The Dual Logic of Historical Events' Impact on the Distribution of Political Engagement: Quasi-Experimental Evidence from the German Reunification Period.

Sergio Galaz-García

How are the effects that historical events have on political engagement socially distributed? As an alternative to theories that see the individual-level political impacts of moments of political contingency as top-down processes of historical imprinting, I introduce a set of distributional hypotheses that see these influences as actively mediated by bottom-up processes of historical sensing. I argue that due to socioeconomic and life-cycle differences in political interest, historical events produce increases in politicization whose strength and socially equalizing effects follow and young adulthood gradient. Politicization increases are highest for young people. These increases are organized in a way that shrinks socioeconomic disparities in their political engagement. Other ages, on the other hand, experience more moderate politicization increases that make socioeconomic disparities in political engagement grow. I test these hypotheses by analyzing patterns of political talk in West Germany relative to France before and during the German Reunification Period (Nov '89-Dec –'90). Using an original indicator that measures yearly levels of eventfulness, I identified this context as a quasi-experimental research setting. The results of my analysis strongly support my argument of perception-based distributional effects of historical events. They indicate that moments of historical contingency carry distinctive and intersectional logics of influence across age and socioeconomic status. They also call for further research on how people make sense and differentially react to the historical contexts they experience.

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INTRODUCTION

Are "historical events"—abrupt and punctuated political ruptures collectively experienced as periods of political contingency—moments of political revitalization? How broadly are they able to increase politicization levels? How do they reshape social disparities in political engagement? Under the historical turbulences of current times, these questions have acquired particular importance. However, they remain unexplored in the literature. Previous studies have focused on studying general event effects on specific issue positions, but we know little of their impact on foundational political dispositions like politicization, or on how these impacts are socially distributed. In this paper, I seek to contribute to shed light on these issues.

I begin by identifying distributional hypotheses on how events affect political engagement. I derive two of them from evenemential and generational/Bayesian updating theories of event influence. They consider these influences primarily as top-down socialization processes directly generated by how the historical weight of events imprints onto people's political orientations. Evenemential research regards historical contingencies as moments of widespread political activation that at least do not increase social inequalities in political engagement. A generational outlook to event effects, on the other hand, suggests that levels of politicization shift only across the life cycle because events produce political engagement solely for people that come of age. Another variant of this approach also

suggests that socioeconomic disparities in politicization increase for young adults when an event occurs because educated youth tends to be particularly sensitive to history.

In addition to these "imprinting" hypotheses, I introduce a new set of expectations on how event influences on politicization are socially distributed. This original outlook brings attention to how perceptual characteristics filter how individuals perceive historical events. This "perception-based" outlook contends that event influences on politicization are determined by how large and elastic the political interest of a person is and by her level of political environmental attention—that is, how much she observes and thinks about ongoing environmental political information. As a result of how these factors vary across age and socioeconomic status, I propose, first, that an event produces generalized gains in politicization that peak in magnitude during young adulthood; and second, that events decrease socioeconomic disparities for young adults while increasing them for the rest of the population.

I test these three sets of hypotheses in a research environment that allows evaluating them under a quasi-experimental environment that is more inferentially robust than the ones used by earlier research. Previous studies have evaluated how politicization changed in a country before and after it was treated with an event. Rather, I evaluate these changes relative to equivalent variations in a control country that experienced no historical contingency in neither of these times. Through the development and deployment of an original quantitative indicator of historical eventfulness, I identified such empirical setting in West Germany and France before and during German Reunification (November 1989 – December 1990). Immediately before this period, these countries exhibited low degrees of historical contingency. But after the fall of the Berlin Wall in November 1989, West

Germany experienced a high degree of historical contingency, while in France it remained very low.

My analysis results support perception-based distributional hypotheses and disconfirm generational and evenemential distributional propositions. I found that relative to France, the Reunification period in Germany was associated with widespread increases in politicization. These increases followed an age gradient centered in young adulthood, diminished political engagement disparities between young people with and without college experience, and increased socioeconomic disparities in this attribute for everyone else. These findings suggest that event influences on politicization follow more graded distributional logics across the life-cycle than the "youth bump" expected by generational literature, and that historical sensitivity should be understood as shaped as much by socioeconomic status than as by age.

The paper is divided into five sections. I first discuss and identify evenemential and generational hypotheses on how event effects on politicization are socially distributed, and introduce a set of perception-based hypotheses on this issue. The second section discusses the research design and the analytical strategies of my investigation. The third section presents the results of my analysis. Finally, the fourth discusses the implications of these results for the hypotheses I test, recapitulates my investigation's contributions, and discusses directions for future research.

DISTRIBUTIONAL THEORIES OF EVENT EFFECTS ON POLITICAL ENGAGEMENT.

Previous investigations on event effects have mainly been oriented at investigating their influences on people's attitudes towards specific political conjunctural issues (Dinas 2013; Legewie 2013; Perrin and Smolek 2009; Hopkins 2010). However, they have largely overlooked examining how they might affect levels and disparities of political engagement, a foundational line of inquiry for contemporary studies on political socialization (Verba and Nie 1978; Jacobs and Skocpol 2005). Nevertheless, several hypotheses on these issues can be derived from evenemential and generational theories of event effects.

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

Evenemential approaches to event effects connect with pioneer historical sociological investigations that have proposed seeing "historical events" as sequences of interrelated political disruptions collectively experienced as emergent political contingencies, leading to durable transformations of political structures (Sewell 1996; Wagner Pacifici 2017). These transformations are often described as leading to moments of full-fledged political engagement. For example, in his influential description of the Storming of the Bastille as an event, William Sewell describes how this takeover "revealed itself in the days that followed [it] as a concrete, unmediated, and sublime instance of *the people* [my emphasis] expressing its sovereign will [Sewell 1996, 852])". In connection with this understanding of what an event is and does, evenemential investigations conceive event influences on individual-level political attributes as overarching, uniform in direction, and having neutral-to-progressive consequences in the social distribution of political engagement (Pennebaker and Haber 1993; Dinisen and Jaeger 2013):

> *Hypothesis 1: Evenemential surge.* The occurrence of an event increases politicization across the board in a way that, at minimum, does not widen pre-existing inequalities in political engagement.

Generational research, on the other hand, sees event influences on political attributes as restricted to young adults due to Bayesian processes of political updating (Bartells and Jackman 2014; Ghitza and Gelman 2014). It contends that events can only reshape the political attributes of individuals that are coming of age because they lack political experience and are making "fresh contact" with history (Mannheim 1952, 253 Weil 1987, 309; Jennings and Niemi 1981; Schuman and Rodgers 2004; Osborne, Jennings and Valentino 2011). By contrast, later ages deny an event a capacity to transform political attributes because they are already saturated with political memories. In consequence, in these life periods people will give the occurrence of an event a marginal role in determining their political orientations.

Given that the young typically tend to engage in politics less than mature adults, generational outlooks to event effects would expect that a moment of political contingency would reduce disparities between these age groups—and therefore, carry only distributional implications across age:

> *Hypothesis 2: Bayesian youth bump.* An event's occurrence only has a politicizing impact on young adults. Consequently, it only diminishes disparities in political engagement between these groups and mature adults.

Another generational theory with distributional implications concerns the formation

of "generation units"—groups of young people with high levels of cultural capital and political familiarity that react to a historical event by intensely reshaping their political orientations (Mannheim 1952, 253; Jennings 1987; Fendrich and Lovoy 1988). To the extent that cultural capital and political expertise are associated with higher levels of educational attainment, the formation of generational units when an event occurs would carry the following expectation:

> *Hypothesis 3: Generational unit formation.* The occurrence of an event will increase socioeconomic gaps in political engagement for young adults.

A perception-based theory of event effects

Despite having different distributional implications, both evenemential and generational outlooks to event effects view them as influences that develop almost mechanically out of their emergence. In their current state, they overlook discussing the specific processes through which people attend, make sense, and react to moments of historical contingency. This omission characterizes event influences as top-down processes of "imprinting" that are straightforwardly determined by the very emergence of a historical contingency. This characterization is particularly patent in evenemential research. It is also identifiable in generational investigations, for which the strength of event effects depends not on how people see history but merely on how much history they have experienced before. Taking individuals as mere passive recipients of history, these positions align to a conception of historical socialization that gives precedence to the mere "occurrence of history" over the actual "experiencing" of it.

As an alternative to these "imprinting-based" positions, I propose an alternative approach to event effects that see them as processes generated as much by the emergence of a large-scale political contingency as by political-perceptual conditions that filter them into people's concrete experience.

The first of these filtering factors is political interest. People that are more interested in politics will be better able to attend and identify an event as a distinctive political stimulus and consider it an important issue to think about. Consequently, they will also become more politically engaged.

Once filtered by political interest, I contend that the politicization gains that people make out of an event's occurrence will be shaped by two additional political sensing features. One is the elasticity of political interest. The comprehensive, contingent and disruptive nature of an event challenges understandings of the everyday as a de-politicized realm of experience (Bourdieu 1972). Consequently, if a person attends a political contingency when she has fluid opinions on how consequential politics is, she will be more likely to adjust her perception about how important politics upwards and become more politically engaged.

The second factor that regulates the effective magnitude of event influences in politicization is the degree to which a person engages in political environmental attention. The more frequently a person seeks and uses information on ongoing issues to form her political opinions (as opposed to, for example, using textbook knowledge about politics, ideological positions, or previous political memories), the more likely an event will politicize her.

Identifying these factors as modulators of an event's ability to change politicization levels does not challenge the expectation that events produce generalized increases in political engagement. Events are phenomena that powerfully increase incentives for political

attention. They challenge people's understandings of politics and its relationship with their everyday. And by concentrating political attention into a single stimulus, they generate a political subject of common interest to talk about. These characteristics make politics more interesting to attend when an event occurs—which will drive political engagement up—regardless of how much people were politically engaged in the first place.

On the other hand, the way political interest, the elasticity of this political attribute, and engagement in environmental political attention change across social characteristics make the perceptual theory of event effects I sketched below carry particular distributional implications.

Like generational research, I contend that young adults are the age group that makes the largest evenemential gains in politicization. This expectation, however, is based less on considering youth as a stage of historical innocence and inexperience than on seeing it as time endowed with sensory characteristics that make it particularly ductile in political terms. In liberal democracies, enfranchisement increases political interest at this life stage (Zeglovits and Zandonella 2011). And in addition to having a short stock of political memories, young adults also carry flexible political positions that have not been fully crystallized (Jennings and Niemi 1981: 113; Dinas 2013). They also take part in interaction foci that are more politically loaded than the ones they frequented as teenagers (Bidart and Lavenu 2005). These conditions make young adults exhibit a high degree of elasticity in their political interest levels and a relatively high level of political environmental attention. As a result, I contend that an event can generate gains in politicization not only for young adults that are already interested in politics, but also for those who were previously politically apathetic. In consequence, young adults as a whole will undergo large increases in political engagement

when an event occurs, but will do so in a way that will decrease politicization gaps across education.

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

On the other hand, adolescents have not yet experienced the bonus of political interest that comes with political enfranchisement. They socialize in environments carrying a weak political load, and their levels of political interest tend to be low, more volatile, and tend to reflect politicization patterns inside the domestic household (Jennings and Niemi 1974). In consequence, politicization gains in this group will tend to be lower than for young adults, and will also tend to increase socioeconomic disparities in political engagement.

Summarizing, the discussion I presented above proposes the expectation:

> *Hypothesis 4. Perceptual filtering.* An event's occurrence will produce generalized increases in political engagement whose magnitude will peak during young adulthood. It will also increase socioeconomic disparities for the population at large but decrease them for young adults.

RESEARCH DESIGN AND CONTEXT

Target Research Design

Currently, the most robust evaluations of event effects in the literature use quasi-experimental research designs that compare an outcome of interest before and after a polity was "treated" with a historical condition external to it. Legewie, for example, evaluates changes in opinions towards immigration in European countries before and after the 2002 Bali terrorist attack (Legewie 2013). Van der Brug, on the other hand, explores shifts in positions on nuclear energy in the Netherlands before and after the Chernobyl nuclear disaster (Van der Brug 2011).

