Social Changes in Impressionable Years and The Formation of Political Attitudes*

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February 2, 2018

Abstract

This paper provides evidence on the legacies of social changes experienced in impressionable ages on adult political attitudes. We show that growing up during the 1930's Jewish expulsions plays a significant role in shaping the political and civic engagement of individuals, and these effects are long lasting. Current adults who experienced the expulsions at impressionable ages are less likely to show interest in politics, less likely to find political activity to be important, and have lower political participation. These results are not found for individuals who were older at the time of the expulsions, and are robust to fixed region and birth-year characteristics, various definitions of impressionable ages, and composition bias induced by differential migration and mortality rates across regions and cohorts. The estimates are also not driven by regional differences in 1930's political participation, party support, Catholic share, exposure and destruction during WWII, urbanization, and other regional characteristics. We provide evidence that the adverse effects of social changes on political attitudes we find are explained by a model of political participation emphasizing the role of civic skills and socioeconomic status acquired at younger ages. Exposure to the expulsions when young is associated with lower adult volunteerism, trust, church attendance, and socioeconomic status.

Keywords: Political Attitudes, Impressionable Years, Social Change, Civic Skills.
JEL Classification: D72, D74, O12, J15.

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1 Introduction

There is a marked increase globally in populations displaced by conflicts, persecutions, violence, and human rights violations, with about 65.6 million people in 2016 estimated to be living as refugees, asylum seekers, or internally displaced persons (UN High Commissioner for Refugees, 2017). These population movements have important consequences for economic and political stability of both home and host countries. Consequently, there has been an active literature on the causes of conflicts and their impacts on health, education, welfare, and risk attitudes; however, much less is known about the impacts of living through these events on political attitudes— especially long-term impacts on young individuals who are not direct victims, survivors, nor displaced by the violence.[1] In this paper, we provide the first causal evidence on the long-term legacies of social changes due to population movements on the political attitudes of individuals who were in their impressionable years during these events, but were neither direct victims nor directly related to the victims.

The formation of political attitudes is an important subject of study, because political interest and participation are widely believed to be essential for proper democratic governance.[2] Voting, as one form of political participation, is associated with the degree to which policy outcomes represent citizens' preferences, and helps to build social capital (Dahl, 1971; Gimpel and Schuknecht, 2003; Highton, 1997; Pateman, 1970). Besides being essential for the proper functioning of a democracy, participation in politics is thought to be important for a range of socioeconomic outcomes: it potentially helps to build social capital, improve public health, empower citizens, and raise aggregate incomes (Acemoglu et al., 2014; Barro, 1996; Carpini and Keeter, 1996; Guiso, Sapienza and Zingales, 2004; Knack and Keefer, 1997; Putnam, 2000; Sanders, 2001; Tavares and Wacziarg, 2001). Hence, a large body of literature attempting to understand the determinants of political behaviour has developed. This topic is even more important in the face of the declining participa-

[1] The literature on the impacts of conflicts and violence on political behaviour remains small. For examples, see Bateson (2012), Bellows and Miguel (2006, 2009), Carmil and Breznitz (1991), Adhvaryu and Fenske (2014), and Humphreys and Weinstein (2007). With the exception of Adhvaryu and Fenske (2014), the other three studies focus on direct victims, survivors, or participants, in the violent episodes under study. Blattman and Miguel (2010) provide a survey of the literature on the causes and consequences of civil wars. Akresh and de Walque (2008), Blattman and Annan (2010), Annan et al. (2011), Leon (2012), and Shemyakina (2011), study the impacts of exposure to violence on physical and mental health, education, and welfare, for direct exposure to conflicts in Rwanda, Uganda, Peru, and Tajikistan, respectively. Callen et al. (2014), Kim and Lee (2014), Miguel, Saiegh and Satyanath (2011), and Voors et al. (2012), examine the impacts of exposure to violence on risk preferences and behaviour.

[2] A generally accepted definition of political participation is any "activity that has the intent or effect of influencing government action — either directly by affecting the making or implementation of public policy or indirectly by influencing the selection of people who make those policies" (Verba, Schlozman and Brady, 1995, p. 38).

tion rates in many established democracies, the simultaneously increasing democratization in many parts of Africa and Latin America, combined with the rise in displaced populations mentioned earlier (O'Toole, Marsh and Jones, 2003; Putnam, 2000; Resnick and Casale, 2011; Schraufnagel and Sgouraki, 2005; UN High Commissioner for Refugees, 2017).

An important open question in the literature on the formation of political attitudes concerns the role of early life socioeconomic circumstances, social interactions, and childhood events, on adult political interest and participation. For instance, are there long-term stable determinants of political behaviour, or is political behaviour determined largely by context-specific cost-benefit analyses? Do social changes in impressionable years influence political behaviour well into adulthood, or are impacts transient? If yes, how and why? We address these questions by investigating the long-term effects of the social changes associated with Jewish expulsions, in Nazi Germany, on the political attitudes of German adults who were in their impressionable years during the expulsions.

After coming into power in 1933, the Nazi government enacted the "Law for the Restoration of the Professional Civil Service," which gave it power to expel all civil servants of Jewish background from national civil service jobs. The immediate impact of the law was a significant loss of highly educated people, ranging from lawyers, doctors and university professors to secondary and primary school teachers, because the Jewish population at this time was significantly more likely to be middle class and concentrated in urban areas, compared to the rest of the population. The expulsions were soon expanded to include blue-collar workers, and eventually all individuals presumed to have at lease one Jewish grandparent (Yahil, 1991). These events led to significant important changes in the structure of German society, from the home, to the office, to the school, and beyond (Evans, 2005; Kaplan, 2005).[3]

In this paper, we treat the expulsions as an exogenous shock to affected German children and young adults, and use differences in exposure across German regions to test whether, and in what ways, growing up during the expulsions had a permanent impact on individuals' political interest and participation as adults. Our focus on childhood and young adults is consistent with the literature on the importance of early life for later cognitive outcomes (Heckman and Kautz, 2013), and is embedded in the well-established *impressionable years* and *increasing persistence* hypotheses,

[3]See the next section for a more detailed description of the Jewish population and effects of the expulsion.

both of which imply that beliefs are mostly formed before full adulthood and fade more slowly with age (Brim and Kagan, 1980; Krosnick and Alwin, 1989).[4]

A challenge in estimating the effects of the Jewish expulsions on political behaviour is that any cohort of individuals has shared experiences that might be correlated with both the expulsions and political behaviour. Thus, we cannot simply compare exposed cohorts to those that were not directly affected by the expulsions. Similarly, we generally cannot identify the impact of the expulsions by comparing areas with different levels of exposure within a particular cohort, because the areas might also be different for other reasons. Therefore, we use the within-region variation across cohorts in the exposure to Jewish expulsions (cohort-by-region variation) to identify the impact of Jewish expulsions on political behaviour, which allows us to account flexibly for time-period, life cycle, and age-specific effects, as well as fixed regional characteristics.[5]

We combine unique data on the fraction of the population who were Jewish in the former West Germany at the lowest representative geographical unit in 1933, from Kessner (1935), with individual-level data from the German Socio-Economic Panel (SOEP, 2012; Wagner et al., 2007).[6] Using pre-1933 percentages of the population who were Jewish as a measure of the magnitude of the expulsion in each region,[7] we find that individuals in their impressionable ages—children and young adults—during the expulsions are significantly less likely to be interested and participate in politics, compared to older individuals or those born long after the war. To put the estimates in perspective, one may compare a young individual in Frankfurt in 1933, where 3.25% of the population was of Jewish origin, to a young individual in Bremen, which was 0.4% Jewish in 1933; the estimates imply that the young individual who was in Frankfurt at the time of the expulsions is five percentage points less likely to participate in local politics, as a result of the expulsions.

[4]The influence of early life events on adult political participation and interest has a long history in political science, and Jennings and Niemi (1974); Miller and Sears (1986); Putnam (2000); Sears (1975) provide important contributions. The importance of childhood and early adulthood years for the formation of political beliefs is linked to the nature of the brain in those critical years (Spear, 2000). Alesina and Giuliano (2011) and Glass, Bengtson and Dunham (1986) provide empirical evidence suggesting that the family environment matters for political participation.

[5]This is a generalized difference-in-differences technique. The empirical strategy involves simultaneously comparing the political behaviours *within a region* of individuals whose childhoods would have been affected by the expulsions with those who were born much later (controls for regional characteristics), and *across regions* with different levels of pre-1933 Jewish population for individuals who would have been affected by the expulsion (controls for cohort characteristics).

