association is visible between narrative diversity and estimated coefficients' figures. Political disruptiveness is not meaningfully associated with average significance rates or estimated coefficients either. Significance rates grow from events that led to no government falls to those that provoked two or more, but the magnitude of this increase is very small. My results also suggest that average estimated coefficients follow a quadratic relationship across the number of government falls provoked by an event. However, none of the event groups related to government falls was significantly correlated with this parameter.

Are cross-regression parameters more strongly organized, on the other hand, by recency and consequentiality?

My results suggest that this is indeed the case. The correlation between significance rates and the year of occurrence of an event is positive, significant, and the strongest of all the groups and event characteristics I examine.

Results also show that events that suddenly changed the direction of state action (for example, the German Revolution or the *Wende*, *Keerpunt,* or *Tournant de la Rigueur* neoliberal turnarounds in Germany and Belgium) are much more frequent predictors of political talk.

On average, they are significant in 83.1% of the regressions. Membership in this group, on the other hand, shows no strong relationship with estimated coefficients.

Finally, I evaluated how different types of events held different kinds of associations with *political talk*. Several exhibit relationships with significance rates or average coefficients worth noting.

Variables encompassing multiple types of events are also correlated significantly and positively with both significance rates and estimated coefficients. On the other hand, the two events related mainly to acts of terrorism—which tend to produce rallying effects (Hetherington and Nelson 2003; Dinesen and Jaeger 2013)—manage to be significant in all the regressions I conducted and hold a significant correlation with significance rate. They also are significantly and negatively correlated with average coefficient values. These results are suggestive, but except for events related to multiple types of disruptions, the very small number of events included by these sets of events greatly limits their inferential power.

Events mainly related to political crises, on the other hand, show no strong relationship with significance rates, but they are negatively and significantly correlated with estimated coefficients.

Results by type of events also exhibit interesting negative findings. Corruption scandals, for instance, exhibit much lower significance rates than the global event mean. (Similarly, variables of exposure to world wars exhibit such smaller SR figures that they are significantly correlated negatively with this parameter, thus indicating that these variables are poor predictors of *predictor talk* in the long run.)

DISCUSSION AND CONCLUSION

Recent influential voices within historical sociology have singled the study of how and when events come to have durable and transformative consequences as an important line of inquiry for social-historical studies (Sewell 1996; Clemens 2007, 528, 541; Wagner Pacifici 2017). Simultaneously, our current times—impregnated as it has been with major bursts of political disruptions across the globe—have also added substantive relevance to this inquiry. After the relative historical restraint of the nineties, unforeseen and powerful political contingencies have taken center stage in contemporary times across the globe and call for a renewed scholarly engagement in the production of fine-grain knowledge on the legacies that historical events have on individual-level political behavior.

My investigation sought to contribute to moving the literature in this direction. Beyond verifying whether event effects on political attributes exist, it focused on studying how, how much, and how frequently moments of political contingency can exert lasting cohort impacts in a foundational political attribute like political engagement. Departing from generational approaches to event effects, I proposed to understand these heterogeneities through the lens of a diachronic outlook to event effects that sees event influences on political behavior as less related to what a historical contingency makes when it occurs than in the micro- and macro- political legacies it leaves when it is no longer active. I tested hypotheses derived from this outlook, as well as others related to generational research and lines of research skeptic of event effects, by exploring how exposures to 34 periods of heightened political contingency from Belgium, France, West Germany, Italy, and the Netherlands were associated with persistent cohort differences in frequency of political talk, an everyday behavioral measure of political engagement, from 1973 to 2002.

My analysis produced findings that allow evaluating the hypotheses I identified on how events persistently affect politicization levels. Table 6 provides a summary of the empirical traction of these hypotheses against the backdrop of my results and a brief description of the key evidence produced by my investigation to sustain them or disconfirm them.