In the terminology of experimental design, these investigations conduct a one Pre-Test Post-Test (PP) quasi-experimental design (Shadish, Cook, and Campbell 2002):

$$O_1^A \; X^B \; O_2^A \; . \qquad (1)$$

While this design controls for inferential issues related to endogeneity, it is still exposed to other types of threats (see Shadish, Cook, and Campbell 2002, 108 for an extensive discussion). Three are particularly important for studies on historical socialization. The first is its coarse-grained theoretical resolution. It uses event treatments from places physically and historically unconnected to the sites where observations were collected. This outlook associates "event effects" with a watered-down understanding of historical events as mere synonyms of "exemplary pieces of information." Second, their results are prone to ambiguous temporal precedence. They assume that the period before the event treatment they study was devoid of political contingency without showing that was effectively the case. Third, they are sensitive to internal validity challenges related to history in both the

experimental (*Ibid, 55)* and the properly historical sense of the word. Because they do not measure dependent variable changes in a control country, they cannot disentangle variations related to long-term historical processes—for example, the end of the Cold War, or the implantation of neoliberalism as a dominant economic outlook—from those associated with the occurrence of a punctuated political contingency.

An analytic setting that addresses these limitations is a Pre-Test Post-Test Control Group (PPC) Quasi Experimental Design (*Ibid,* 137):

$$\frac{\cancel{X}\ O_1^T \quad X^T \quad O_2^T}{\cancel{X}\ O_1^C \quad \cancel{X} \quad O_2^C} \ . \qquad (2)$$

This setting compares how a dependent variable in Country T changed between a period of no contingency (the crossed-X) and a subsequent moment treated with an event with respect to the equivalent variation in a control country C that exhibited null levels of contingency in both of these periods.

Identifying a research environment close to the quasi-experimental conditions I just described requires instruments to measure and compare historical contingency degrees between periods. We currently lack these instruments. Historical contingency in a given period is measured by the literature in terms of whether a historical event happened in it or not. This dichotomous metric can accurately identify moments of high historical contingency, but it cannot distinguish times that near "zero" values of this attribute from others when conjunctural issues were important but not sufficiently so to be collectively experienced as a defining moment of political discontinuity. As an alternative to this raw metric, I develop an indicator that measures historical contingency continuously.

Measuring Eventfulness

Based on the finding that stronger occurrences tend to be covered longer and more frequently by media (Oliver and Maney 2000), my indicator of historical contingency—henceforth "eventfulness"—measures this temporal quality by calculating how concentrated media coverage was across on-going political issues at a given time. It uses the following formula:

$$eventfulness = \frac{ev_t}{ENE_t} = \frac{ev_t}{[1/\sum_{i=1}^{n}\left(\frac{ev_{it}}{n_t}\right)^2]}, \qquad (3)$$

where n_t refers to the aggregate number of newsweekly covers dedicated to developing political stories in a year, and ev_{it} to the number of covers that each of these issues received.

The denominator is the "Effective Number of Events" (ENE). It adopts the logic of the Laakso-Taagepera index of party fragmentation (Laakso and Taagepera 1979) to calculate the number of ongoing political situations that would gain equal coverage as cover stories conditional on observed inequalities in their coverage. For instance, if newsweeklies covered only three political conjunctural topics in a year, and each became a cover topic on average 15 times, the ENE would equal 3. If, on the other hand, only one topic had been covered as a headliner 15 times and the rest only 2, the ENE value would decrease to 1.54). I use the ENE to divide ev_t —the number of times that newsweeklies published developing political stories as cover topics in a given year. *Eventfulness,* the resulting number, can be interpreted as

the average amount of weeks that an event was featured as a cover topic in a year, controlling for coverage inequalities across them. Larger numbers indicate higher eventfulness. Zero-values indicate that no current topics were headlined.

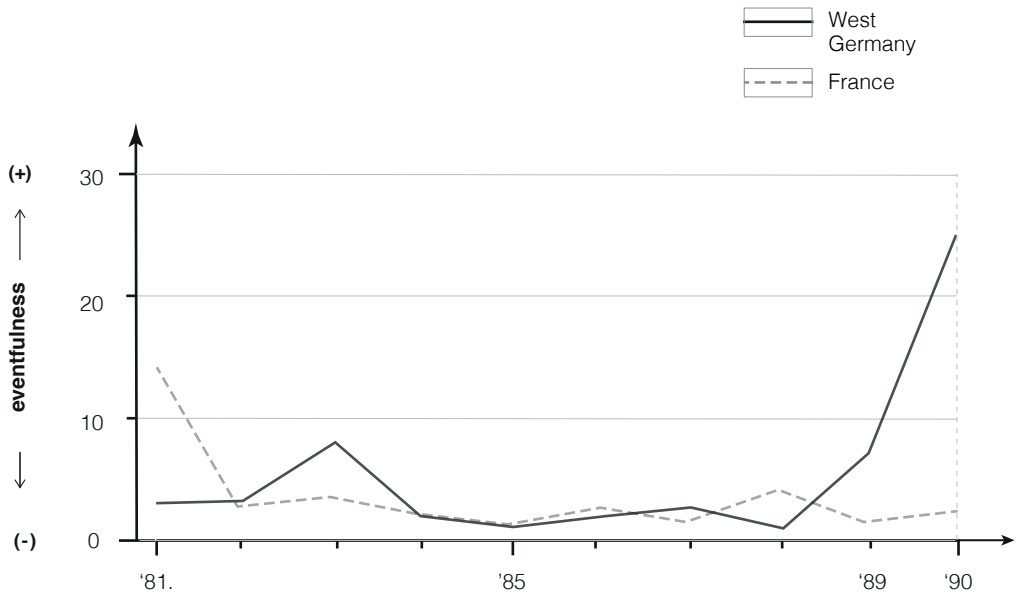
The German Reunification Period in West Germany and France as a robust quasi-experimental context

To explicitly identify a research context close to a PPC quasi-experimental analysis, I calculated *eventfulness* figures in West Germany and France between 1980 and 1990 using an original database on cover topics of *Der Spiegel* and *Le Nouvel Observateur*.[1] Figure 1 plots these values across the decade I inspect. In these periods, West Germany and France were comparable in many important attributes. Between 1975 and 1990, the correlation on GDP per capita variations between these two countries was 0.997, and the one on the average schooling years of their population 0.984. Both countries also underwent similar politico-economic trends throughout the eighties: they faced a steep economic crisis at the beginning of the decade, implemented neoliberal reforms to tackle them, and experienced a long period of government stability afterward.[2]

[1] In the period I inspected, *Der Spiegel* was the biggest-circulation and most influential West German newsweekly (Conradt and Langenbacher 2013). In France, *Le Nouvel Observateur* was also commonly perceived as the most influential newsweekly together with *L'Express*, and it was second to it in terms of circulation. I selected *Le Nouvel Observateur* over *L'Express* because its political orientation was roughly similar to *Der Spiegel*'s, and because the one from *L'Express* was fluid over the time I analyzed (Moores 1998a, 1998b).

[2] Data on GDP per capita comes from the World Bank; figures on average schooling come from De la Fuente and Doménech 2012. Unemployment and strike rates are also similar between France and West Germany (OECD 2020, Bordogna 2010). West Germany and France exhibited differences, on the other hand, concerning their political systems. West Germany was a federal parliamentary republic with a stable and little-fragmented party system, while France held a presidential regime with a much more fractured and fluid collection of political parties. For my investigation, it is unclear how these differences might strongly affect comparability between these two countries.

FIGURE 1
Yearly *Eventfulness* Values[1]
France and Germany, 1981-1990



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

In West Germany, the eighties start with low *eventfulness* values. They then undergo a moderate peak in 1983, when Helmut Kohl, the chairman of the German demochristian party (CDU), became chancellor after fourteen years of socialist governments. It then exhibited minimal levels up until 1988. That year *eventfulness* equals one, its minimum possible value: in that year, no current issue became a headline story more than once. But the next year, despite the considerable fragmentation of coverage observed in its first three quarters (see Appendix A), *eventfulness* jumps to 7.08. This high figure is almost entirely related to the concentration of media coverage towards a single topic after November 1989. At this point, cover stories began to concentrate on the East German political and migratory crises that forced the opening of the Berlin Wall and the whole inner German border, precipitated the

implosion of the coercive apparatus of the East German state, and started to make possible something that until then seemed to belong to wishful thinking: the possibility of German reunification (Bozo, Rodder and Sarotte 2017; Hirschmann 1993).

This possibility ended up being vertiginously materialized. Throughout 1990 Helmut Kohl was able to solve at a remarkable speed thorny issues whose successful resolution for the reunification cause was highly uncertain at the time. These issues included negotiating a monetary union aligned with the interests of West Germany and the needs of its Eastern counterpart, securing politically satisfactory terms of unification to both East and West German constituencies, and persuading the nuclear powers not to oppose a unified German State. By the second half of 1990, these matters had all been worked out. East and West formally merged on October 3. A few weeks later, the first all-German democratic elections conducted since 1932 took place on December 2. During this year, the many subplots of German Reunification became *Der Spiegel* cover topics 29 times—a little bit more than seven months—. Accordingly, the 1990 value of *eventfulness* skyrocketed to 24.941, an all-time high for both the eighties and the nineties (results available upon request).

These figures indicate that for West Germany, the 14 months that separate the Fall of the Berlin Wall in November 1989 from the December 2 all-German elections—a period I will call "Reunification"—can be considered a "treatment" period. In this year, West Germany underwent an abrupt moment of political contingency that originated exogenously from its domestic political conditions. In addition—and equally importantly— West German *eventfulness* patterns through the eighties also show that the 14-month period that preceded

Reunification (which I will call "pre-Reunification") held minimum values of historical contingency, and therefore, can be considered a robust pre-test period.[3]

In France, the eighties started with high *eventfulness* values generated by unprecedentedly large Socialist victories in the presidential and parliamentary elections of 1981. After that, values for this statistic rapidly diminished to near-1 levels. *Eventfulness* reaches the somewhat large figure of 4.42 in 1988, when regular Presidential and Legislative elections took place. Taking these pre-scheduled occurrences out of consideration, this year, the value of *eventfulness* equaled one. In 1989, the value of this estimator was 1.5, and the following year it kept being close to the unity level (2.5). These numbers indicate that France can be considered a control country in these time periods, as it exhibits low *eventfulness* values in them.

In this investigation, I leverage these conditions to test distributional hypotheses on event effects on politicization using the following research design, which approximates PPC research design:

$$\frac{\cancel{X}\ O_1^{Ge}\ (\text{Reunif.})\ O_2^{Ge}}{\cancel{X}\ O_1^{Fr} \quad \cancel{X} \quad O_2^{Fr}}. \qquad (4)$$

[3] For my investigation, German Reunification also has the additional advantage of providing a rough but useful control for political memories experienced during youth. In 1989, the youth experiences of not only young adults, but also of seniors and adults in West Germany were informed to some degree by occurrences and events related to German Unity: the *de facto* partition of the country in 1945, the 1948 Berlin crisis, the construction of the Berlin Wall in 1961, and the implementation of *Ostpolitik* and the political crises it triggered in 1972 (Winkler 2007).

ANALYTICAL STRATEGY

The pre-test/post-measurements I conduct to test my investigation's hypotheses concern the statistical performance of socio-demographic covariates as regressors of political engagement.

Data, Variables, and Model Specification

Data and Dependent Variable. The data I analyze comes from Eurobarometer survey responses. The dependent variable is a question measuring frequency of political talk, an everyday behavioral measure of political engagement: "When you get together with friends, would you say you discuss political matters frequently (2), occasionally (1), or never (0)?" This question was included in five Eurobarometer waves during Reunification and in three in its preceding period; the temporal location of each is shown in Appendix B. These waves provide data on political talk for 8,406 respondents aged 15 and up for West Germany (3,216 before and 5,190 during Reunification), and 7,994 for France (3,031 and 4,963, respectively).