[6]These geographical unites were formally referred to formally as *Raumordnungsregionen* (RORs) and are determined by the Federal Planning office based on economic inter-linkages. They are most similar to metropolitan statistical areas (MSA) in the U.S. See Knies and Spiess (2007) for more detailed information on regions in the SOEP.

[7]We show in Section 3.3 that the change in the *share* of the Jewish population, following the expulsions, was similar across regions. Therefore, regions with an initially larger Jewish proportions would have also lost a relatively larger share of their population to the expulsions.

This estimate is important quantitatively, as studies have found that door-to-door canvassing, for example, increases voter turnout by an average of seven percentage points (de Rooij, Green and Gerber, 2009).

The estimated impacts may be interpreted as being causal under the assumption that the trends in political attitudes would have been the same in all regions if it had not been for the expulsions (parallel trends). We test this assumption directly by running a placebo test which shows that the impact of Jewish expulsions is neither found for individuals who were past their impressionable years during this time period nor for individuals who were born in the 1960s, after the expulsions. Therefore, we are able to rule out explanations for our results that rely on region-wide or cohort-specific factors, and also general region-cohort trends in political behaviour and attitudes. These results also imply that expulsions have no spillover impacts on later generations born in regions with higher ex-ante proportions of Jewish people.[8]

Our results could also be confounded by the possibility of other economic and political events that were specific to high-exposure regions, and also disproportionately affected individuals in their impressionable ages. These include economic and institutional/political shocks that could affect the young *differentially*, as has been documented by several studies (see Giuliano and Spilimbergo 2014; Lewis-Beck 1990; Remmer 1991; Kenneth M. Roberts 1999; Tufte 1980; Wilkin, Haller and Norpoth 1997). However, we note that fixed-region characteristics are already accounted for by the region fixed effects. Similarly, cohort-specific characteristics are already accounted for by the birth-year fixed effects. Nonetheless, we draw on several sources of historical data to demonstrate that our results are not driven by a variety of region-specific shocks or characteristics that could have had differential impacts on impressionable-aged individuals. On the socioeconomic front, the results are not explained by the differential impacts of region-level unemployment rates, income per capita, population density, urbanization rates, and the Catholic share, all of which could have played a role in the expulsions (Becker and Pascali, 2016). Furthermore, we find that affected children and young adults in high exposure areas are not more likely to migrate, nor do they have higher mortality rates, as would be predicted if it were the case that the more politically active were more likely to migrate or die during the turbulent times under study.

[8]We are able to use cohorts born after the war as a placebo-treated cohort because of the rapid post-war recovery across Germany (Akbulut-Yuksel, 2014; Ichino and Winter-Ebmer, 2004).

On political events, we find that the impact of the expulsions are not confounded by the differential impacts of the shares of votes for the Nazi and Communist parties within the region, or turnout rates for the 1932 elections, all of which might have driven the intensity of expulsions (Satyanath, Voigtländer and Voth, 2017; Becker and Pascali, 2016). The results also hold when we drop all regions with high vote shares for the Nazi party, where the Nazi party obtained more than 40% of the vote, also indicating that initial levels of support for the party is not driving the results. Importantly, the impact of the expulsions on political attitudes is also not explained by the differential impact of World War II (WWII), as measured by the volume of destruction (rubble per capita). We are further able to rule out the impact of WWII, because the estimated impacts of the expulsions decline significantly when we expand the treatment group to include individuals who experienced the war but the not the expulsions in their impressionable years, and in fact do not find any impacts when we restrict the treated cohort to individuals who spent more of their impressionable years during WWII. As a final check, we show that the length of impressionable years spent in the expulsions matters, and not just having grown up during the expulsions. The non-impacts of WWII on political attitudes are consistent with the results in Adhvaryu and Fenske (2014). Overall, none of these region-specific events or characteristics have large differential impacts on the subsequent political attitudes of contemporary children and young adults, with the point estimates close to zero and insignificant.

We argue that the impacts of the expulsions on the formation of political attitudes are consistent with the impacts of growing up during a period of social change. Just as Acemoglu, Hassan and Robinson (2011) find in Russia, the Jewish expulsions resulted in the loss of an important middle class in the regions in which they are located, which would greatly affect the environment in which young individuals are socialized, at home and schools (Jennings and Niemi, 1974; Sears, 1975). For instance, the expulsions resulted in the loss of more than 15 percent of university professors, many teachers, doctors, lawyers, and other professionals, who were dismissed as a result of the law (Akbulut-Yuksel and Yuksel, 2015; Evans, 2005; Moser, Voena and Waldinger, 2014; Strauss, 1983; Yahil, 1991). In addition, the expulsions would have also exposed the young to associated violent conflicts, which have been shown to have psychological and material impacts (Akresh and de Walque, 2008; Blattman and Annan, 2010; Annan et al., 2011; Leon, 2012; Shemyakina, 2011).

However, exposure to war, or worse education/labour market outcomes, are not sufficient to decrease political interest and participation; in fact, several studies find that exposure to violence increases political interest and participation even when treated individuals face worse labour market and education outcomes. This is especially true for ex-combatants and direct victims of conflict (Bateson, 2012; Bauer et al., 2016; Bellows and Miguel, 2006, 2009; Blattman, 2009; Carmil and Breznitz, 1991).

The association between exposure to social change in impressionable years and adult political attitudes is best explained by drawing on insights from the literature on the importance of the social environment in the formation of political attitudes, which moves beyond the role of socioeconomic status (Alesina and Giuliano, 2011; Brady, Verba and Schlozman, 1995; Glass, Bengtson and Dunham, 1986; Jennings and Markus, 1977; Jennings and Niemi, 1974; Putnam, 2000; Verba and Nie, 1972; Verba, Schlozman and Brady, 1995). The key idea is that the expulsions changed the environment in which children and young adults were socialized (home, neighbourhoods, schools), which subsequently influenced interest in politics, socioeconomic status as adults, and crucially the ability to acquire the resources required for political participation in adulthood, such as income and civic skills (Brady, Verba and Schlozman, 1995). Consistent with this theory, we find that, relative to members of the same cohort who lived in areas with relatively less Jewish shares, exposed children and young adults earn lower wages and have less schooling. Specifically on civic skills, the results show that the affected cohort are also less likely to believe politics is important, less likely to attend church, less likely to volunteer, and have lower levels of trust. In summary, our results on the negative impacts of exposure to the expulsions on political attitudes are consistent with relatively lower adult civic skills and socioeconomic status in adulthood.

Contributions and Related Literature:

We make a significant contribution to studies of the political legacies of conflict, summarized in Section 4 of Blattman and Miguel (2010), by examining impacts over the very long-term, impacts on individuals in their impressionable ages, and on individuals who were not necessarily direct victims. In a recent study, Adhvaryu and Fenske (2014) found no impact on political attitudes and behaviour following conflicts in Africa, which is explained as being a result of resilience and post-traumatic

growth (Tedeschi and Calhoun, 2004). Furthermore, Bellows and Miguel (2006, 2009) and Blattman (2009) find conflict exposure has a positive impact on political participation in Sierra Leone and Uganda, respectively. One important difference between our study and Bellows and Miguel's study on Sierra Leone is that the latter examined the impact of the conflict on adults (with an average age of 42) in the early 2000s, while we emphasize the long-term impact on individuals who were still in their impressionable ages. Studying the impact on adults rules out one channel through which conflict may impact political behaviour, by changes in the socialization environment and the ability to acquire civic skills, which are largely completed by adults. Blattman (2009) examines the impact of conflict on individuals in Uganda who were participants in and victims of the conflict, many of whom also experienced personal growth and acquired civic skills during and after the conflict, in spite of worse schooling outcomes (Blattman and Annan, 2010). Besides examining impacts on individuals in their impressionable ages, our findings add to this growing body of literature by providing a mechanism through which the negative impacts of conflict on political behaviour might persist. This occurs when the ability to acquire civic skills in childhood is affected, as was the case for children and young adults during the Jewish expulsions who experienced signicant changes in German society.