My findings do not find supportive evidence of Hypothesis 1, which considers levels of political engagement insensitive to historical socialization processes. Event variables and cohort levels of educational attainments were both significant predictors of *political talk* (the impact of the latter, however, ran in the opposite direction to the one espoused by the literature). Similarly, they did not find support for Hypothesis 2, which expected that once controlling for the broader historical socialization factors, historical events would not be significantly associated with political discussion. Sixteen events were robust predictors of *political talk* in the presence of cohort-level controls. Substantively, the magnitude of influence of these contingencies was not marginal. The range of their predicted impact was similar to the one related to gdp growth per capita, and in some cases it was comparable to income's.

Besides showing that major political contingencies can persistently impact levels of politicization, my findings also allow evaluating hypotheses on how this impact takes place.

Generational hypotheses perform poorly as explanatory arguments of evenemential influence on political discussion. Hypothesis 3a, which predicted that the association between events and political talk would be consistently positive. Of the event variables I analyzed, only a slim majority (23, or 53.4% of the event set) exhibited on average positive associations with *political talk*. Among the events that perform as robust predictors of *political talk*, six have a negative impact.

TABLE 6
EMPIRICAL EVALUATION OF HYPOTHESES

Hypotheses	(1) Empirical Support	(2) Key Evidence
H1.— No historical effects.	No	Event variables and cohort levels of educational attainment are robust predictors of *political talk*.
H2.— No event effects independent of historical socialization contexts. . .	No	16 events variables are robust significant predictors of *political talk* in regressions that include cohort-level controls.
Generational Hypotheses		
H3a.—Positive Effects.	No	20 event variables exhibit negative mean est. coefficients. 9 of them are unidirectional at standard levels of confidence, and 6 are are also robustly significant negative predictors of *political talk*.
H3b.— Temporal Stability of Effects. . .	No	No robustly significant event variable but one occurred before 1975.
H3c.— Intensity increases robustness of influence.	No	No association between indicators of political disruptiveness, experiential strength, or narrative diversity of an event and its robutness or magnitude of influence on *political talk*.
Diachronic Hypotheses		
H4a.— Fading Effects.	Strong	Strong, positive, and significant correlation between an event's recency and its significance rate.
H4b.— Polarization affects directionality.	Indicative	Events mainly associated with acts of terrorism are negatively and significantly correlated with negative effects on *political talk*.
H4c.— Effectiveness affects robustness of influence. . .	Strong	Positive and significant correlation between an event's capacity to modify state action and its significance rate.

Contrary to Hypothesis 3b, I did not find evidence to support the view that event effects go unaffected by time. On the contrary, they suggest that the newness of an event is an important factor in making it have an impact on politicization. The recency of an event is positively and significantly correlated with significance rates. In addition, excepting the German Revolution events, events that occurred before the seventies fail to be systematically associated with levels of politicization.

Finally, I found that events that scored high in political disruptiveness, experiential intensity, and narrative diversity were no more robust as explanators of *political talk* than events with lower scores. These findings provide little evidence to support Hypothesis 3c, which contends that we should expect an event to be a more robust predictor of political engagement the more intense it manifested in people's experiences when it occurred.

My analysis results align better with the diachronic hypotheses on event effects I introduced in this investigation. Consistent with Hypothesis 4a, which posits that event effects on politicization erode over time, my results strongly indicate that an event's capacity to be a robust predictor of *political talk* diminishes as it fades from present experience. In fact, the relationship between the year when an event occurred and its significance is the strongest among the event characteristics I analyzed.

My results also trend in a direction supportive of Hypothesis 4b, which posits that an event's direction of impact on politicization is positive or negative depending on whether it is polarizing or cohesive. The two events mainly related to terrorist attacks, which typically produce rallying effects, are robustly significant and unidirectional predictors of political talk. On the other hand, events related to insurrections—which are typically related to high levels of political polarization—, are the type of events that exhibits the largest positive correlation with *political talk*. The duration of an event, which can be thought of as more conducive for

the development of political polarization, is also strongly, positively, and significantly associated with political talk.