Key independent variables. I measured age, educational attainment, and income categorically. I measured age in terms of membership to one of four age brackets: 15 to 19 years (which I refer to as adolescence), 20 to 29 years (youth), 30 to 59 years (adulthood), and 60 years and more (seniority). For income, I generated variables indicating adscription to one of five monotonically increasing earning brackets.[4] To measure educational attainment, I generated four categories indicating education termination: *below high school*; (education

[4] Information on lower and upper values of income brackets is provided in Appendix B.

finished at 15 years old or less); *high school* (finished between 16 and 18 years old)*; some college* (finished between 19 and 21 years)*;* and *complete college* (finished at 22 years old or more). I also included a variable indicating *ongoing studies.*[5]

I include these three sets of categorical variables as regressors. The reference category for each is the one associated with the highest level of political engagement: highest income earning, adulthood, and complete higher education (Neundorf and Smets 2017; Mayer 2011; Schlozman, Page, Verba, and Fiorina 2005). This strategy allowed me to interpret variations in coefficient magnitudes as reductions or increases in political talk disparities.

In addition, to evaluate socioeconomic shifts in political talk across the life cycle, I included interactions indicating whether a teenager, young adult, or senior was still studying or had college experience (1) or not (0).

Controls. I included variables measuring gender (reference = female), rural/urban residence, marital status, regional location, and residence in Paris or Berlin (1) or elsewhere (0).[6]

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

Model Specification. Ordinal categorical variables like political talk are typically analyzed using ordered logistic models. For my analysis, an important drawback of these models is their results cannot be robustly compared due to unobserved heterogeneity. I therefore

[5] These indicators are imperfect measures of educational attainment due to continuing adult education. However, as available research indicates that relatively few people take part in continuing education, and a large part of the ones that do so already have higher degrees of educational attainment (Nuissl and Pehl 2004), I expect measurement error to be marginal.

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

regress political talk using linear models, an alternative that has been recently recommended by recent methodological pieces (Breen, Karlson, and Holm 2018; Mood 2010, 78). The estimated coefficients yielded by these models are unbiased and consistent, and simulation analyses have found them to be nearly identical to average marginal effects derived from ordered logistic models. To control for heteroskedasticity, I use robust standard errors to calculate models' estimates.[7]

Difference in Differences (diff) Analyses: Parameters and Outcomes of Interest

I test distributional hypotheses on event effects by evaluating how different West German changes in the association of political talk with sociodemographic characteristics before and during Reunification were relative to those observed for France. For ease of exposition, I will refer to these difference-in-differences as "diff" or "relative" differences.

I examine these differences by evaluating variations in estimated coefficients associated with sociodemographic characteristics. I first calculate variations in these estimates before and during Reunification for West Germany and France separately and then evaluate if variations between these periods were significant. I then calculate differences in German variations of these coefficients relative to the French ones and evaluate if they are larger/smaller at standard levels of statistical significance. In both of these examinations, I use Wald structural break tests.[8]

I conduct these analyses to identify shifts in the associations between political talk and sociodemographic characteristics that can be robustly attributed to Reunification. The

[7] Linear models might still be problematic if a large proportion of predicted values show numbers beyond the range of the dependent variable. Inspection of results showed no evidence of this problem.

[8] Based on Toyoda's results (1974) on Chow tests' inferential accuracy, it is likely that the robustness of Wald tests is sensitive to heteroskedasticity. This issue, however, is likely to be negligible for my research because the number of observations I analyze is substantially larger than the threshold at which this problem has been found to be no longer substantial (n=50; see Jayatissa 1974).

strongest supportive finding in this regard would be one where coefficient variations are statistically significant for West Germany and also significantly larger or smaller than French ones. Another indicative but somewhat less robust finding would be one in which German variations do not attain statistical significance but are still significantly larger or smaller than French changes.

I also analyze diffs in predicted values (PVs) of political talk for all possible combinations of values of age, education, income, gender, marital status, and residential variables. For ease of explanation, I will refer to these combinations as "vectors of regressors" or as "empirical characterizations" of the regression models. To examine how predicted values of political talk vary by sociodemographic characteristics, I calculate and analyze descriptive statistics of PV *diff* variations for groups of characterizations sharing a specific social attribute in common.

RESULTS

I present my results in three stages. I first discuss regression results for West Germany and France before Reunification to identify baseline socioeconomic disparities in *political talk*. I then evaluate *diffs* in estimated coefficients. Subsequently, I analyze relative differences in *political talk* PVs.

The results I discuss come from linear regression models that included the socioeconomic categorical variables I introduced in the section above as regressors. As a robustness check, I also analyzed estimated coefficients and PV results from models that include gender and Berlin/Paris residence indicators for age categories. Following the advice of Long and Mustillo (2018), I also analyzed changes in predicted probabilities of frequently talking about politics from ordered logistic models that used this unrestricted specification,

and another one that uses the same specification of the linear model whose results I report below. Results from these models, available in Appendixes C and D, are substantively similar to the ones I report below.

Pre-Test Measurements: determinants of political talk before Reunification in West Germany and France.

What was the association between political talk and sociodemographic characteristics in West Germany and France before the Berlin Wall Fall?

Figure 2 presents results from a regression model that includes all socioeconomic categories and interactions between youth and education as independent variables (results for region controls not shown; results are available upon request).[9] Columns 1 to 3 show estimated coefficients' magnitude, standard deviation, and 95% confidence intervals for West Germany. Columns 4 to 6 present equivalent estimates for France.

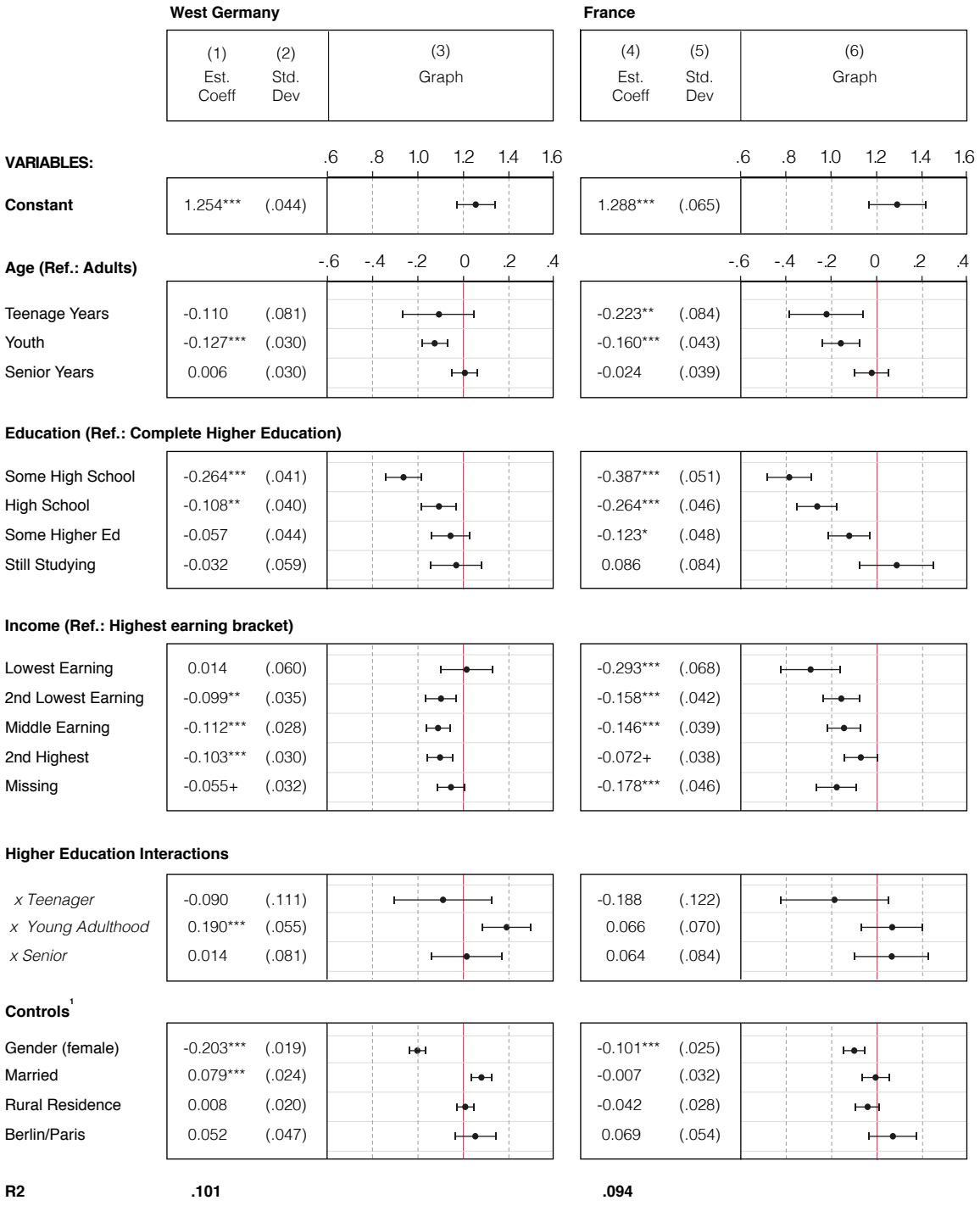
For West Germany, the constant term is significant and sets the baseline value of political talk at 1.252, slightly above the "sometimes talk about politics" threshold.

Regression results show several socioeconomic disparities in political talk. The largest occurs across education. The difference between the least and the most talkative category of this dimension—or its "disparity range," including its reference category—equals 0.264. Differences in political talk expand monotonically, and at a steeper rate, the larger the education gap of a respondent is with respect to college graduates, the most talkative category. On the other hand, people with ongoing studies hold political talk values that do not depart significantly from college graduates'.

[9] Compared to a restricted model without these interactions, this model shows larger goodness-of-fit measures and similar results for education, income and control variables compared to a restricted model without interactions of age and education—see Appendix E.

FIGURE 2

Determinants of Political Talk for West Germany and France before Reunification
Model 1



VARIABLES:	West Germany (1) Est. Coeff	West Germany (2) Std. Dev	France (4) Est. Coeff	France (5) Std. Dev
Constant	1.254***	(.044)	1.288***	(.065)
Age (Ref.: Adults)				
Teenage Years	-0.110	(.081)	-0.223**	(.084)
Youth	-0.127***	(.030)	-0.160***	(.043)
Senior Years	0.006	(.030)	-0.024	(.039)
Education (Ref.: Complete Higher Education)				
Some High School	-0.264***	(.041)	-0.387***	(.051)
High School	-0.108**	(.040)	-0.264***	(.046)
Some Higher Ed	-0.057	(.044)	-0.123*	(.048)
Still Studying	-0.032	(.059)	0.086	(.084)
Income (Ref.: Highest earning bracket)				
Lowest Earning	0.014	(.060)	-0.293***	(.068)
2nd Lowest Earning	-0.099**	(.035)	-0.158***	(.042)
Middle Earning	-0.112***	(.028)	-0.146***	(.039)
2nd Highest	-0.103***	(.030)	-0.072+	(.038)
Missing	-0.055+	(.032)	-0.178***	(.046)
Higher Education Interactions				
x Teenager	-0.090	(.111)	-0.188	(.122)
x Young Adulthood	0.190***	(.055)	0.066	(.070)
x Senior	0.014	(.081)	0.064	(.084)
Controls[1]				
Gender (female)	-0.203***	(.019)	-0.101***	(.025)
Married	0.079***	(.024)	-0.007	(.032)
Rural Residence	0.008	(.020)	-0.042	(.028)
Berlin/Paris	0.052	(.047)	0.069	(.054)
R2	**.101**		**.094**	

NOTES:

+ Significant at the .1 level; * Significant at the .05 level; **Significant at the .01 level; ***Significant at the .001 level.

[1] Regional Control Categories not included; results available upon request

Earning brackets are also significantly but less strongly associated with political talk (disparity range=0.126). The coefficients of these categories describe a quadratic relationship between income and political engagement. Middle-earning categories talk less frequently about politics than both the top and the bottom earning categories. The estimated coefficients of these two categories are very close and statistically equal. Respondents from the missing value category report lower levels of political at 0.1 confidence levels.