Our results contribute to the debate on the size of human capital externalities especially the long-run relationship between education and political attitudes. While most studies find little evidence of wage externalities to human capital (Acemoglu and Angrist, 1999; Moretti, 2004; Rauch, 1993), we provide evidence of political externalities to human capital. Our estimates imply that large changes in human capital, on the scale of the Jewish expulsion, can have long lasting impacts on political behaviour by altering schooling attainments and adult civic skills. This is consistent with the results of recent empirical studies on the impact of schooling on political behaviour (Campante and Chor, 2012; Dee, 2004; Larreguy and Marshall, 2017; Milligan, Moretti and Oreopoulos, 2004; Siedler, 2010). A key distinction in our study is highlighting the importance of context-relevant civic skills, because lower schooling and labour market outcomes do not necessary imply a decline in political participation Bellows and Miguel (2006, 2009).

A second contribution of our paper is to the literature examining the causal impacts of events in childhood and young adulthood on political attitudes and behaviour (Adhvaryu and Fenske, 2014;

Giuliano and Spilimbergo, 2014; Jennings and Markus, 1977, 1984; Kim and Lee, 2014; Madestam and Yanagizawa-Drott, 2012; Malmendier and Nagel, 2011). Consistent with most of the literature, we find that events in childhood and early adulthood have strong and persistent impacts on adult political behaviour. However, we do not find evidence that the differences in political attitudes for exposed cohorts are driven by macroeconomic shocks, as per Giuliano and Spilimbergo (2014) and Malmendier and Nagel (2011), which may reflect the fact that the economic impacts of the war and associated military policies were felt across most regions of Germany. Consistent with the results of Madestam and Yanagizawa-Drott (2012) regarding the impact of the Fourth of July celebrations on civic engagement, we also find that the impacts of events in full adulthood are not persistent given that experience of the expulsions do not have long-term impacts on the political behaviour of adults. Our paper provides further evidence on the importance of early life events, especially conflict and war, for adult attitudes and behaviour, as predicted by the *impressionable years* and *increasing persistence* hypotheses (Brim and Kagan, 1980; Krosnick and Alwin, 1989; Jennings and Markus, 1977; Sears and Funk, 1999). We also add to this literature by further showing that these impacts do not spill over into the political behaviours of future generations.

The results in this paper are related directly to studies of the determinants of political attitudes, particularly regarding the importance of an individual's socioeconomic status for their political behaviour (Campbell et al., 1960; Putnam, 2000; Verba, Schlozman and Brady, 1995; Wolfinger and Rosenstone, 1980). Unlike most empirical studies analyzing the link between socioeconomic status and political behaviour using survey and census data (see Bekkers 2005; Brady, Verba and Schlozman 1995; Finkel and Muller 1998, and Putnam 1995), our empirical contribution to this literature is the demonstration of the causal association between events in childhood that shape individuals' civic skills and socioeconomic status, and adult political interest and behaviour. Hence, we are able to explore how exogenous differences in childhood experiences influence political interest and engagement in adulthood, and further show that these impacts are significant and long lasting.

Finally, this paper improves our understanding of the long-term causes, consequences, and impacts, of the Nazi regime and the holocaust on economic performance, schooling, social and human capital, political and social attitudes, and financial institutions (Acemoglu, Hassan and Robinson, 2011; Akbulut-Yuksel and Yuksel, 2015; Braun, 2016; D'Acunto, Prokopczuk and Weber,

2015; Grosfeld, Rodnyansky and Zhuravskaya, 2013; Pascali, 2009; Satyanath, Voigtländer and Voth, 2017; Voigtländer and Voth, 2015; Waldinger, 2012, 2010). Consistent with our findings, Acemoglu, Hassan and Robinson (2011) find that the degree of Jewish persecution during the holocaust in Russia is also related negatively to current political behaviour, and attribute this to changes in the social structure within Russian society. While the two studies are related, our study examines a different country, and we emphasize the channel through which an exposure to persecutions is related to political behaviour at an individual level, as opposed to aggregate changes in the social structure. Our results may be interpreted as evidence that social structure is important for political behaviour by changing individuals' abilities to acquire civic skills, especially in a situation like Germany's where social capital was important for the rise of the Nazi regime in the first instance (Satyanath, Voigtländer and Voth, 2017).

2 Historical Background on Jewish Expulsions

The historical circumstances surrounding the Nazi regime and its policies, including Jewish expulsions, have been discussed extensively in the literature (Evans, 2005; Friedlander, 2009; Kaplan, 2005; Voigtländer and Voth, 2015; Yahil, 1991). This section summarizes the history of Jewish expulsions in Germany, with a focus on factors that are important for our empirical analyses. Specifically, we focus on two facts: (1) the expulsion policy, encapsulated in the "Law for the Restoration of the Professional Civil Service," was driven and implemented by the national government, and (2) the policy led to the expulsion of Jewish professionals, who eventually left the country or were killed in the holocaust. The first point helps to address concerns about whether the expulsions were *targeted* or implemented differently in some regions, and the second demonstrates that the policy led to important population movements and changes in the socioeconomic structure of German society over this period.

The German Nazi party gained power in 1933, at which time it was estimated that there were about 520,000 Jewish peopl in the *Deutshes Reich*, making up 0.8% of the population. The Jewish population had been remarkably successful in professional occupations, such as medicine, teaching, law, journalism, finance, business, and academia, and made up a larger proportion of the middle and

upper-middle classes. The expulsion policy, initiated on April 7, 1933, was an attempt to purge Jewish people from the civil service and several professional occupations that were government-controlled. By May of the same year, the expulsion was extended to postal service workers, railroad operators, professional associations, trade guilds and many other occupations (Friedlander, 2009; Kaplan, 2005). The purges were extensive and defined anyone with at least one Jewish parent or grandparent as Jewish. In the event that an individual could not prove sufficiently that he was not Jewish, he had to provide further evidence from experts on racial research from the Ministry of Interior (Yahil, 1991). It is important to note here that the policy was initiated and implemented nationally by the Nazi party, with little of the implementation being left to regions (Friedlander, 2009).

The impact of the policy on the Jewish population in Germany was severe, and led to a considerable loss of human capital in key sectors. For example, the German Municipal Statistical Yearbooks report that 8.3 percent of teachers were dismissed in 1933, a figure that rose to 32 percent of female secondary school heads in cities like Berlin with significantly higher Jewish populations (Evans, 2005). Famously, the expulsions were also extended to academia, seeing the expulsion of about 15 percent of university professors for being Jewish, in addition to over 2,000 research scientists and other scholars (Evans, 2005; Waldinger, 2010). In the legal profession, the policy saw the expulsion of 16 percent of lawyers, with all Jewish lawyers having lost their admission to the bar by 1938. In the medical profession, all Jewish doctors had lost most of their non-Jewish patients by July 1933, as insurance companies would only reimburse fees for Jewish patients, which effectively put the doctors out of work (Yahil, 1991). Germany found it difficult to fill the vacancies which opened up in affected occupations, and the purge reflected genuine changes in the social structure and education institutions (Acemoglu, Hassan and Robinson, 2011; Waldinger, 2010).

Our aim is to study the impact of these social changes on the subsequent political behaviour of German children and young adults who were in their impressionable years during the expulsions. As a result of the national nature of the policy, we can reasonably infer that the degree of exposure in each region should be proportional to the initial concentration of the Jewish population, all else being equal. For example, we would expect to see more individuals, relative to the population, forced to leave their jobs in Frankfurt than in the average German region, because Jewish people

made up a larger proportion of the population of Frankfurt — which we do show to be the case in Table 1. We take advantage of this plausibly exogenous source of variation in losses across regions, generated by regional differences in the proportion of the population who were Jewish, in order to estimate the impact of the social change associated with the expulsions on the formation of political attitudes, after controlling for fixed regional and cohort characteristics. Details on the identification strategy, and data are provided in the next section.

3 Empirical Strategy and Data

3.1 Empirical Strategy

We identify the impact of Jewish expulsions on political behaviour by exploiting the region-by-cohort variation in the intensity of the expulsions, as a share of each region's population. This strategy helps us to account and control for region-specific and cohort-specific differences simultaneously. Plausibly exogenous regional variation comes from the fact that the policy was formulated and implemented nationally, which means that any differences in the proportion of the population expelled across regions should depend only on differences in the initial proportion of Jewish people. The main cohort variation comes from comparing individuals who were of impressionable age at the start of the expulsions with those who were born after WWII and the reconstruction, and for whom no direct effect is expected. We also test for robustness of the estimates across different age bands for the definition of impressionable ages, as well as comparing individuals who were older during the expulsions to those born in the 1950's.[9] As was mentioned earlier, this focus on childhood and young adults is motivated by the well-established *impressionable years* and *increasing persistence* hypotheses in social psychology (Brim and Kagan, 1980; Krosnick and Alwin, 1989). These theories imply that beliefs are largely formed before full adulthood and fade more slowly with age, suggesting that the early years are crucial for belief formation.[10]

[9] We note that if there are spillovers due to the transmission of political values or behaviour across cohorts, then one might expect an indirect effect on the unborn, in which case the within-region cohort variation would deliver lower-bound estimates of the effect. However, we do not find evidence of any significant impact on the political behaviour of the unborn.