However, the small quantity of events upon which these results are premised prevents them from being taken as fully conclusive. Nevertheless, they indicate a pattern that should be explored more thoroughly by future research.

With respect to directionality of influence, my analysis also found that nearly all the periods of political contingency associated with the implantation of neoliberal economic reforms in the early eighties were robust negative predictors of cohort levels of political discussion. These results suggest that this economic liberalization period played a depoliticizing role as a political socialization environment. The study of this potential role constitutes a relevant avenue for future research.

Finally, my findings produce evidence that is strongly supportive of Hypothesis 4c, which contends that events that generated sweeping changes in state structures of policies tend to be more robust predictors of political talk. This type of events are positively, strongly, and significantly associated with significance rates.

All in all, the results of my analysis strongly indicate that besides being able to impact collective memory processes and issue positions, historical events can also perform as influential socialization factors for foundational political attributes like politicization.

My analysis also produced suggestive evidence on how these influences are organized, indicating that events are not different so much in the *degree* to which they relate to political engagement but in their *logic* of association with it. This association seems to be dynamic and recursively negotiated in connection with people's political changes across the life cycle and with ongoing political developments. The findings that support this contention are based on a research design that modeled event effects not as discontinuous but as graded

across cohorts, that introduced controls for cohort-level factors not related to evenemential exposure, and that centered in the analysis of 360 regressions.

While this research environment provides a more robust research environment to study event effects than the standard approaches that are used to do so, it still carries several inferential limitations related to the small number of events ascribed to several analytically relevant categories of contingencies, and whether these results can be generalized to contingencies from other places and periods. These are relevant caveats that need to be addressed by subsequent research. My study seeks to motivate these future investigations by providing an expanded and enhanced set of theoretical outlooks, research designs, measurement instruments, and empirical results to conduct comparative research on event effects and keep refining our knowledge of the role that historical contingency plays as a political socialization factor. Against the backdrop of the multiple contingencies that have defined the political experiences of recent times, there will be many historical moments and substantive motivation to take on these tasks in coming years.

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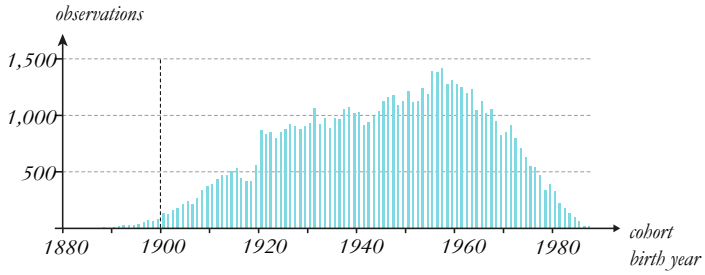
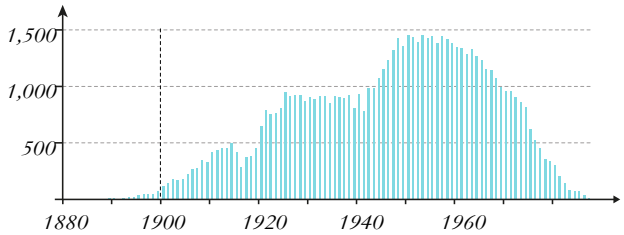
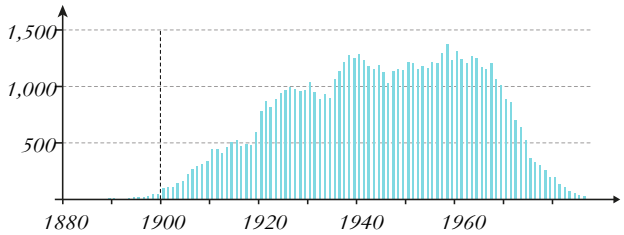
1.— Belgium