In a similar vein, life-cycle brackets are also significantly and moderately associated with political talk (disparity range=0.116). Regression results describe a bimodal relationship between age and political engagement. Seniors and adults are frequent talkers at statistically comparable levels; teenagers and adults, on the other hand, exhibit political talk figures that sit at a level 0.1 units smaller than the one for older. This gap is statistically significant for young adults, but fails to become so for adolescents due to a large standard deviation. Coefficient results from age/education interactions do not indicate significant political talk disparities across educational attainment except for young adults. People with college experience in this age bracket talk substantially more (0.190, a significant difference) than those without it. The teen/college experience interaction term's estimated coefficient is also large (and surprisingly negative), but a large standard deviation prevents it from attaining statistical significance.

Results for control variables report a large and significant talk gap between men and women (0.203) and a small but statistically significant difference between married and single people (0.079). Differences across regions, rural/urban residence, and between Berliners and people living elsewhere are marginal and non-significant.

Broadly speaking, France exhibits similar results. The constant term (1.288) is also significant and similar in magnitude in its Eastern neighbor. Socioeconomic disparities follow the same direction and patterns of statistical significance than in Germany, but they are more linearly organized and stronger in magnitude. The disparity range of income, for example, is almost three times larger than Germany's. An exception is gender: in France, talk disparities between men and women are half of West Germany's.[10]

Diffs in Estimated Coefficients

How did the baseline talk gaps I discussed above change in West Germany relative to France during Reunification?

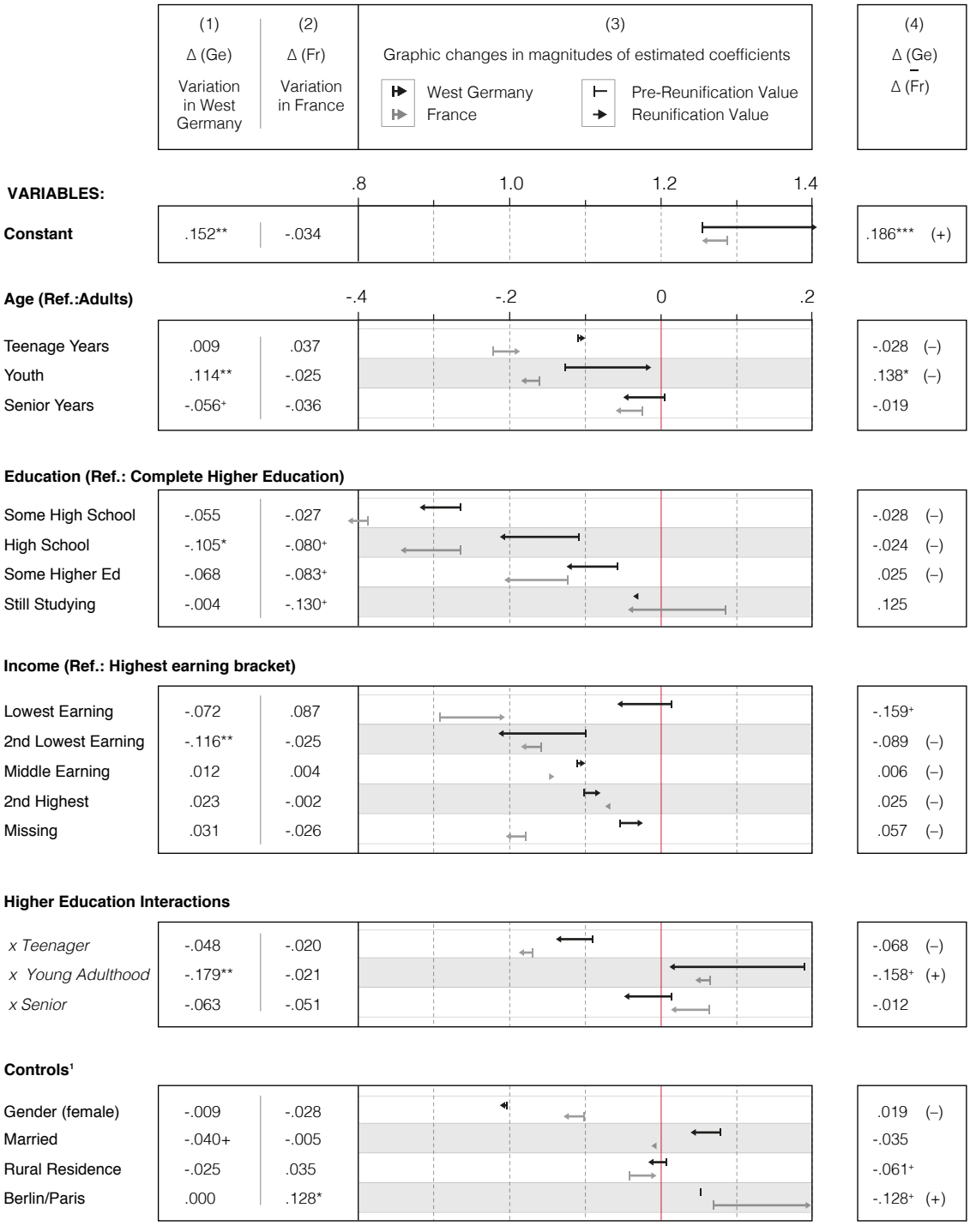
I examine this question with the help of Figure 3. In Columns 1 and 2, I report the magnitude and the statistical significance of coefficients variations for West Germany and France at standard confidence levels. Column 3 also shows these variations graphically. Column 4 reports the magnitude of *diffs* between West Germany and France. It also reports levels of significance at standard levels of confidence.

For variables whose coefficients do not flip direction, *diffs* can be interpreted as changes in talk disparities between a variable and its reference category. For variables with positive coefficients (identified by the "(+)" symbol in column 4), negative *diff* numbers indicate relative decreases in talk gaps in West Germany with respect to France, and positive numbers indicate relative increases. For covariates with negative coefficients (indicated by the "(-)" symbol), this relationship reverses. Negative numbers indicate that talk gaps increased more in West Germany than France, and positive figures that it shrank. Column 4

[10] Regarding regional indicators, the Northwest, the East, and the Southeast exhibit large, positive, and significant coefficients.

FIGURE 3

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



NOTES:

+ Significant at the .1 level; * Significant at the .05 level; **Significant at the .01 level; ***Significant at the .001 level

[1] Regional Control Categories not included; results available upon request

also reports whether German changes were larger or smaller than French ones at standard significance levels of confidence using Wald tests.

Due to space considerations, I center my discussion in variation results for the variables of interest, and do so focusing mainly on *diff* figures, and by identifying covariates that exhibit statistically significant changes that can be attributable to German Reunification (see "Analytical Strategy" section).

The German variation in the constant term is statistically larger than its French counterpart, which undergoes only a small contraction.

Across the life cycle, each age bracket holds particular *diff* logics. Disparities between teens and adults expand slightly, at comparable magnitudes, and non-significantly in Germany and France. On the other hand, the *political talk* deficit that young adults exhibited with respect to mature adults before Reunification undergoes a large and significant decrease in West Germany. This variation is also statistically larger than the moderate expansion it suffers (0.025) in France. On the other hand, seniors flip the direction of their political talk levels relative to adults'. This expansion (0.56) is significant in West Germany but fails to be significantly larger than France's (0.036).

Concerning education, the political talk deficit observed between people without a college degree and no longer in school and those who finished college increases in both West Germany and France. The increase in this gap is slightly larger in Germany for people without college experience and smaller in this country for people with an incomplete higher education. These figures depict a moderate (but statistically non-significant) expansion of talk gaps across educational attainment. In West Germany, variations in talk gaps between college graduates and people with ongoing studies expand marginally (0.004). However, it is quite large in relative terms (0.125) due to the strong contraction of this group's talk levels in

France. Despite its large magnitude, standard errors in coefficients prevent this *diff from* becoming significant (see Figure 2 and Appendix F).

Across income, the largest differences in variations between West Germany and France occur in the two bottom-earning indicators. *Diff* values are strong and significant for the lowest-earning bracket due to large (but not significant) variations that go the opposite direction in each country. For the second-lowest earning bracket, the talk gap relative to highest-income earners expands considerably (0.116). Still, it fails to be statistically larger than the moderate increase in this gap observed for France (0.025). West German and French variations are moderate and statistically similar in all other income brackets and the missing response categories.

Concerning education/life cycle brackets interactions, people with higher education experience, regardless of age, decrease their *political talk* levels relative to people with less education in West Germany and France. Although these changes are all stronger in West Germany, only the one related to young adults is statistically larger than its French counterpart. The contraction of the coefficient indicating college experience and being young (0.179) is the largest observed for any variable over the periods I analyze.

With respect to control variables, Parisian residents increase their *political talk* levels considerably and significantly compared to other French people. This variation is also statistically larger than the one that Berlin residents exhibit relative to other Germans.[11] *Diff* figures for other controls, on the other hand, fail to attain statistical significance.

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

Diffs in Predicted Values

I now proceed to evaluate how their joint action changed how frequently people talked about politics. I analyze how *political talk* PVs changed over a set of 1,296 regressors vectors that include all possible combinations of values of age, education, income, gender, marital status, and residential variables.[12]

Table 1 presents *political talk* PV parameters for groups of regressors combinations with a common sociodemographic characteristic (for ease of exposition, I will use this common characteristic to refer to a specific set of combinations). It also reports the average PV gap across age, education, and income. For each of these groups, Columns 1 and 2 report average PVs of *political talk* before Reunification for West Germany and France, and Column 3 shows differences in these values between Germany and France in that period. Columns 4 and 5 report average PV changes before and during Reunification in Germany and France, respectively, and Column 6 presents average PV *diffs***.** Positive *diff* values indicate larger average changes in West Germany, and negative figures smaller ones**.** Finally, Column 7 shows the "Positive Diff Rate", or PDR, which calculates the proportion of regressors' combinations that exhibited larger variations in *political talk* PVs in Germany than in France. I will focus mainly on discussing figures from these two columns.

Pooled together, 86.2% of the combinations of regressors I analyze undergo larger PV changes in Germany than in its Western neighbor. They also exhibit PV variations that are, on average, larger in Germany than in France by 0.132 units. This relative variation

[12] I excluded from the analysis Berlin and Paris residents living in rural areas and adolescents with college experience. Regressors' vectors indicating urban and rural residence were added a figure equal to the average value of regional coefficients. Regressors' vectors related to Berlin/Paris residence were added a figure equivalent to the coefficient of their regional category.