[10] For recent applications of this hypothesis to political and economic behaviour, see Giuliano and Spilimbergo (2014), who find that growing up during a recession has strong impacts on adult political preferences. Madestam and Yanagizawa-Drott (2012) also find that attending the Fourth of July celebrations in youth has strong persistent impacts on political behaviour, but the effect on older individuals is smaller and non-persistent.

With all these in mind, we implement a generalized difference-in-differences strategy in which our treatment variable is an interaction between the proportion of Jewish people in the region and an indicator for being between the ages of 6 and 23 at the start of the expulsions (the impressionable/treated cohort). Specifically, we estimate the equation below:

$$Y_{irt} = \alpha + \beta(FractionJewish_r \times ImpressionableCohort_{it}) + \rho_r + \tau_t + \delta' \mathbf{X}_{irt} + \epsilon_{irt}, \qquad (1)$$

where Y_{irt} is a measure of political behaviour (interest and participation) for individual i in region r born in year t. $ImpressionableCohort_{it}$ is an indicator variable that is equal to 1 if an individual i's year of birth t was between 1910 and 1927 (making them between the ages of 6 and 23 at the start of the expulsions), and 0 otherwise. The parameter of interest, β, estimates an intent to treat (ITT) effect, as we are looking at the pool of individuals who were potentially treated as children and young adults.

We also include region fixed effects, ρ_r, to account for the fact that regions with different proportions of Jewish people might have fundamentally different political behaviours. Birth-year events and all shocks that are common to individuals born in each birth year are accounted for using birth-year fixed effects, τ_t. Birth-year fixed effects are even more general than, and already account for, "impressionable cohort" fixed effects. Lastly, we control for a number of individual and household characteristics in the vector $\mathbf{X}_{irt}$, including gender and rural dummies, and parental education. The error term is denoted by ϵ_{irt}, is assumed to be possibly correlated within regions and is clustered by region. In our baseline estimates, the treatment group is defined as individuals born between 1910 and 1927, as they were likely to be of school and impressionable ages in 1933 when the expulsions began. Our control group consists of individuals who were born between 1951 and 1960, and is chosen because they were not affected directly by the expulsions, WWII or the subsequent reconstruction (Akbulut-Yuksel, 2014; Ichino and Winter-Ebmer, 2004).

We perform robustness checks of the baseline results using different treatment and control groups. First, we *extend* the definition of impressionable years to include all individuals who were born during the war and were children or young adults during the expulsions (cohorts born between 1910 and up to 1945), in order to test the robustness of the results to the selection of our baseline

treatment group. We include the cohort born between 1928 and 1933, who were likely too young to have been in school before the expulsions began but were possibly in school at some point during the war (schooling is often regarded as an important avenue for political socialization; see Dee, 2004; Jennings and Niemi, 1974; Jennings and Markus, 1984; Milligan, Moretti and Oreopoulos, 2004; Siedler, 2010). However, note that in addition to the expulsions, the socialization of the cohort born between 1933 and 1945 was also very likely to have been positively influenced by the rapid post-war reconstruction which might confound our results (see Ichino and Winter-Ebmer 2004 for a formal structural test). We also *restrict* the treated group to the cohort born between 1915 and 1927 as they were within compulsory schooling ages during the expulsions. Second, we use individuals who were born between 1900 and 1909, and therefore experienced the expulsions but probably not in their impressionable years, as an alternative control group. Third, we depart from a cohort-based definition of treatment and use a continuous measure of treatment by calculating the number of years in which the individual would have experienced the expulsions, which clearly illustrates that our estimates capture lived experiences beyond age and cohort effects.[11] Lastly, we also include individuals born between 1946 and 1950 in the control group, in order to ensure that the analysis is robust to using the full sample of people born between 1946 and 1960 as the control group. Our results are robust to these alternate definitions of the treatment and control groups.

We carry out additional falsification tests of the identification assumption. Our strategy identifies the coefficient β as a causal impact of the expulsions on the formation of political attitudes if the impressionable cohort and those born after 1950 would have had the same *trends* in political behaviour across regions with different ex-ante proportions of Jewish peool had the expulsions not occurred. We evaluate this assumption by performing a falsification test in which we compare individuals born between 1951 and 1960 to both individuals born in 1961–1970 and individuals born between 1900 and 1909. The aim of this exercise is to show that there are no systematic trends in political behaviour across cohorts and regions with different proportions of the population being Jewish, except for the cohorts who were children and young adults during the expulsions. This exercise also helps us to test for spillover effects of the expulsions on future political behaviour. We

[11] This continuous measure of exposure is calculated as the length of exposure to the Nazi Regime, which was between 1933 and 1945; thus, we have a total of 12 years of possible exposure. We also assume that the child was affected by the expulsions if they are between 6 and 23 years of age during the period 1933 to 1945. Thus, an individual born in 1910 would have only one year of experience, while an individual born in 1930 would have had nine years of exposure.

also investigate the robustness of our results to the potential measurement and composition bias that might have been generated by internal migration and mortality rates for the exposed cohort.

3.2 Data Description

Our analyses are built around individual and household data from the 1985 German Socio-Economic Panel (SOEP) in former West Germany, which is a representative survey of West Germans residing in private households. From the SOEP, we collect a battery of information on individual and household characteristics, including parental and childhood environments. The sample is restricted to individuals born between 1910 and 1960, who would all have been adults (older than 25) at the time of the survey in 1985.[12]

We focus on the impact of the expulsions at level of the smallest geographical unit provided in SOEP, called the *Raumordnungsregionen* (RORs or regions). The measure of the proportion of Jewish people in the population in each region is obtained from Kessner (1935), who provides city-level information on the percentages of individuals who were affiliated with various religious groups in 1933. We use the percentage in 1933, obtained from the 1933 German population census, because the expulsions began in 1933, as was explained in the historical background. It is important to note that while we define individuals as "Jewish" based on their religious identification, the expulsions defined individuals as Jewish based on much wider racial/ethnic criteria (Evans, 2005). However, this discrepancy should not pose major problems because 91.5 percent of "racial" Jewish people in the 1939 German Reich also had Jewish religious affiliations (Blau, 1950).[13]

The key outcome variables are measures of individuals' interest in politics and participation in local politics. We examine interest in politics because it is generally related to civic engagement and political participation (Bekkers, 2005; Brady, Verba and Schlozman, 1995). From the SOEP, we use the question asking, "First of all in general: How interested are you in politics?" Individuals

[12]The analysis looking at the impact of the expulsion on German children assumes that there are very few Jewish respondents in the survey. This is supported by data from the 2007 SOEP which show that only 0.24 percent of respondents in former West Germany belonged to "other religious organizations," which excludes Protestants, Catholics, Evangelicals, Other Christians, Islamic religious organizations and non-denominations. Other data show that the fraction of the population of Germany in 1946 who were Jewish was only 0.15 percent.

[13]Given the high correlation between "racial" and practising Jewish people in 1939, we believe that the percentage of religious Jewish people serves as a good proxy for the percentage of the population who were Jewish in 1939. In fact, there is reason to believe that this correlation would have been higher in 1933, because about half of the Jewish population had already emigrated before 1939, and Jewish of mixed ancestry who were largely considered Jewish primarily as a result of the Nuremberg laws of 1935 constituted a significant fraction of non-religious Jewish people in 1939 (Evans, 2005).

are assumed to be interested in politics if they indicate "strong" or "very strong" interest, and not interested if they indicate "weak" or "none". For political participation, we use the question asking how often individuals participate in "citizen initiatives, parties, community politics." Individuals are coded as not being participants if they indicate that they "Never" participate. The data reveal that 36% of all individuals indicate a strong or very strong interest in politics, but only 8.2% of individuals in the sample actively participate (see Table 1). We also collect further data on individuals' employment, income, schooling, and other measures of civic engagement, which we use to test the possible channels through which the expulsions might have affected political behaviour. The description of the construction of these individual-level variables is left to the Appendix, but they are summarized in Table 1.