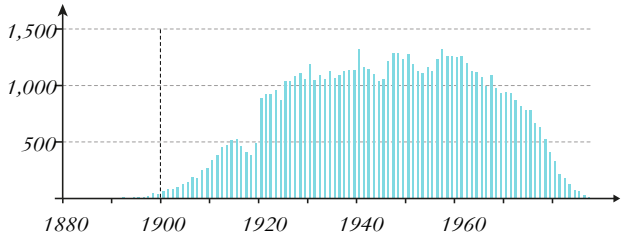
2.— France



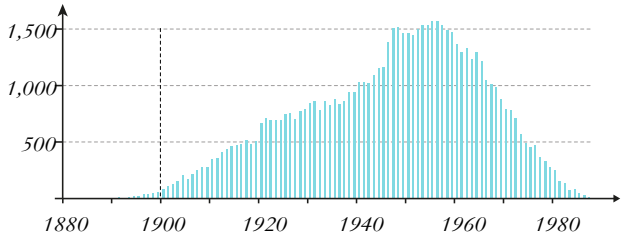
3.— West Germany



4.— Italy



5.— Netherlands



[1] Source: Mannheim Eurobarometer Trend Dataset

APPENDIX B (SAMPLE)
Event Characteristics and Selected Bibliography

Appendix B is not included in this document due to its length. This page presents an excerpt with information from two different events. The complete version of the Appendix can be consulted in
https://www.dropbox.com/s/o43v80pa3gekasi/GalazGarcia_Appendix%20B..pdf?dl=0

Sample 1
1918-1920. German Revolution Events (West Germany)
Period with multiple events

Duration:	19 months.[1]
Peak Beginning:	January 1918: Major strikes against the continuation of World War I erupt in Berlin.
Peak End:	April 1920: The German army breaks into the Ruhr and crushes an uprising of workers in the region.
Narrative Sequences:	January 1918 Strikes; First World War Defeat; Downfall of Second Reich events and German Revolution Events; Kapp Putsch ad Ruhr Uprising.
Government Falls:	Von Baden I; Council of People's Deputies; Scheidemann I ; Bauer I; Müller I.

Selected Bibliography:

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[1] The duration of this evenemential period adds two non-contiguous periods of contingency: the January Strikes, the End of the First World War, and the German Revolution events (January 1918-May 1919); and the Kapp Putsch and the Ruhr Insurrection (March-April 1920).

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Sample 2
Italy
1918-1920. End of First World War and events from the *Biennio Rosso* Period (Italy)
(Period with multiple events)

Duration:	33 months.
Peak Beginning:	January 1918: Last year of First World War
Peak End:	December 1920: Italian army evicts the Italian expeditionary force occupying the Yugoslav city of Rijeka.
Narrative Sequences:	End of the First World War; *Biennio Rosso* strikes and factory occupations; fascists squad violence and Palazzo Accursio Massacre; Fiume/Rijeka occupation crises; Ancona munity; Electoral Results of the 1919 General Election.
Government Falls:	Orlando I; Nitti I; Nitti II.

Selected Bibliography:
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Cioffi, Marco. 2007. *'*24 Maggio 1920. L'eccidio di via Nazionale". *Dimensioni e problemi della ricerca storica* 1: 93-111.
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APPENDIX C

Generation of cohort size and cohort education variables

Theoretically, including *cohort size* and *cohort education* as independent variables required assigning to each 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 80 different cohorts. There are no readily-available data series to measure the size or the level of educational attainment for the 80 cohorts of the 5 countries I analyze. I constructed this series using a variety of historical statistical sources that allowed me to produce minimally consistent measures of cohort size and educational attainment for these cohorts. The paragraphs below discuss how I used these sources to construct indicators of 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* (1975). 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 to 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 s; in 1946, they came from Mitchell (1975). From 1947-1951, I used linear imputations using 1946 and 1951 figures.

Cohort Education

Keeping into account that educational attainment levels have 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 on 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*:

(2)

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

, 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.