TABLE 1

Variations in predicted values of political talk between West Germany and France before and during Reunification between West Germany and France

Groups of Regressor Combinations:	N	Linear Regression, Main Model: Pre-Reunification (1) GER	(2) FRA	(3) GER minus FRA	Change during Reunification (4) ΔGER	(5) ΔFRA	(6) Avg. Diffs: ΔGER-ΔFRA	(7) Positive Diff Rate (PDR)
ALL	**1296**	**1.031**	**.907**	***.124***	**.031**	**-.100**	***.132***	***.862***
Age								
Teenage Years[2]	216	.879	.656	*.223*	.043	-.047	*.090*	*.787*
Youth	360	1.048	.872	*.176*	.064	-.114	*.178*	*.925*
Adulthood	360	1.061	.993	*.068*	.058	-.076	*.134*	*.878*
Senior Years	360	1.075	1.007	*.068*	-.035	-.143	*.108*	*.828*
AGE GAP[3]		.196	.351		.001	-.043	*.044*	
Education								
Some High School	288	.832	.641	*.191*	.066	-.045	*.111*	*.819*
High School	288	.987	.765	*.222*	.017	-.098	*.115*	*.826*
Some Higher Ed	216	1.124	.989	*.135*	-.025	-.140	*.115*	*.866*
Higher Education	216	1.181	1.113	*.068*	.043	-.056	*.100*	*.838*
Still Studying	288	1.092	1.101	*-.009*	.044	-.161	*.206*	*.955*
EDUCATION GAP[3]		.349	.472		-.023	-.082	*.059*	
Earning Brackets								
Lowest Earning	216	1.104	.755	*.349*	-.020	-.019	*-.001*	*.514*
2nd Lowest Earning	216	.991	.890	*.101*	-.064	-.132	*.068*	*.750*
Middle Earning	216	.979	.903	*.076*	.064	-.102	*.165*	*.968*
2nd Highest	216	.987	.977	*.010*	.075	-.109	*.184*	*.991*
Highest	216	1.090	1.049	*.041*	.052	-.106	*.158*	*.949*
Missing	216	1.035	.870	*.165*	.083	-.133	*.216*	*1.000*
INCOME GAP[3]		.125	.294		.066	-.152	*.218*	

NOTES:

[1] Excludes combinations with complete and incomplete higher education

[2] Proportion of regressors' combinations whose changes in Predicted Values were larger in West Germany than in France

[3] Difference between the highest and the smallest average predicted value

TABLE 1 (CONT.)
Variations in predicted values of political talk between West Germany and France before and during Reunification between West Germany and France

Model 1, OLS

Groups of Regressor Combinations:	N	Pre-Reunification (1) GER	(2) FRA	(3) GER minus FRA	Change during Reunification (4) ΔGER	(5) ΔFRA	(6) Avg. Diffs: ΔGER-ΔFRA	(7) Positive Diff Rate (PDR)
Tenagers, by education								
Teens without College Exp.	144	.857	.581	.276	.034	-.028	.062	*.729*
Teens with College Exp.	72	.921	.805	.116	.061	-.085	.146	*.903*
TEEN EDUCATION GAP[3]		.064	.224		.027	-.057	.084	
Youth, by education								
No Higher Education	144	.840	.645	.195	.138	-.090	.229	*.972*
w/ Higher Ed. Experience	216	1.187	1.024	.163	.015	-.129	.144	*.894*
YOUTH EDUCATION GAP[3]		.347	.379		-.123	-.039	-.084	
Adult, by education								
No Higher Education	144	.967	.805	-.162	.025	-.065	.090	*.826*
w/ Higher Ed. Experience	216	1.123	1.118	-.005	.081	-.083	.164	*.912*
ADULT EDUCATION GAP[3]		.156	.313		.056	-.018	.074	
Seniors, by education								
No Higher Education	144	.973	.781	-.192	-.031	-.102	.071	*.764*
w/ Higher Ed. Experience	216	1.143	1.158	.015	-.038	-.170	.132	*.870*
SENIOR EDUCATION GAP[3]		.170	.377		-.007	-.068	.061	

NOTES:
[1] Excludes combinations with complete and incomplete higher education
[2] Proportion of regressors' combinations whose changes in Predicted Values were larger in West Germany than in France
[3] Difference between the highest and the smallest average predicted value

makes mean *political talk* differences between these countries more than double before and during Reunification (0.124 vs. 0.256).

Across age, young adults exhibit PDRs and average *diffs* that are notoriously larger than the already lopsided values of these parameters (92.5% and 0.178, respectively).

Equivalent estimates for adults approach global values, while teenagers and seniors report more discreet figures. In West Germany, observed PV variations do not change its disparity rage across age. However, it is larger in relative terms (0.044) compared to the decrease it undergoes in France.

Across education, regressors' vectors indicating ongoing studies exhibit bigger variations in West Germany almost always (PDR =0.979), and at a very a strong average magnitude (0.206). Combinations of regressors' values indicating education completion, on the other hand, exhibit PDR average diff values near the global estimates of these parameters. These shifts produce only a moderate decrease in the education talk gap in West Germany. However, this variation is larger than the steeper reduction it undergoes in France by 0.059 units.

Across income, vectors indicating adscription to the top 3 earning categories and the missing value group exhibit average German variations that are strongly larger (average *diff* > 0.165) and nearly always positive (PDR's of at last 0.968) than in France. In the bottom-two earning categories, these parameters exhibit much smaller values. For the second-lowest earning bracket, the PDR value drops to 0.750, and the average *diff* to 0.068—almost half of its global value. For the bottom income bracket, these values drop further to 0.515 and minus 0.001. This group of combinations is the only set for which average variations are bigger in France than in Germany. These broad variations make the income gap in political talk in West Germany grow substantially (0.066, almost half of its pre-Reunification value). This growth is larger than France's in 0.218 units.

Last but not least, results also show important educational redistributions of political talk across the life cycle. These changes follow two sharply different patterns. In adolescence, adulthood, and seniority, people with college experience carry substantially larger average

*diff*s than their less educated peers (+0.084, +0.074, and +0.061, respectively). These shifts intensify pre-reunification differences in political talk between these groups (+130% for adolescence, +47.4% for adults, and +35% for seniors). PDR variations between these groups also behave similarly. But for young adults, the opposite occurs. Young People without college experience increase their political talk predicted values at a magnitude 60% larger than the gains that their more educated counterparts generate (0.229 vs. 0.144, respectively)[13]. This variation slashes the pre-existing gap between these education groups by almost a quarter. Young adults without higher education also exhibit larger PDR's than their educated peers (0.972 vs. 0.894).

DISCUSSION AND CONCLUSION

In both popular media and scholarly research, historical events have often been characterized as moments of political reinvigoration. The empirical verification of this assumption, however, has largely been overlooked. My investigation sought to fill this gap by identifying and testing hypotheses on how broadly distributed event effects on politicization are and how they reshape pre-existing social disparities in political engagement. I did so by analyzing how relationships between socioeconomic attributes and political talk changed in West Germany relative to France before and during the German Reunification process, a historical condition that briskly halted a period of low eventfulness in West Germany but left it uninterrupted in France.

This investigation allowed me to make three sets of contributions to the literature.

[13] In the predicted value analysis of the unrestricted ordered logistic model, the gap between people with and without college experience was positive, but very close to zero. It was, however, nine times smaller than the next smallest gap increase (0.048 for teens and seniors, See Appendix D).

Theoretically, I clarified the distributional implications of existing theories of event effects. Noting how they tend to consider these influences exclusively as macro-social "imprinting" processes, I introduced an alternative perceptual theory of event effects and derived an original set of distributional hypotheses from it.

Analytically, I studied event effects using a more robust quasi-experimental framework than previous studies. It examines event influences by comparing variations of an outcome of interest in a country before and after it was treated with an event with equivalent variations in a country where both of these periods were uneventful. I developed a way to identify empirical instances close to these conditions—such as the German Reunification period— by generating and using an original indicator of eventfulness.

Substantively, I generated specific results on the politicization shifts that German Reunification provoked. By doing so, I produced concrete findings on how historical events change social disparities in political engagement and how broadly they increase it.

How much and how robustly, then, did sociodemographic disparities in political engagement change during Reunification?

I found that this historical process was associated with a substantial reorganization of political talk across age. I observed that for young adults in West Germany, the Reunification period starkly increased their political engagement, so much that their levels of political talk became statistically comparable to those of mature adults. This increase was also notoriously and significantly bigger than France's.

I also found supportive evidence associating Reunification with increases in socioeconomic disparities in political talk, particularly concerning income. Differences in political talk between the highest and the lowest-earning brackets increased notoriously in

West Germany. This growth achieved statistical significance for the second-lowest income-earning category, and was significantly bigger than France's for the bottom-earning bracket. The education talk gap also expanded in West Germany relative to France, but this increase was not paired with significant coefficient variations.

Finally, I also found that Reunification also brought changes in socioeconomic disparities of political talk across age. Young adults without college experience almost completely erased their political talk deficit relative to their more educated peers. Relative political talk levels of people with college also decreased for adolescents and seniors, but these changes were not significant for Germany in absolute terms or relative to France.

Besides these disparity changes, my analysis also showed how Reunification changed predicted political talk levels for different kinds of people.

I found that 86% of 1,296 different combinations of regressors' values exhibited changes in predicted values of political talk larger in West Germany than in France. This rate indicates a strong association of Reunification with widespread politicization gains. But I also found that the combinations of regressors' values in which political talk growths were bigger in France were not randomly distributed. They were disproportionately concentrated in low-income earning categories. I also found that disparities in predicted values political talk between people with and without college experience amplified for teens, adults, and seniors, but steeply decreased for young adults.

These findings suggest that the evenemential literature has somewhat overstated the capacity that historical events have to act as comprehensive devices for political engagement. Reunification seems to have brought politicization up in a generalized but not entirely complete way. What is more important, those who appear to have made no gains or even

decrease their political engagement in this period are people of lower socioeconomic status. These results disconfirm Hypothesis 1, which regards events as moments of full-fledged and equalizing political activation. They also suggest that age has been overemphasized as the sole social dimension structuring historical sensitivity. In my analysis, variations in estimated coefficients and predicted political talk values were at least as big across income as across age.

My findings also indicate that generational investigations might also be overestimating the degree to which shifts in politicization concentrate in young adults. My results did not support Hypothesis 2, which sees evenemential effects on politicization as exclusive of youth. People at this life stage certainly increased their political engagement levels the most during Reunification, but people of other ages also made substantial increases in it. My investigation did not found supportive evidence for processes of generational unit formation either. Hypothesis 5, which established that socioeconomic disparities in political engagement within youth would grow due to these processes, was strongly disconfirmed by my findings. In fact, I found the opposite relationship: the socioeconomic distribution of political engagement for young adults became more equal during Reunification.

My results do support the distributional propositions of Hypothesis 6, which are premised in an approach to event effects that see them as actively filtered by individual-level attributes for political sensing. This hypothesis proposes that both the magnitude and the socioeconomic distribution of event effects on politicization varied across the life cycle. More specifically, it contends that increases in politicization follow a young adult gradient, and that socioeconomic disparities in this attribute decreased in this age group but expanded for the population at large.

The verification of these propositions carry relevant implications for event-based research on political socialization.

The first concerns our understanding of how event effects come about. Standard takes on these influences tend to see events mainly as large and external socio-historical facts than as micro-political historical experiences. My analysis puts into question these outlooks by failing to find supportive evidence for their distributional implications. Instead, my findings suggest that the political impacts of moments of intense political contingency are constructed in connection to both the occurrence and the experience of history. My investigation brings attention to this alternative understanding of event effects by introducing and supporting the empirical traction of a perception-based model of historical sensitivity. This model sees event influences in politicization as filtered by levels of political interest and amplified by the elasticity of political interest that people hold and the degree to which they use environmental information to make political sense of the world. In the future, conducting investigations aimed at theory building and empirical research on other historical contexts can be instrumental to further evaluate this outlook.

Second, my findings also have relevant implications for how we normatively characterize historical events and their political legacies. They indicate that a depiction of events as democratically revitalizing political phenomena commonly should be taken with a grain of salt. Instead, they suggest that the politicizing impact of events carries a dual distributive logic: they reduce inequalities in political engagement for young adults but exacerbate them for the rest. In this light, historical events acquire an intriguing Janus-face role in bringing societies closer to a state of equality in political engagement. Looking at their own present, historical events seem to perform a regressive role, widening pre-existing inequalities in political engagement between resourceful and disadvantaged people. But

staring at the future, they carry the potential of becoming progressive forces, seeding the possibility of having a more equally involved citizenry in the years to come. The degree to which this potential might materialize depends, of course, on how well preserved the distributional changes in politicization they generate for young adults are maintained over time. Whether this is the case also constitutes an interesting direction for further research.

Third, and from a broader analytical perspective, my findings hope to bring awareness of the active role that historical factors play in defining political orientations, attitudes, and behaviors. Identifying and explaining social differences in these attributes has been a core objective of contemporary lines of research on political socialization. However, understanding how these differences might change according to specific historical circumstances is a line of inquiry that has been often overlooked. My results provide an invitation to research this capacity further and hope to have contributed with new methodological instruments and novel theoretical perspectives to do so.