The dataset also includes additional information at the regional level that is collected from a range of different sources. These include the average income per capita and the unemployment rate in 1932, the shares of votes received by the Nazi and Communist Parties, and turnout rates, for the two federal elections of 1932. We also have region-level information on population and area, and the share of Catholics in 1933. Lastly, we obtain a measure of the region-level wartime destruction by compiling information on the volume of residential rubble at the end of WWII in each region. These data are obtained from various years of the German Municipal Statistical Yearbook, and will be used to test interpretations and alternative explanations for our baseline results. We match regions in the SOEP to digitized data on the city-level fraction of the population who were Jewish, and other historical socioeconomic variables. Individuals in the SOEP have unique regions (RORs) which are matched to the percentage of the city-level population who were Jewish from Kessner (1935). This is possible because every city reported by Kessner (1935) belongs to only one region in the SOEP.[14]

[14]We are able to match cities to 47 regions that form the aggregate unit of analysis. These 47 regions account for 85% of the West German population at the time of the survey.

3.3 Descriptive Characteristics

Region Characteristics

Descriptive statistics for regional characteristics are presented in the top panel of Table 1, and the regions are split according to intensity of Jewish expulsions represented by the percentage of Jewish people in the population of the region in 1933. The table illustrates some key differences across regions, and characteristics associated with relatively higher Jewish populations. We see in the top panel that the percentage of Jewish people in the population of former West Germany is given as 1.2%, which is slightly larger than the average of 0.8% reported in the 1933 census. This is largely because the Yearbook reports the percentage of Jewish people in the population in cities with 50,000 or more inhabitants, which are more likely to have relatively larger Jewish populations. Furthermore, the data show a significant degree of variation in the initial percentage of Jewish people, ranging from an average of 1.96% in "above-average" regions to 0.72% in regions with below-average Jewish population shares.

Importantly for us, the data also reveal the decline in the share of Jewish people in the population as a result of the expulsions and the holocaust, with these proportions dropping to 0.3 and 0.07 in high and low areas, respectively, by 1946. We note that while the *absolute loss* is larger in areas with relatively larger Jewish populations (1.66 vs. 0.642), the *percentage declines* are quite similar (0.85 vs. 0.89). We take this as evidence of the uniform implementation of the expulsions across regions, with the impact on population shares being larger in areas with larger initial Jewish populations. This difference in the share of the population lost during the expulsions, as a result of initial differences in the proportion of Jewish people, rather than differences in expulsion rates, is precisely the region-level variation that we exploit in the analyses.

The top panel of Table 1 also highlights some of the characteristics of regions with high and low Jewish population shares in 1933. In general, we do not find any systematic differences across high and low intensity regions in terms of WWII destruction, as measured by the quantity of rubble per-capita during WWII, and unemployment rates are very similar. Moving on to political variables, we do not see any systematic differences in share of votes to the Communist or Nazi parties in 1932 elections, nor are there significant differences in turnout rates. There are also no significant

differences in the urban share of the region's population nor in the share of Catholics. Lastly, there are no region-specific differences in 1932/33 income per capita, population and land areas. Overall, while there are known historical characteristics that have driven the location of Jewish populations and persecution (Satyanath, Voigtländer and Voth, 2017; Voigtländer and Voth, 2012), we do not find systematic differences across regions in the broad set of characteristics considered here. Nevertheless, we do include region fixed effects in the analyses to account for any unobserved differences across regions, and as a robustness check we also allow for differential impacts of observed region characteristics on individuals in their impressionable ages.

[Table 1 about here.]

Individual Characteristics

Next, we provide a brief summary of individual and household characteristics from the SOEP in the lower panel of Table 1. Across all variables, we once again do not find systematic differences across regions in interest in politics, political participation, volunteerism, trust attitudes, and church attendance. This implies that any results will be driven by cohort-by-region differences, as opposed to differences across regions alone. Furthermore, individuals complete about 11.3 years of schooling on average, and over 80% of individuals have mothers and/or fathers with a basic education. Furthermore, 59% of the respondents live in rural areas and 54% are female. The average age of the respondents is 47 years, indicating that these are adults with relatively established political attitudes. Next, we use the region-cohort variation in the extent of the expulsions to estimate the impact of social change on individuals' long-term political behaviour.

4 Estimated Impact of Expulsions on Political Behaviour

4.1 Baseline Difference-in-Differences Estimates

[Table 2 about here.]

Table 2 reports the difference-in-differences estimates of the effect of Jewish expulsions on the subsequent political behaviour of Germans who were at impressionable ages at the time of the

expulsions. The top panel (Panel A) shows the impact of the expulsions on individuals' interest in politics and political participation, under the baseline discrete measure of treatment. The estimate in column (1) of the table indicates that, in a region with an average Jewish population in 1933, individuals who were in their impressionable years during the expulsions are about 4.6 percentage points less likely to express an interest in politics than individuals in the same region who were born after the expulsions and the war.[15] This coefficient is important quantitatively, and its importance can be illustrated in two different ways. First, given that about 36% of the sample indicate an interest in politics, the estimates imply that the general interest in politics among the cohort who were in their impressionable years during the expulsions is about 13% (4.6/36) lower than the overall mean in a region with the average percentage of Jewish people in 1933. A second way of illustrating the significance of this estimate is to compare a young individual in 1933 Frankfurt, where 3.25% of the population were of Jewish origin, to a young individual in Bremen, where only 0.4% of the population were Jewish in 1933. In this case, the estimates imply that a young individual who was in Frankfurt at the time of the expulsions is 11 percentage points less likely to indicate an interest in politics than an individual in the same cohort from Bremen, with all else being equal. The estimates also indicate that individuals who grew up in Frankfurt during the expulsions will show an average political interest that is one-third of the German mean, all else the same.

Columns (2)–(3) of the top panel control for parental education. We find that having parents with more than basic education increases an individual's interest in politics, a result that is generally consistent with the typical role of education and the family in political socialization (Glass, Bengtson and Dunham, 1986; Jennings and Niemi, 1974; Verba, Schlozman and Brady, 1995). However, this does not mitigate the impacts of the expulsions on children and young adults.[16] Overall, the estimates in columns (1)–(3) show that the expulsions had a quantitatively significant impact on political interest for individuals who were in their impressionable years during the expulsions.

[15] This is obtained by multiplying the estimated difference-in-differences coefficient, 0.039, by the average percentage of Jewish people in the sample, 1.19.

[16] We also allow the difference-in-differences estimates to vary based on parental education. An increase in parental education does not affect the estimated impact of the expulsions on individuals' political interest, and the estimated interaction terms are insignificant. Thus, it appears that factors outside the home were also responsible for the decline in political interest among the cohort who lived in more exposed areas during their impressionable years. Results are available upon request.

Columns (4)–(6) of Table 2 shows similar results regarding the impact of the expulsions on political participation. The estimates in column (4) imply that, in a region with an average Jewish population in 1933, individuals who were in their impressionable years during the expulsions are about 2.2 percentage points less likely to participate in local politics than individuals from the same region who were born after the expulsions and the war. As with the results on political interest, this estimate is statistically and quantitatively meaningful, representing a 26% percent lower political participation for the impressionable cohort than for the unaffected cohort in a region with the average Jewish population share in 1933, and also a 5.2 percentage point drop in political participation—more than 50% lower than the national average—between individuals from the affected cohort in Frankfurt (high proportion of Jewish people) and Bremen (below average Jewish population share). Furthermore, the 2.2 percentage point decline in political participation (26% below the mean) is important, given that canvassing, which is an important method of increasing political participation, increases the voter turnout by an average of 7 percentage points, from a base of about 50%—representing a 14% change (de Rooij, Green and Gerber, 2009). Columns (5) and (6) of Panel A continue to show that parental education is an important determinant of political participation, although they are not jointly significant in column (6). These results lead us to conclude that our point estimates for the impact of the expulsions on political participation are robust to family characteristics, and that the expulsions led to a significant decline in political participation for individuals who were in their impressionable years.

4.2 Estimated Impacts by Length of Exposure

Next, we evaluate the impact of the expulsions by length of exposure in Panel B of Table 2. That is, we ask whether individuals for whom most of their childhood and early adulthood was during the period of the expulsions are affected more than those who experienced the expulsions for only a few years. This question is relevant because the impact of the expulsions is not expected to be the same for all individuals within the cohort, and one might wonder whether the impact is restricted to individuals who were exposed most intensively at particular ages.