From 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 universities, strictly speaking, stopped being reported after 1970. Since tertiary education is a broader category than a university education, I rescaled these figures to make them minimally consistent with *cohort education* figures drawn from earlier year's information

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 D1
META-ANALYTIC RESULTS: ESTIMATED COEFFICIENTS FOR INDIVIDUAL CONTROLS, INCOME AND EDUCATION[1]
OLS MODEL

	Ind. Controls		Income					Education			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Countries	Female	Married	Second lowest	Median	Second highest	Highest	Non Resp.	High School	Some College	College or more	Ongoing
1.— Belgium											
Estimated Coeff.[1]	-0.195***	0.007**	0.032***	0.113***	0.183***	0.078***	0.050***	0.151***	0.312***	0.446***	0.369***
Significance Rate[2]	*1.000***	*0.000*	*1.000***	*1.000***	*1.000***	*1.000***	*1.000***	*1.000***	*1.000***	*1.000***	*1.000***
2.— France											
Estimated Coeff.	-0.147***	-0.027***	0.079***	0.167***	0.256***	0.255***	0.004+	0.182***	0.310***	0.455***	0.460***
Significance Rate	*1.000***	*1.000***	*1.000***	*1.000***	*1.000***	*1.000***	.000	*1.000***	*1.000***	*1.000***	*1.000***
3.— W.Germany											
Estimated Coeff.	-0.208***	0.040***	-0.007+	0.036***	0.063***	0.083***	0.036***	0.112***	0.213***	0.306***	0.275***
Significance Rate	*1.000***	*1.000***	*1.000*	*1.000***	*1.000***	*1.000***	*1.000***	*1.000***	*1.000***	*1.000***	*1.000***
4.— Italy											
Estimated Coeff.	-0.300***	-0.027***	0.061***	0.096***	0.157***	0.173***	0.260***	0.238***	0.297***	0.420***	0.402***
Significance Rate	*1.000***	*1.000***	*1.000***	*1.000***	*1.000***	*1.000***	*.944***	*1.000***	*1.000***	*1.000***	*1.000***
5.— Netherlands											
Estimated Coeff.	-0.057***	0.037***	-0.172***	-0.051*	0.073**	0.131***	-0.080***	0.156***	0.278***	0.359***	0.389***
Significance Rate	*1.000***	*1.000*	*1.000***	.708	.347	*1.000*	*1.000***	*1.000***	*1.000***	*1.000*	*1.000***

Significance: +0.1 level; * 0.05 level; ** 0.01 level; 0.001 level.
[1] Significance levels show if the distribution of values were unidirectional at stadard levels of confidence.
[2] Significance levels from Robut Significance Indicator: probability that of a variable being significant in at least 75% of models, given observed significance rate.

APPENDIX D2
META-ANALYTIC RESULTS: ESTIMATED COEFFICIENTS FOR AGE, RESIDENCE AND PERIOD VARIABLES, AND CONSTANT[1]
OLS MODEL

	Age		Residence							Period		Cons.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Countries	Age	Age Sq.	Rural Residence	. . . . . Regional Controls.						Election	GDP growth	Ongoing
Belgium				*Flanders*	*Wallonia*							
Average Coef.	0.015***	-9.6e-5***	-0.027***	-0.082***	-0.023***	—	—	—	—	0.016***	0.031***	-0.032
RS:	*1.000*	*1.000*	*1.000*	*1.000*	*1.000*	—	—	—	—	*1.000*	*1.000*	*.486*
France				*North*	*NW*	*East*	*West*	*SW*	*SE*			
Average Coef.	0.016***	-1.5e-4***	-0.049***	-0.106***	-0.036***	-0.046***	-0.033***	-.0047***	-0.003	0.020***	0.106***	0.567
RS:	*1.000*	*1.000*	*1.000*	*1.000*	*1.000*	*1.000*	*1.000*	*1.000*	*.000*	*1.000*	*1.000*	*.763*
West Germany				*NW*	*SW*							
Average Coef.	-4.3e-5	-4.7e-5	-0.026***	0.021***	-2.1e-4	—	—	—	—	-6.6e-4	-0.008***	1.154***
RS:	*.333*	*.750*	*1.000*	*1.000*	*.000*	—	—	—	—	*.000*	*1.000*	*1.000*
Italy				*NE*	*Center*	*South*	*Islands*					
Average Coef.	0.023***	1.9e-4***	0.028***	0.008	0.022***	-0.034***	-0.021***	—	—	0.013**	-0.003	0.463
RS:	*1.000*	*1.000*	*1.000*	*.000*	*1.000*	*1.000*	*1.000*	—	—	*.916*	*.514*	*.500*
Netherlands				*West*	*East*	*South*						
Average Coef.	0.013***	1.3e-4***	-0.012**	-0.028***	0.014***	-0.049***	—	—	—	0.047***	-0.005***	0.511
RS:	*1.000*	*1.000*	*.930*	*1.000*	*1.000*	*1.000*	—	—	—	*1.000*	*1.000*	*.777*