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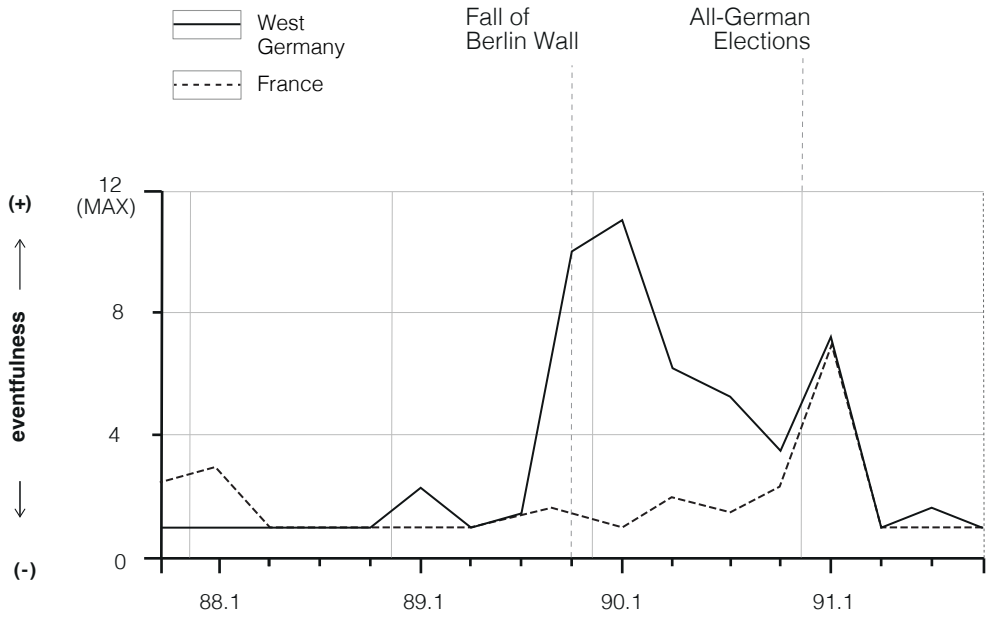
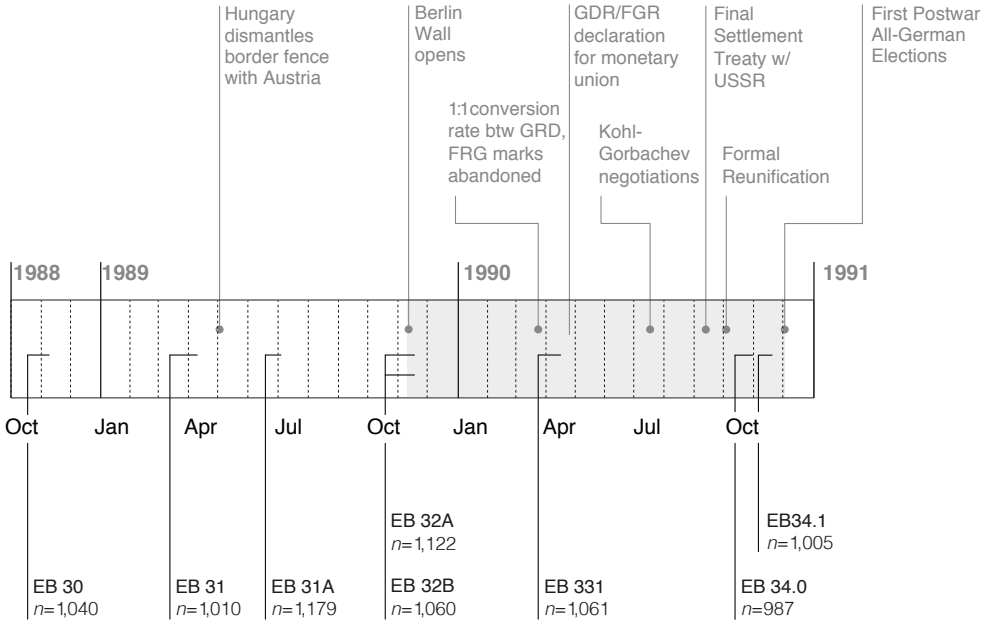
APPENDIX A
Quarterly values of *Eventuflness*[1]
France and Germany, 1988-1991



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

APPENDIX B

Temporal location of Reunification occurrences and Eurobarometer waves under analysis



APPENDIX C1

Determinants of Political talk in West Germany and France Before and After Reunification, Model 2

Variables:	West Germany			France			Diff
	Coefficients		Variation	Coefficients		Variation	
	(1) Before Reunif.	(2) During Reunif.	(3) ΔGE	(4) Before Reunif.	(5) During Reunif.	(6) ΔGE	(7) ΔGE -ΔFR
Constant	1.258***	1.421***	*0.163***	1.314***	1.274***	*-0.040*	*0.203**
Age							
Ref: Adults							
Teenage Years	-0.095	-0.117+	*-0.022*	-0.250**	-0.294***	*-0.044*	*0.022*
Youth	-0.098**	0.016	*0.114**	-0.141**	-0.195***	*-0.054*	*0.168***
Senior Years	-0.042	-0.104***	*-0.062*	0.026	-0.058	*-0.084*	*0.022*
Education							
Ref: Complete College.							
Some High School	-0.261***	-0.317***	*-0.056*	-0.390***	-0.421***	*-0.031*	*-0.025*
High School	-0.105**	-0.213***	*-0.108**	-0.269***	-0.350***	*-0.081+*	*-0.027*
Some Higher Ed	-0.05	-0.119***	*-0.069*	-0.125*	-0.207***	*-0.082+*	*0.013*
Still Studying	-0.035	-0.044	*-0.009*	0.086	-0.044	*-0.130+*	*0.121*
Earning Brackets							
Ref: Highest Earning							
Lowest Earning	0.012	-0.065	*-0.077*	-0.297***	-0.206***	*0.091*	*-0.168+*
2nd Lowest Earning	-0.098**	-0.223***	*-0.125***	-0.158***	-0.186***	*-0.028*	*-0.097+*
Middle Earning	-0.113***	-0.106***	*0.007*	-0.145***	-0.141****	*0.004*	*0.003*
2nd Highest	-0.102**	-0.083**	*0.019*	-0.072	-0.076*	*-0.004*	*0.023*
Missing	-0.053	-0.029	*0.024*	-0.175***	-0.207****	*-0.032*	*0.056*
Teenage Interactions							
Higher Ed. Exp.	-0.092	-0.134+	*-0.042*	-0.196	-0.162	*0.034*	*-0.076*
Male	-0.043	-0.012	*0.031*	0.010	0.202*	*0.192**	*-0.161*
Berlin/Paris residence	0.062	0.390*	*0.328*	0.201	0.037	*-0.164*	*0.492+*
Youth Interactions							
Higher Ed. Exp.	0.198***	0.013	*-0.185***	0.064	0.037	*-0.027*	*-0.158+*
Male	-0.066	-0.086*	*-0.02*	-0.064	-0.004	*0.060*	*-0.080*
Berlin/Paris residence	-0.054	0.193*	*0.246**	0.066	0.076	*0.010*	*0.237+*
Senior Iteractions							
Higher Ed. Exp.	0.003	-0.066	*-0.069*	0.072	0.003	*-0.069*	*0.000*
Male	0.107*	0.115**	*0.008*	-0.108+	-0.015	*0.093*	*-0.085*
Berlin/Paris residence	-0.225	0.069	*0.294*	0.045	0.072	*0.027*	*0.267*
Controls							
Gender (female)	-0.198***	-0.203***	*-0.005*	-0.138***	-0.147***	*-0.009*	*.040*
Married	0.066**	0.02	*-0.046+*	-0.003	-0.014	*-0.011*	*-0.035*
Rural Residence	0.008	-0.017	*-0.025*	-0.042	-0.006	*0.036*	*-0.061+*
Berlin/Paris	0.076	-0.035	*-0.111+*	0.023	0.160***	*0.137**	*-0.248*

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

APPENDIX C2

Variations in predicted values of political talk between West Germany and France before and during Reunification between West Germany and France

Linear Regression, Unrestricted Model

Groups of Regressor Combinations:	N	Pre-Reunification (1) GER	Pre-Reunification (2) FRA	Pre-Reunification (3) GER minus FRA	Change during Reunification (4) Δ GER	Change during Reunification (5) Δ FRA	Change during Reunification (6) Avg. Diffs: ΔGER-ΔFRA	(7) Positive Diff Rate (PDR)
ALL	**1296**	**1.017**	**.914**	***.103***	**.059**	**-.106**	***.165***	***.861***
Age								
Teenage Years[2]	216	.901	.691	*.210*	.106	-.082	*.187*	*.870*
Youth	360	1.040	.874	*.166*	.101	-.112	*.213*	*.961*
Adulthood	360	1.070	.986	*.084*	.026	-.075	*.100*	*.692*
Senior Years	360	1.009	1.016	*-.007*	.024	-.146	*.169*	*.925*
AGE GAP[3]		.108	.325		-.019	-.023	.004	
Education								
Some High School	288	.822	.650	*.172*	.101	-.054	.155	*.837*
High School	288	.979	.771	*.208*	.048	-.104	.152	*.833*
Some Higher Ed	216	1.106	.992	*.114*	-.004	-.141	.137	*.847*
Higher Education	216	1.157	1.116	*.041*	.065	-.059	.123	*.824*
Still Studying	288	1.076	1.111	*-.035*	.073	-.169	.242	*.951*
EDUCATION GAP[3]		.335	.466		-.036	-.076	.040	
Earning Brackets								
Lowest Earning	216	1.088	.758	*.330*	.007	-.020	.027	*.634*
2nd Lowest Earning	216	.977	.897	*.080*	-.040	-.139	.099	*.829*
Middle Earning	216	.963	.910	*.053*	.092	-.107	.199	*.917*
2nd Highest	216	.973	.983	*-.010*	.103	-.115	.218	*.926*
Highest	216	1.076	1.055	*.021*	.085	-.111	.196	*.917*
Missing	216	1.022	.880	*.142*	.109	-.144	.252	*.944*
INCOME GAP[3]		.125	.297		.069	-.156	.225	

NOTES:

[1] Excludes combinations with complete and incomplete higher education

[2] Proportion of regressors' combinations whose changes in Predicted Values were larger in West Germany than in France

[3] Difference between the highest and the smallest average predicted value

APPENDIX C2 (CONT.)

Variations in predicted values of political talk between West Germany and France before and during Reunification between West Germany and France

Model 2, OLS

Groups of Regressor Combinations:	N	Pre-Reunification (1) GER	(2) FRA	(3) GER minus FRA	Change during Reunification (4) ΔGER	(5) ΔFRA	(6) Avg. Diffs: ΔGER-ΔFRA	(7) Positive Diff Rate (PDR)
Tenagers, by education								
Teens without College Exp.	144	.882	.062	.820	.095	-.069	.164	*.847*
Teens with College Exp.	72	.938	.837	.101	.126	-.109	.235	*.917*
TEEN EDUCATION GAP[3]		.056	.775		.031	-.040	.071	
Youth, by education								
No Higher Education	144	.829	.645	.184	.178	-.087	.265	*1.000*
w/ Higher Ed. Experience	216	1.181	1.026	.155	.049	-.129	.178	*.935*
YOUTH EDUCATION GAP[3]		.352	.381		-.129	-.042	-.087	
Adult, by education								
No Higher Education	144	.978	.796	-.182	-.008	-.066	.058	*.611*
w/ Higher Ed. Experience	216	1.132	1.112	-.020	.048	-.081	.129	*.745*
ADULT EDUCATION GAP[3]		.154	.316		.056	-.015	.071	
Seniors, by education								
No Higher Education	144	.915	.783	-.132	.031	-.095	.127	*.611*
w/ Higher Ed. Experience	216	1.072	1.172	.100	.018	-.180	.198	*.745*
SENIOR EDUCATION GAP[3]		.157	.389		-.013	-.085	.072	

NOTES:

[1] Excludes combinations with complete and incomplete higher education

[2] Proportion of regressors' combinations whose changes in Predicted Values were larger in West Germany than in France

[3] Difference between the highest and the smallest average predicted value

APPENDIX D1

Ordered Logistic Coefficients for Political Discussion
West Germany and France before and During Reunification