To address this question, we break down the exposed cohort by the estimated length of exposure (ranging from 0 to 12 years). While we leave the details of this calculation for appendix Table A4,

we are computing the number of years that an individual would have been between the ages of 6 and 23 between 1933 and 1944 (from the beginning of the expulsions to the last full year of Nazi Regime). Note that this measure of exposure is not the same as age; for example, an individual born in 1917 would have had the same eight years of childhood and young adulthood exposure as an individual born in 1931, because one turned 24 in 1941 (exposure between 1933 and 1940) and the other turned 6 in 1937 (exposure between 1937 and 1944). The estimated length of exposure is then used as the measure of treatment, allowing us to differentiate between individuals within the exposed cohort but with different intensities of exposure to the expulsions, while still controlling for birth-year and region fixed effects. The results from this alternative definition of exposure are presented in Panel B of Table 2.

Columns (1)-(3) of Table 2 continue to show that exposure to the expulsions has a significant negative impact on political interest. The coefficient is interpreted to mean that an individual with only one year of exposure to the expulsions would be 0.26 percentage points less likely to be interested in politics than an individual living in the same region but with no exposure to the expulsion, all else being the same. This impact increases with the length of exposure, because an individual with about six years of exposure is 1.56 percentage points less likely to indicate interest in politics than an individual with no exposure. Furthermore, columns (4)-(6) of Panel B indicate that, all else being the same, an additional year of exposure is also associated with a 0.11 percentage point decline in political participation for individuals who lived in a region with the average Jewish proportion in 1933. These estimates are robust to the inclusion of the mother's and father's education, and indicate that the impact of the exposure does increase with the estimated length of exposure.

In summary, the results in Panel B of Table 2 show that the negative impacts of the expulsions on political behaviour are larger among individuals for whom more of their childhood and young adulthood was passed during the expulsions and the subsequent turbulent period. Hence, we conclude that the length of exposure—not merely belonging to the cohort born between 1910 and 1927—is important for understanding the impact of the expulsions, independent of any birth-year fixed effects.

4.3 Falsification Tests, Assignment Rules, and Composition Bias

Next, we test the identification assumption behind the difference-in-differences estimates, evaluate the robustness of the estimated impacts of the expulsions to the rule assigning individuals to treatment and control groups, and examine the robustness of our estimates to systematically different migration and mortality rates across regions—sampling bias.

Falsification Test

We present evidence on the validity of the assumption behind our identification strategy in Table 3. Recall that our identification assumption is that there would have been similar trends in political attitudes for cohorts across regions with different ex-ante proportions of Jewish people, if it had not been for the expulsions. Stated differently, our identification assumption is valid if there are no differential cohort-region trends in political behaviour independent of exposure to the expulsions. We test this by comparing political behaviours across regions with different proportions of the population being Jewish in 1933, but using the younger cohort born between 1951 and 1960 as a placebo treatment group, and other (older and younger) cohorts as the control group.[17]

As the results in Table 3 show, there are no significant differences in political interest and participation across these cohorts in regions with differing proportions of the population being Jewish in 1933, if all else is the same. This is the case when we compare individuals born between 1951 and 1960 to both individuals born between 1961 and 1970 and an older cohort born between 1900 and 1909.

[Table 3 about here.]

In addition, these results indicate an absence of spillover effects of the expulsions on the political behaviour of the younger cohorts. If there were spillovers across generations, we might expect to see persistent differences in political behaviour among members of the cohort that was not exposed to the expulsions directly but was born right after the events, but this is not found in the data. Furthermore, the absence of significant differences between individuals who were born before 1910

[17] We perform the falsification tests using both cohorts because the younger cohort, born between 1961 and 1970, are relatively young to have fully-formed political attitudes in 1985, and there are too few left of the older cohort, born between 1900 and 1909, by 1985.

and those who were born after the Nazi Regime indicates that the impacts that we observe are not found for individuals who were already adults during the expulsions. Thus, the impacts that we identify are specific to individuals who were at an impressionable age in areas with higher Jewish population shares, not merely those who lived through the expulsions, nor is it reflective of general cohort-region trends. This is as expected, given the rapid recovery in German infrastructure following the end of WWII (Akbulut-Yuksel, 2014; Ichino and Winter-Ebmer, 2004).

Robustness to the Assignment Rule and WWII Exposure

We assess the robustness of our baseline results by utilizing a range of definitions for the treatment and control groups, and also by estimating the impacts of the expulsions treating individuals born in 1910–1923, 1910–1933 and 1910–1938, 1910-1945, 1915-1927, and 1908-1927, as alternative treatment groups in Table 4. The first four categories expand the definition of treatment to include those who would have also been impacted by the war in their impressionable ages. The last two categories, 1908-1927 and 1915-1927, examine robustness to excluding individuals, born between 1908-1914, who may have been too old to be affected the social changes during the expulsions. In Table 5, we also extend the control group to include all individuals born immediately after the war, between 1946 and 1960.[18]

[Table 4 about here.]

The first column of Table 4 provides the estimates for individuals born between 1910 and 1923, who were therefore between the ages of 10 and 23 at the start of the expulsions in 1933, and the second column includes estimates of the impact of the expulsions on individuals who were born from 1910–1933. For these groups, we find that the expulsions continue to have a strong and significant negative impact on the general interest in politics and political participation, and the point estimates remain very similar to the baseline specification. The same is true when we include individuals who were born during the expulsions and the subsequent war, between 1933 and 1945, but the point estimates for participation in local politics becomes weaker. The decrease in the estimated impacts when the treatment group is extended to individuals born after the expulsions

[18]The year 1938 is chosen because the expulsions largely ended in 1939, which also marked the beginning of WWII, and the year 1945 marked the end of WWII.

had already begun (1933-1945) is evidence that the impact of initial Jewish populations is not being driven by WWII—else we would have seen similar impacts as for the 1910-1927 cohort. The estimates in the last two columns also show that the results are robust to excluding or including the older treated cohorts, born between 1908 and 1914. In Appendix Table A1, we also divide the treatment group into different cohorts based on their birth year, and estimate the impacts of the expulsions by cohort. Results show that the estimated impacts are strongest for those who were 6-18 (born between 1915 and 1927) at the start of the expulsions, and weakest for those born between 1928 and 1938 who would have been even younger during the WWII. This confirms that our results are not being driven by WWII, nor a specific definition of impressionable age.

In Table 5 we further extend the control group to include individuals born just after the war, between 1946 and 1960. While this is a relatively noisy control group, especially when we include individuals born around 1945 in the treated category as in column (5), we continue to find that growing up in an area that was more greatly impacted by the expulsions continue to be strongly negatively associated with interest in politics and political behaviour. Overall, we conclude that the estimated impacts of the expulsions on interest in politics and political participation is robust to the definition of impressionable years and a different categorization of the control group.

[Table 5 about here.]

Robustness to Differential Migration, Mortality, and Cohort Sizes

Individuals in our analyses are counted as being exposed to the expulsions based on their current region of residence, under the assumption that, on average, their current region of residence will be the same as that in their impressionable years.[19] However, it is possible that individuals may have moved since the expulsions, and might be living in regions other than that in which they lived in their early years. This would potentially introduce measurement error to our treatment variable and bias our estimates towards zero. We note that we do not expect differential migration rates given the well-documented low rates of mobility in Germany, which are known to be even lower in early childhood (Hochstadt, 1999; Rainer and Siedler, 2009; Pischke and von Wachter, 2008).

[19] With the exception of the 2012 wave of SOEP (SOEP, 2012), the SOEP and other individual level German datasets only include information on the current region of residence prior to the 2000's.

As a more formal check of on the potential measurement errors induced by internal migration, we restrict the sample to non-movers, who are defined as individuals who currently live in the same region as in their childhood. The results from running the estimation on this restricted sample are shown in columns (1) and (2) of Table 6. We find that, for a region that was 1% Jewish in 1933, the difference-in-differences estimates of the expulsions on interest and participation in politics for non-movers, at -0.05 and -0.03, are even larger in magnitude than the baseline estimates using the whole sample. However, the differences from the baseline estimated impacts are not statistically significant. This evidence indicates that any potential measurement error induced by internal migration is unlikely to be biasing our results. This is consistent with the literature on German internal migration cited in the previous paragraph.

Furthermore, we evaluate the possibility of differences in internal migration rates conditional on the percentage of Jewish people in the population in 1933. For example, it is possible that areas with larger than average Jewish populations attracted young migrants during this time period, who are unlikely to be interested in local politics. Column (3) of Table 6 classifies individuals as movers if they no longer live in their childhood region of residence, and estimates the difference-in-differences impact of the expulsions using the probability of moving as the dependent variable. The estimate of the probability of moving is close to zero and statistically insignificant, suggesting that our results are not driven by differential migration rates across regions, for the cohort in their impressionable years.