[1] Robust Significance Statistic (RSS) significance levels: [+]Significant at the .1 level; * Significant at the .05 level; **Significant at the 0.01 level; ***Significant at the .001 level

APPENDIX D3
META-ANALYTIC RESULTS: ESTIMATED COEFFICIENTS FOR COHORT-LEVEL CONTROLS[1]
OLS MODEL

Countries	(1) 1906-1910	(2) 1911-1915	(3) 1916-1920	(4) 1921-1925	(5) 1926-1930	(6) 1931-1935	(7) 1936-1940	(8) 1941-1945	(9) 1946-1950	(10) 1951-1955	(11) 1956-1960	(12) 1961-1965	(13) 1966-1970	(14) 1971-1975	(15) 1976-1980	(16) 1981-1985	(17) 1986-1987
Belgium																	
Average Coef.	.078***	.130	.133	.083	.102	.052	.105	.175	.200	.247	.221	.150	.179	.167	.150	.041	-.033
RS:	*1.000*	*.750*	*.611*	*.167*	*.361*	*.111*	*.361*	*.056*	*.583*	*.694*	*.639*	*.361*	*.500*	*.389*	*.333*	*.139*	*.000*
France																	
Average Coef.	.023	.125	.020	.040	.234	.221	.242	.239	.266	.289	.312	.292	.237	.263	.190	.055	-.159
RS:	*1.000*	*.000*	*.000*	*.000*	*.500*	*.500*	.528	*.500*	*.611*	*.722*	*.861*	*.694*	*.500*	*.611*	*.500*	*.583*	*.333*
West Germany																	
Average Coef.	-.037	-.068	-.081	-.184***	.225	.211	.192	.160	.121	.090	.069	.028	.038	-.011	-.037	-.221	-.387
RS:	*1.000*	*.000*	*1.000*	*1.000*	.250	*.250*	.250	*.250*	*.250*	*.278*	*.444*	*.417*	*.417*	*.333*	*.250*	*.5000*	*.500*
Italy																	
Average Coef.	.081	.120	.153	.187	.289	.295	.331	.381**	.453**	.499**	.500**	.387	.250	.156	.185	.147	.203
RS:	*1.000*	*.000*	*.389*	*.389*	.667	*.667*	*.806*	*.944*	*1.000*	*1.000*	*1.000*	*.806*	*.638*	*.278*	*.306*	*.111*	*.056*
Netherlands																	
Average Coef.	.013	.014	.042	.027	.054	.058	.080	.054	.002	.002	-.024	.001	.007	-1.58e-4	-.075	-.182	-.136
RS:	*1.000*	*.000*	*.000*	*.000*	*.000*	*.000*	*.000*	*.000*	*.000*	*.000*	*.000*	*.028*	*.111*	*.111*	*.333*	*.333*	*.333*

[1] Robust Significance Statistic (RSS) significance levels: +Significant at the .1 level; * Significant at the .05 level; **Significant at the 0.01 level; ***Significant at the .001 level