	Germany				France			
	Before Reunif.		During Reunif.		Before Reunif.		During Reunif.	
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Variables:	Coeff.	Std.Dev.	Coeff.	Std.Dev.	Coeff.	Std.Dev.	Coeff.	Std.Dev.
Age (Ref: Adults)								
Teenage Years	-0.475	(0.293)	-0.377*	(0.180)	-0.680***	(0.248)	-0.513*	(0.234)
Youth	-0.491***	(0.122)	-0.051	(0.098)	-0.470***	(0.130)	-0.567***	(0.100)
Senior Years	0.019	(0.117)	-0.201	(0.085)	-0.072	(0.109)	-0.199	(0.091)
Education (Ref: College)								
Some High School	-1.066***	(0.163)	-1.148***	(0.119)	-1.130***	(0.150)	-1.266***	(0.119)
High School	-0.441**	(0.160)	-0.749***	(0.117)	-0.746***	(0.140)	-1.026***	(0.109)
Some Higher Ed	-0.217	(0.177)	-0.440***	(0.131)	-0.332*	(0.142)	-0.616***	(0.107)
Still Studying	-0.119	(0.252)	-0.128	(0.184)	0.267	(0.239)	-0.104	(0.163)
Income (Ref: Highest earning)								
Lowest Earning	0.045	(0.230)	-0.223	(0.201)	-0.873***	(0.202)	-0.633***	(0.172)
2nd Lowest Earning	-0.394**	(0.138)	-0.786***	(0.110)	-0.473***	(0.123)	-0.532***	(0.100)
Middle Earning	-0.443***	(0.116)	-0.362***	(0.087)	-0.414***	(0.114)	-0.418***	(0.093)
2nd Highest	-0.408**	(0.125)	-0.293***	(0.093)	-0.205	(0.112)	-0.212*	(0.093)
Missing	-0.217	(0.131)	-0.088	(0.105)	-0.530***	(0.135)	-0.613***	(0.097)
Higher Education Interactions								
x Teenager	-0.367	(0.419)	-0.48+	(0.275)	-0.525	(0.356)	-0.561+	(0.303)
x Young Adulthood	0.732**	(0.234)	0.055	(0.180)	0.223	(0.206)	0.142	(0.152)
x Senior	0.06	(0.291)	-0.135	(0.198)	0.181	(0.240)	0.074	(0.192)
Controls[1]								
Gender (female)	-0.824***	(0.080)	-0.793***	(0.061)	-0.303***	(0.072)	-0.409***	(0.056)
Married	0.32***	(0.093)	0.141	(0.069)	-0.015	(0.092)	-0.031	(0.065)
Rural Residence	0.03	(0.082)	-0.073	(0.063)	-0.105	(0.082)	-0.012	(0.062)
Berlin/Paris	0.201	(0.216)	0.170	(0.148)	0.196	(0.153)	0.611***	(0.118)
Education (Ref: College)								
Cut 1	-2.866***	(0.183)	-3.152***	(0.134)	-2.043***	(0.193)	-2.061***	(0.149)
Cut 2	0.837***	(0.173)	0.232+	(0.124)	0.432*	(0.189)	0.570***	(0.145)

NOTES:

+ Significant at the .1 level; * Significant at the .05 level; **Significant at the .01 level; ***Significant at the .001 level.

[1] Regional Control Categories not included; results available upon request

APPENDIX D2

Variations in predicted probabilities of frequently talking about politics in West Germany and France before and during Reunification

Ordered Logistic Regression, Main Model

Groups of Regressor Combinations:	N	Pre-Reunification (1) GER	(2) FRA	(3) GER minus FRA	Change during Reunification (4) Δ GER	(5) Δ FRA	(6) Avg. Diffs: ΔGER-ΔFRA	(7) Positive Diff Rate (PDR)
ALL	**1296**	**.172**	**.189**	***-.017***	**.028**	**-.040**	***.069***	***.890***
Age								
Teenage Years[2]	216	.094	.094	*.000*	.033	-.010	*.042*	*.907*
Youth	360	.189	.175	*.014*	.039	-.045	*.084*	*.928*
Adulthood	360	.182	.220	*-.038*	.048	-.033	*.081*	*.908*
Senior Years	360	.191	.230	*-.039*	-.006	-.062	*.056*	*.825*
AGE GAP[3]		.097	.136		.006	-.033	.039	
Education								
Some High School	288	.081	.088	*-.007*	.035	-.011	.046	*.913*
High School	288	.140	.124	*.016*	.023	-.028	.051	*.858*
Some Higher Ed	216	.216	.211	*.005*	-.001	-.059	.058	*.838*
Higher Education	216	.253	.271	*-.018*	.041	-.025	.066	*.838*
Still Studying	288	.201	.277	*-.076*	.039	-.079	.118	*.979*
EDUCATION GAP[3]		.172	.183		.006	-.033	.039	
Earning Brackets								
Lowest Earning	216	.211	.129	*.082*	-.003	-.008	.005	*.542*
2nd Lowest Earning	216	.150	.178	*-.028*	-.017	-.046	.029	*.810*
Middle Earning	216	.145	.186	*-.041*	.043	-.042	.084	*1.000*
2nd Highest	216	.149	.218	*-.069*	.049	-.047	.096	*1.000*
Highest	216	.204	.252	*-.048*	.041	-.052	.093	*.991*
Missing	216	.173	.170	*.003*	.057	-.047	.104	*1.000*
INCOME GAP[3]		.066	.123		.031	-.067	.098	

NOTES:

[1] Excludes combinations with complete and incomplete higher education

[2] Proportion of regressors' combinations whose changes in Predicted Values were larger in West Germany than in France

[3] Difference between the highest and the smallest average predicted value

APPENDIX D2 (CONT.)

Variations in predicted probabilities of frequently talking about politics in West Germany and France before and during Reunification

Model 1, Ordered Logistic Model

Groups of Regressor Combinations:	N	Pre-Reunification (1) GER	(2) FRA	(3) GER minus FRA	Change during Reunification (4) ΔGER	(5) ΔFRA	(6) Avg. Diffs: ΔGER-ΔFRA	(7) Positive Diff Rate (PDR)
Tenagers, by education								
Teens without College Exp.	144	.088	.074	.014	.027	-.002	.030	*.868*
Teens with College Exp.	72	.108	.133	-.025	.044	-.024	.068	*.986*
TEEN EDUCATION GAP[3]		.020	.059		.017	-.022	.039	
Youth, by education								
No Higher Education	144	.087	.089	-.002	.065	-.021	.086	*1.000*
w/ Higher Ed. Experience	216	.257	.232	.025	.023	-.060	.083	*.880*
YOUTH EDUCATION GAP[3]		.170	.143		-.042	-.039	-.003	
Adult, by education								
No Higher Education	144	.132	.135	.003	.025	-.022	.047	*.875*
w/ Higher Ed. Experience	216	.216	.277	.061	.063	-.040	.103	*.931*
ADULT EDUCATION GAP[3]		.084	.142		.038	-.018	.056	
Seniors, by education								
No Higher Education	144	.134	.127	-.007	-.001	-.032	.031	*.799*
w/ Higher Ed. Experience	216	.229	.298	.069	-.009	-.082	.073	*.843*
SENIOR EDUCATION GAP[3]		.095	.171		-.008	-.050	.042	

NOTES:

[1] Excludes combinations with complete and incomplete higher education

[2] Proportion of regressors' combinations whose changes in Predicted Values were larger in West Germany than in France

[3] Difference between the highest and the smallest average predicted value

APPENDIX D3

Determinants of Political talk in West Germany and France
Before and After Reunification
Model 2, Ordered Logistic Model

	West Germany				France			
	Before Reunif		During Reunif.		Before Reunif		During Reunif.	
	(1)	(2)	(3)	(4)	(6)	(7)	(8)	(9)
Variables:	Est. Coeff	Std. Dev	Est. Coeff	Std. Dev	*Est. Coeff*	*Std. Dev*	*Est. Coeff*	*Std. Dev*
Age *(Ref.: Adults)*								
Teenage Years	-0.404	(.351)	-0.423+	(.222)	-0.750**	(.278)	-0.814**	(.264)
Youth	-0.374*	(.150)	0.059	(.122)	-0.406**	(.153)	-0.600***	(.124)
Senior Years	-0.169	(.149)	-0.415***	(.108)	0.079	(.153)	-0.221+	(.120)
Education (*Ref.: Complete College.)*								
Some High School	-1.057***	(.165)	-1.145***	(.120)	-1.136***	(.151)	-1.289***	(.120)
High School	-0.430***	(.162)	-0.757***	(.118)	-0.758***	(.140)	-1.045***	(.110)
Some Higher Ed	-0.19	(.177)	-0.419**	(.131)	-0.338	(.142)	-0.619***	(.107)
Still Studying	-0.124	(.253)	-0.154	(.184)	0.270	(.241)	-0.103	(.163)
Income (*Ref.: Highest Earning.)*								
Lowest Earning	0.037	(.230)	-0.248	(.201)	-0.886***	(.202)	-0.634***	(.172)
2nd Lowest Earning	-0.392**	(.139)	-0.815***	(.111)	-0.474***	(.124)	-0.538***	(.100)
Middle Earning	-0.449***	(.116)	-0.386***	(.087)	-0.412***	(.114)	-0.418***	(.093)
2nd Highest	-0.405**	(.126)	-0.306**	(.093)	-0.206+	(.112)	-0.215*	(.093)
Missing	-0.206	(.131)	-0.109	(.105)	-0.520***	(.136)	-0.619****	(.097)
Teenage Interactions								
Higher Ed. Exp.	-0.381	(.422)	-0.465+	(.275)	-0.551	(.357)	-0.555	(.303)
Male	-0.192	(.320)	-0.078	(.232)	0.022	(.254)	0.580**	(.221)
Berlin/Paris residence	0.341	(1.039)	1.521*	(.605)	0.551	(.384)	0.103	(.281)
Youth Interactions								
Higher Ed. Exp.	0.780**	(.238)	0.069	(.182)	0.223	(.207)	0.116	(.154)
Male	-0.286	(.188)	-0.320*	(.153)	-0.181	(.190)	-0.008	(.138)
Berlin/Paris residence	-0.22	(.438)	0.660	(.334)	0.115	(.253)	0.232	(.176)
Senior Iteractions								
Higher Ed. Exp.	-0.021	(.293)	-0.22	(.199)	0.206	(.241)	0.034	(.195)
Male	0.447	(.213)	0.460**	(.147)	-0.312+	(.187)	0.004	(.150)
Berlin/Paris residence	-0.827	(1.188)	0.195	(.413)	0.088	(.281)	0.249	(.254)
Controls								
Gender (female)	-0.811***	(.110)	-0.750***	(.086)	-0.408***	(.102)	-0.452***	(.079)
Married	0.268**	(.095)	0.073	(.070)	-0.001	(.094)	-0.039	(.066)
Rural Residence	0.033	(.082)	-0.07	(.063)	-0.105	(.082)	-0.009	(.062)
Berlin/Paris	0.300	(.304)	-0.128	(.206)	0.097	(.186)	0.492***	(.146)
Cut 1	-2.894***	(.186)	-3.218***	(.137)	-2.111***	(.199)	-2.124***	(.154)
Cut 2	0.822***	(.175)	0.180	(.127)	0.367+	(.194)	0.512***	(.150)

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

APPENDIX D4

Variations in predicted probabilities of frequently talking about politics in West Germany and France before and during Reunification

Ordered Logistic Regression, Unrestricted Model

Groups of Regressor Combinations:	N	Pre-Reunification (1) GER	Pre-Reunification (2) FRA	Pre-Reunification (3) GER minus FRA	Change during Reunification (4) Δ GER	Change during Reunification (5) Δ FRA	Change during Reunification (6) Avg. Diffs: ΔGER-ΔFRA	(7) Positive Diff Rate (PDR)
ALL	**1296**	**.166**	**.191**	***-.025***	**.044**	**-.040**	***.084***	***.891***
Age								
Teenage Years[2]	216	.105	.106	*-.001*	.080	-.015	*.095*	*.917*
Youth	360	.181	.174	*.007*	.062	-.041	*.103*	*.950*
Adulthood	360	.188	.218	*-.030*	.029	-.033	*.062*	*.806*
Senior Years	360	.167	.233	*-.066*	.019	-.061	*.080*	*.903*
AGE GAP[3]		.062	.127	-.065	-.030	-.033	.003	
Education								
Some High School	288	.080	.090	*-.010*	.050	-.011	.061	*.913*
High School	288	.138	.125	*.013*	.040	-.027	.068	*.858*
Some Higher Ed	216	.208	.211	*-.003*	.010	-.058	.068	*.843*
Higher Education	216	.240	.272	*-.032*	.054	-.025	.078	*.856*
Still Studying	288	.193	.282	*-.089*	.058	-.080	.139	*.965*
EDUCATION GAP[3]		.160	.182		.004	-.033	.037	
Earning Brackets								
Lowest Earning	216	.203	.129	*.074*	.014	-.006	.020	*.662*
2nd Lowest Earning	216	.146	.180	*-.034*	-.006	-.046	.041	*.792*
Middle Earning	216	.139	.189	*-.050*	.057	-.041	.099	*.968*
2nd Highest	216	.144	.220	*-.076*	.064	-.047	.111	*.977*
Highest	216	.197	.255	*-.058*	.062	-.051	.113	*.954*
Missing	216	.169	.174	*-.005*	.071	-.049	.120	*.995*
INCOME GAP[3]		.064	.126		.036	-.070	.106	

NOTES:

[1] Excludes combinations with complete and incomplete higher education

[2] Proportion of regressors' combinations whose changes in Predicted Values were larger in West Germany than in France

[3] Difference between the highest and the smallest average predicted value

APPENDIX D4 (CONT.)