One might also be concerned about the possibility of differential mortality rates across regions with different Jewish populations in 1933 that might have an impact on political participation and interest through the age structure, independently of the expulsions. Note that birth-year fixed effects already account for national differences in mortality rates across cohorts. Nevertheless, we address concerns regarding differential region-cohort mortality rates by utilizing the panel structure of SOEP and tracking the mortality of individuals in the dataset between 1985 and 2011. The mortality variable is an indicator that is equal to 1 if an individual died between 1985 and 2011 and 0 otherwise. Results presented in column (4) of Table 6 indicate that mortality rates across regions are similar for the treated and control cohorts, with the coefficient being close to zero (0.008) and statistically insignificant. Therefore, we conclude that a composition bias across cohorts and

regions, induced by differential mortality rates, is unlikely to be driving our results.

We further evaluate the importance of composition bias further by estimating the differences in cohort sizes across regions and cohorts. Composition bias might potentially arise if, for example, individuals who were more politically active died disproportionately during the expulsions in regions with higher Jewish proportions. To address this potential issue, we follow Meng, Qian and Yared (2015) and test whether birth cohort size in a given region is related to ex-ante Jewish population. The results, presented in column (5) of Table 6, show that birth cohort size is not associated with the Jewish population share in 1933; therefore, suggesting that our results are not an artefact of composition biases induced by potential differences in mortality rates and cohort sizes across regions.

[Table 6 about here.]

In summary, we have shown that the estimated impacts of the Jewish expulsions on the political behaviour of Germans who were at an impressionable age during the expulsions are robust to a number of possible biases, and have also provided supportive evidence of our identification assumption. We have demonstrated that there are no region-cohort trends for those born after the turbulent events or those who were already adults before the expulsions, relative to younger cohorts who were not exposed to the expulsions. We also find that our results are not affected by the potential measurement errors generated by internal migration. The estimates are similar to the baseline when the sample is restricted to non-movers, and we do not find any significant cohort-by-region variation in migration rates. Lastly, we document the robustness of our results to various forms of age-related composition biases that may be induced by differential mortality. Hence, we conclude that the estimated impacts are generally robust. We next examine the robustness of the estimated impacts to other contemporary events that might vary by region and cohorts.

4.4 Role of Other Contemporary Events and Mechanisms

Germany underwent a number of tumultuous events between 1933 and 1950, some of which were contemporaneous and might be related to differences in the intensity of the expulsions across regions. We evaluate the potential of various historical events that might confound the relationships that

we find between the expulsions and the long-term political behaviour of the impressionable aged individuals. In this exercise, we are interested primarily in events between 1933 and 1950 that could have had different intensities across regions and the potential to have differential impacts across cohorts. It is important to note that nationwide events and general events that are specific to a particular region or cohort, but not both, are already accounted for in our analyses by our region and birth-year fixed effects. We study a host of these events, and the results are shown in Table 7, with the baseline results included in the first column. It is reassuring that the difference-in-differences estimates remain economically and statistically significant even after accounting for other contemporary historical events.

State-Time Trends

We begin in column (2) of Table 7 and use the fact that we consider lower levels of geographical aggregation than the state to include the linear state-time trends in the regression explicitly in order to account flexibly for statewide events that might have different impacts on different cohorts. We find that the difference-in-differences estimate of the impact of the expulsions on the affected individuals remains negative and statistically significant after controlling for state-time trends. While the estimated impact on an interest in politics is slightly smaller, the estimated impact on participation in local politics is slightly larger, although neither estimate is statistically different from the baseline. This shows that the estimated impact of the expulsions on individuals at impressionable ages is neither confounded nor explained by contemporaneous state-specific policies.

Wartime Destruction

Next, we examine the possibility that our expulsion variable might be capturing wartime experiences that may be correlated with the intensity of the expulsions, and which might have a disproportionate impact on the young. Using the volume of rubble per capita as a proxy for wartime destruction within the region, we re-estimate the baseline equation and include an interaction between wartime destruction and the affected cohort dummy as an additional variable. The results, presented in the third column of Table 7, show that wartime destruction does not have a differential impact across cohorts or across regions within a cohort, all else being the same. Importantly, our baseline

estimates of the impact of the expulsions on a general interest in politics and participation in politics remain largely unchanged. We interpret this result as evidence that the wartime destruction does not explain the results that we find.

[Table 7 about here.]

Adverse Macroeconomic Conditions

One important confounder could be the fact that the intensity of Jewish expulsions may have coincided with the intensity of adverse macroeconomic conditions, which could have had a lasting impact on individuals' preferences and political behaviour (Giuliano and Spilimbergo, 2014). Columns (4) and (5) of Table 7 address this concern by including interactions between the regional unemployment rate, income and population density in 1932 as proxies for regional macroeconomic conditions, and an indicator for the individual being at an impressionable age. We find that including measures of the regional unemployment rate in column (4) does not change the difference-in-differences estimate of the impact of the expulsions significantly for either interest in politics and participation. The impact of the regional unemployment rate on the cohort at an impressionable age is close to zero and statistically insignificant. Furthermore, column (5) also includes controls for income per capita and population density, with an indicator for being in the cohort at an impressionable age. We continue to find that the estimated impacts of the expulsions on the political behaviour of the affected cohort remain largely the same.

Further, Giuliano and Spilimbergo (2014) demonstrate that children and young adults who grow up in times of adverse macroeconomic conditions tend to express greater support for left-leaning parties. In light of this, we assess the impact of the expulsions on the party support of contemporary children and young adults in order to evaluate the importance of macroeconomic conditions for our result further. The results are contained in Table A2 of the Appendix. Columns (2) and (3) indicate that individuals in the affected cohort are significantly more likely to support the Social Democrats (SPD) but no more or less likely to support the Christian Democratic Union (CDU). Looking at broad party support, the results show that, when the major parties are aggregated in columns (4) and (5), individuals in the affected cohort are no more or less likely to support left-leaning (SPD, Greens) or right-leaning (CDU, CSU, FDH) parties in different regions. We take

these results on the absence of differential left- or right-party support, along with the results on the unemployment rate, income, and population density, as evidence that region-by-cohort variation in German macroeconomic conditions does not explain the differences in political behaviour that are associated with the intensity of Jewish expulsions.

Regional Differences in Political Support and Social Capital

Differences in political support and modes of participation across regions in 1933 Germany could also be related to the expulsions and may have had a large impact on the young (Putnam, 2000; Madestam and Yanagizawa-Drott, 2012; Satyanath, Voigtländer and Voth, 2017; Voigtländer and Voth, 2015). For example, regions with higher fractions of Jewish residents in 1933 might have shown stronger support for the Nazi party, and the contemporary young in these places may have felt ostracized, alienated, and unable to participate in politics. A second potential confounding factor arises from the fact that members of the Communist party were also expelled from the civil service at the same time, meaning that our results might also capture the expulsion of communists or the reluctance of individuals in communist-dominated areas to participate in current German politics. Although Waldinger (2010) argues that only a small fraction of professionals were dismissed for being communist, nevertheless, we provide an empirical to rule out this possibility. Third, studies have shown that Jewish persecution were historically less likely in cities with higher Catholic shares (Becker and Pascali, 2016); therefore, we also consider the role of differences in Catholic share in 1933.

In column (6) of Table 7, we utilize information on the percentages of votes for the Nazi and Communist parties in the November 1932 federal elections to account for the potential differential impacts of the support and votes received by these parties on the affected cohorts, as well as initial social capital (support for the Nazi party was positively correlated with initial social capital as shown by Satyanath, Voigtländer and Voth, 2017). The results reveal that the differences in party support at the beginning of the expulsions do not have differential impacts on the long-term political behaviour of the impressionable aged individuals, and the estimates are generally small and not statistically significant. In column (7), we further investigate the importance of initial political participation in a given region, as reflected in turnout rates, and find that regional

turnout rates in the 1932 election are not systematically related to political attitudes of the affected cohorts later on. Moving to column (8), the estimates show that the Catholic share does not have any differential impacts on the cohort in their impressionable ages during the expulsions. Once again, the difference-in-differences estimates remain robust. We therefore conclude that regional differences in initial political support, political participation, or Catholic shares, do not explain the estimated effects of the Jewish expulsions on the long-term political attitudes of German children and young adults.