Variations in predicted probabilities of frequently talking about politics in West Germany and France before and during Reunification

Model 2, Ordered Logistic Model

Groups of Regressor Combinations:	N	Pre-Reunification (1) GER	(2) FRA	(3) GER minus FRA	Change during Reunification (4) ΔGER	(5) ΔFRA	(6) Avg. Diffs: ΔGER-ΔFRA	(7) Positive Diff Rate (PDR)
Tenagers, by education								
Teens without College Exp.	144	.098	.085	.013	.071	-.008	.079	*.889*
Teens with College Exp.	72	.117	.148	-.031	.098	-.029	.127	*.972*
TEEN EDUCATION GAP[3]		.019	.063		.027	-.021	.048	
Youth, by education								
No Higher Education	144	.079	.088	-.009	.082	-.018	.100	*1.000*
w/ Higher Ed. Experience	216	.250	.231	.019	.049	-.057	.105	*.917*
YOUTH EDUCATION GAP[3]		.171	.143		-.033	-.039	.006	
Adult, by education								
No Higher Education	144	.137	.132	-.005	.010	-.023	.033	*.764*
w/ Higher Ed. Experience	216	.222	.274	.052	.041	-.040	.081	*.833*
ADULT EDUCATION GAP[3]		.085	.142		.031	-.017	.048	
Seniors, by education								
No Higher Education	144	.123	.126	.003	.018	-.027	.045	*.889*
w/ Higher Ed. Experience	216	.196	.305	.109	.019	-.084	.103	*.912*
SENIOR EDUCATION GAP[3]		.073	.179		.001	-.057	.058	

NOTES:

[1] Excludes combinations with complete and incomplete higher education

[2] Proportion of regressors' combinations whose changes in Predicted Values were larger in West Germany than in France

[3] Difference between the highest and the smallest average predicted value

APPENDIX E

Determinants of Political talk in West Germany and France Before and After Reunification
Restricted Model[1]

	West Germany				France			
	Before Reunif		During Reunif.		Before Reunif		During Reunif.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables:	*Est. Coeff*	*Std. Dev*	*Est. Coeff*	*Std. Dev*	*Est. Coeff*	*Std. Dev*	*Est. Coeff*	*Std. Dev*
Constant	1.282***	(.042)	1.397***	(.032)	1.308***	(.063)	1.258***	(.047)
Age *Ref: Adults*								
Teenage Years	-0.231***	(.054)	-0.168***	(.036)	-0.333***	(.063)	-0.308***	(.046)
Youth	-0.068**	(.026)	-0.003	(.022)	-0.123***	(.034)	-0.158***	(.024)
Senior Years	-0.01	(.028)	-0.059**	(.022)	-0.013	(.035)	-0.057*	(.027)
Education *Ref: Complete Higher Ed.*								
Some High School	-0.299***	(.037)	-0.305***	(.028)	-0.403***	(.047)	-0.421***	(.036)
High School	-0.149***	(.036)	-0.200***	(.027)	-0.280***	(.042)	-0.352***	(.032)
Some Higher Ed	-0.045	(.044)	-0.124***	(.025)	-0.120*	(.048)	-0.205***	(.035)
Still Studying	0.019	(.050)	-0.061	(.041)	0.027	(.068)	-0.065	(.045)
Earning Brackets *Ref: Highest Earning*								
Lowest Earning	0.031	(.060)	-0.047	(.057)	-0.294***	(.068)	-0.199***	(.055)
2nd Lowest Earning	-0.093**	(.035)	-0.211***	(.031)	-0.163***	(.042)	-0.184***	(.033)
Middle Earning	-0.109***	(.028)	-0.100***	(.024)	-0.149***	(.039)	-0.141***	(.031)
2nd Highest	-0.103***	(.031)	-0.082**	(.025)	-0.074+	(.038)	-0.074*	(.031)
Missing	-0.056+	(.033)	-0.024	(.029)	-0.181***	(.046)	-0.206***	(.032)
Controls								
Gender (female)	-0.203	(.019)	-0.213***	(.016)	-0.103***	(.025)	-0.129***	(.018)
Married	0.076	(.024)	0.039*	(.190)	-0.014	(.032)	-0.012	(.021)
Rural Residence	0.008	(.020)	-0.019	(.017)	-0.043	(.028)	-0.008	(.020)
Berlin/Paris	0.058	(.047)	0.055	(.038)	0.070	(.054)	0.200***	(.039)
R2	**.096**		**.106**		**.092**		**.102**	

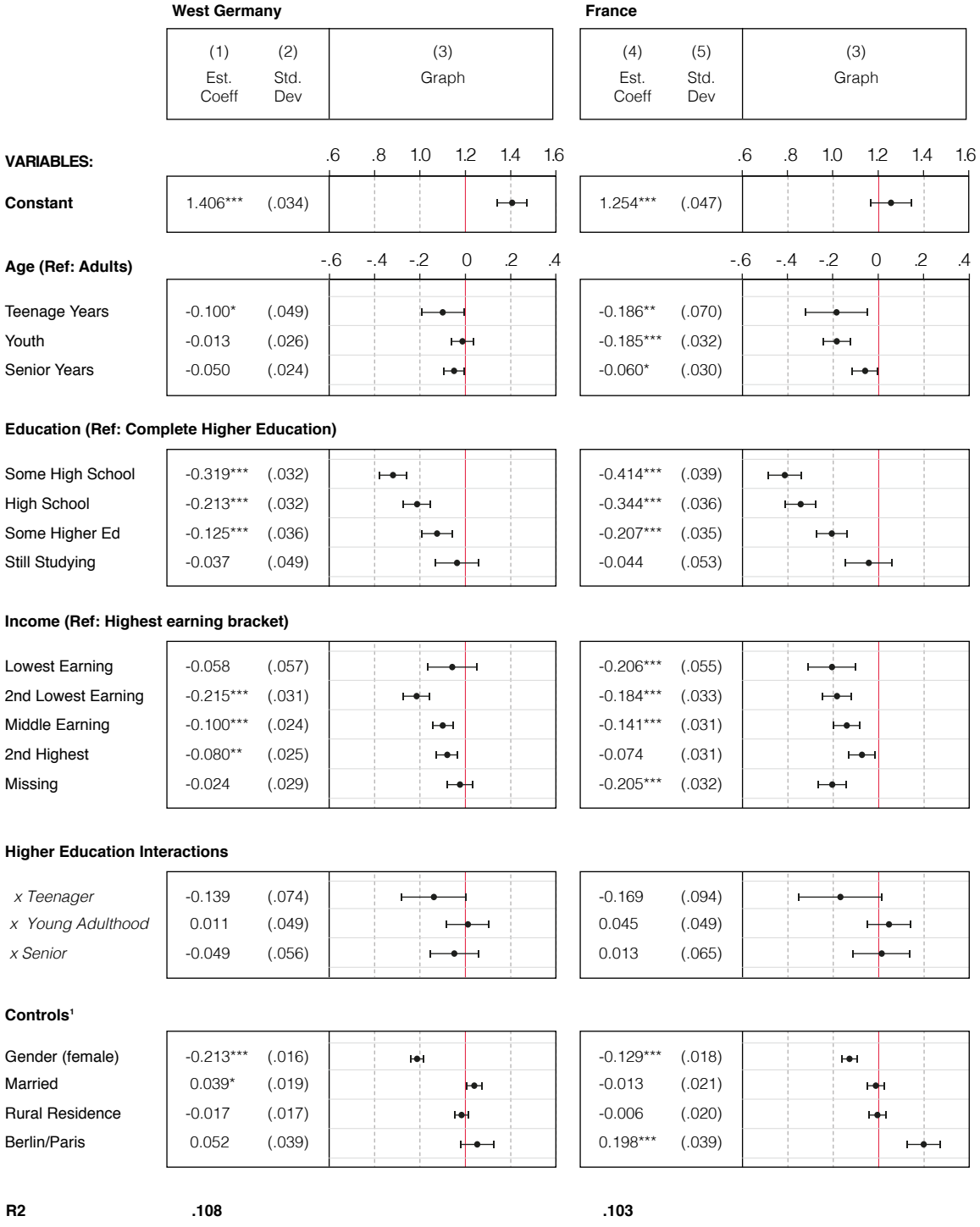
NOTES:

+ Significant at the .1 level; * Significant at the .05 level; **Significant at the .01 level; ***Significant at the .001 level.

[1] Regional Control Categories not included; results available upon request

APPENDIX F

Determinants of Political Talk for West Germany and France during Reunification, Model 1



	West Germany (1) Est. Coeff	West Germany (2) Std. Dev	France (4) Est. Coeff	France (5) Std. Dev
VARIABLES:				
Constant	1.406***	(.034)	1.254***	(.047)
Age (Ref: Adults)				
Teenage Years	-0.100*	(.049)	-0.186**	(.070)
Youth	-0.013	(.026)	-0.185***	(.032)
Senior Years	-0.050	(.024)	-0.060*	(.030)
Education (Ref: Complete Higher Education)				
Some High School	-0.319***	(.032)	-0.414***	(.039)
High School	-0.213***	(.032)	-0.344***	(.036)
Some Higher Ed	-0.125***	(.036)	-0.207***	(.035)
Still Studying	-0.037	(.049)	-0.044	(.053)
Income (Ref: Highest earning bracket)				
Lowest Earning	-0.058	(.057)	-0.206***	(.055)
2nd Lowest Earning	-0.215***	(.031)	-0.184***	(.033)
Middle Earning	-0.100***	(.024)	-0.141***	(.031)
2nd Highest	-0.080**	(.025)	-0.074	(.031)
Missing	-0.024	(.029)	-0.205***	(.032)
Higher Education Interactions				
x Teenager	-0.139	(.074)	-0.169	(.094)
x Young Adulthood	0.011	(.049)	0.045	(.049)
x Senior	-0.049	(.056)	0.013	(.065)
Controls[1]				
Gender (female)	-0.213***	(.016)	-0.129***	(.018)
Married	0.039*	(.019)	-0.013	(.021)
Rural Residence	-0.017	(.017)	-0.006	(.020)
Berlin/Paris	0.052	(.039)	0.198***	(.039)
R2	**.108**		**.103**	

NOTES:

[1] Regional Control Categories not included; results available upon request

+ Significant at the .1 level; * Significant at the .05 level; **Significant at the .01 level; ***Significant at the .001 level.