Urbanization in 1933

Lastly, it is possible that national or statewide policies might have had differential impacts on the young in more densely populated and prosperous regions with generally higher proportions of Jewish people in the population. It could be the different impacts of these policies on individuals who are at an impressionable age that is responsible for the results that we find, rather than the expulsions. Hence, column (9) includes an interaction between the urban share of the region and an indicator for being in the affected cohort. We also note that the urban share could be used as a proxy for macroeconomic conditions, given that urbanization tends to be correlated positively with incomes in the developed world. While we do not find any significant differences in the impact of the urban share on political interest, we do find that individuals who grew up in regions with higher urbanization rates during the expulsions tend to participate more in politics, with this impact being significant at the 90% level. Nevertheless, the impact of the intensity of the 1933 Jewish expulsions on contemporary individuals who are at an impressionable age remains quantitatively and statistically similar, suggesting that our baseline results are not driven by urban shares.

In summary, we have demonstrated that the difference-in-differences estimates of the impacts of Jewish expulsions on the impressionable cohort are robust, and are neither confounded nor explained by a variety of contemporary historical events nor by regional characteristics. More specifically, they are robust to state-specific policies, and even to controlling for the state-specific trends in political participation and interest. Furthermore, we show that the estimated impact of the expulsions is robust to any cohort-specific impacts of WWII, a variety of macroeconomic conditions (unemployment, income, population, and urban share), support for the Nazi and Com-

munist parties, Catholic shares, and initial social capital. Next, we provide an interpretation for the impact of the expulsions on contemporary children and young adults that link social change during the expulsions to socialization, socioeconomic status (SES), and civic skills.

5 Interpretation and Channels

Why would growing up during the expulsions have an impact on political behaviour in adulthood? Our hypothesis is that the expulsions led to significant changes social and human capital that altered the political "resources" acquired by children and young adults, which is in turn reflected in adult political attitudes. The theoretical links between the expulsions and the adult political behaviour of individuals then in their impressionable ages is best described by models of political attitudes that move beyond socioeconomic status, as in Brady, Verba and Schlozman (1995).[20] There are two important aspects of the framework; the first consists of the impacts of social changes on individuals and the environment in which individuals are socialized. These will have impacts on schooling, and labour market outcomes, as have been found in other contexts (Akresh and de Walque, 2008; Blattman and Annan, 2010; Annan et al., 2011; Leon, 2012; Shemyakina, 2011). However, this is not enough to decrease political interest and participation, as studies have found increased political interest among ex-combtatants, for example (Bauer et al., 2016). It must also be the case that conflict inhibits the opportunities to acquire civic skills required for political participation; this could be on the job, at the home, or as part of volunteer and religious organizations such as churches.

Thus, there are a number of theoretical channels linking the expulsions to the adult SES of young Germans who grew up in areas where the expulsions were more intensely felt, and their consequent adult political behaviour. Changes in the social environment as a result of the expulsions would have had a negative impact on socialization in schools (Case, Lin and McLanahan, 2001; Ermisch and Francesconi, 2001; Gruber, 2004), and the acquisition of civic skills within the family

[20]Related theories based on rational choice models would predict that exposed individuals are more likely to choose not to participate because of the reduced net benefits of participation (Downs, 1957; Olson, 1965). We believe that these models perform well for predicting participation in specific political actions, but not the long-term trends in political participation that we examine. For instance, it is not clear why individuals who grew up during the expulsions would only have consistently lower net material benefits from participation in areas with a high pre-1933 proportion Jewish people. Furthermore, Finkel and Muller (1998) and Schlozman, Verba and Brady (1995) show that self-interest is not a good predictor of political participation. Hence, we focus on theories where socioeconomic status influences political participation.

and other social organizations (Jennings and Niemi, 1974; Glass, Bengtson and Dunham, 1986). It is also possible that social capital declined more intensively in areas with greater exposure to the expulsions, and this would have had a negative impact on the socialization of children and young adults (Jennings and Markus, 1977; Satyanath, Voigtländer and Voth, 2017). In addition, Waldinger (2010) demonstrates that the expulsions also had a negative effect on German doctoral students, and Akbulut-Yuksel and Yuksel (2015) show that individuals who were of school age at the time of the expulsions have less schooling; all of which would have negatively impacted adult SES of children growing up during this time period. Lastly, exposure to the expulsions could have also affected aspiration levels which would be reflected in lower levels of schooling, income, and general socioeconomic status (Kosec and Mo, 2017). Our data allows us to test the impacts of the expulsions on different measures of civic skills and socioeconomic status, and this is where we turn to next.

The Empirical Evidence for Causal Channels

Table 8 provides evidence on the impact of the expulsions on the treated cohort's SES, civic skills, and opportunities for the acquisition of civic skills. The table is estimated based on equation 1, but with different dependent variables from the SOEP.[21] Column (1) restricts the sample to individuals who are still working in 1985. Within this sample, we find that exposure to the expulsions is associated with significantly lower incomes in adulthood. Because incomes are measured as log hourly wages, the interpretation is that growing up in a region with the average proportion of Jewish people is associated with incomes that are about 15% (0.1334×1.19) lower than those of other members of the same cohort who grew up in a region with no Jewish population. In column (2), we do not find evidence for any differential impacts on employment, and column (3) of the table shows that the treated cohorts have significantly less schooling, all else being equal.

The results in columns (1) and (3) demonstrate the important impacts of the expulsion on the SES of the exposed cohort in regions with higher proportions of Jewish people. As was explained earlier, this is a result of a combination of factors, including the expulsion of a significant proportion of teachers and professors, changes in the family and school environment, as well as the impacts

[21] Details on the construction of these variables are provided in the Appendix.

of the trauma associated with these events on aspirations, even for non-victims (Kosec and Mo, 2017). These results would therefore imply that lower schooling attainments and incomes led to a decrease in individuals' interest in politics and political participation, because money and civic skills, acquired both at school and at home are resources required for political participation (Brady, Verba and Schlozman, 1995; Jennings and Niemi, 1974; Putnam, 2000).

[Table 8 about here.]

A novel important implication of the framework outlined earlier is that growing up during the expulsions would have decreased individuals' civic skills, and might also have affected opportunities of developing civic skills in adulthood. For instance, SES could influence individuals' job types and organizational responsibilities on the job. There might also be an impact on party support or volunteerism, and therefore on the opportunity to develop civic skills as part of a political party or volunteer organization. While some of these opportunities vary with SES, other activities, such as participation in a church, are not correlated strongly with SES (Brady, Verba and Schlozman, 1995). The link between social change, SES, and civic skills, is important, because studies in other contexts have found that even though exposure to conflict and violence lowers SES, it is also able to spur and encourage political interest and participation (Bauer et al., 2016). Therefore, we provide further evidence on the impact of the expulsions on civic skills, and the opportunity to acquire civic skills in adulthood in columns (4)–(7) of Table 8.

First, notice that the estimates in column (2) of Table 8 show that the treated cohorts are neither more nor less likely to be employed, even though the estimates in column (1) indicate that they earn lower average wages. The relevance of this result can be seen from the similar results of Brady, Verba and Schlozman (1995), which imply that activities that help to develop civic skills on the job are more important than actually being employed, and the opportunity to participate in such job-related activities increases with SES. Hence, we may conclude from the evidence in columns (1) and (2) that the cohort that was exposed to the expulsions while young may be less likely to participate in and develop an interest in politics, not because they are less likely to be employed, but because they are less likely to be involved in job-related activities that build civic skills.

Moving on to column (4) to (7), we find that the treated cohorts are less likely to believe that political activity is generally important. Furthermore, the estimates also show that the exposed cohorts are less likely to participate in volunteer organizations, and have lower levels of trust. Furthermore, they are less likely to attend church, which is an important avenue for the development of civic skills. Taken together, these results provide further channels through which the expulsions may have affected individuals' interest in politics and political participation. An exposure to the expulsions is related to lower rates of both participation in organizations in which civic skills could be developed (volunteer groups and church) and social capital (trust).[22] Consequently, they are also less likely to view political activity as important, which is often related to lower rates of political interest and participation, and lower levels of SES (Brady, Verba and Schlozman, 1995; Finkel and Muller, 1998; Schlozman, Verba and Brady, 1995).

Overall, the empirical results support explanations that are based on a framework in which the expulsions led to significant changes in the socialization environment of the exposed cohort. We show that, consequently, individuals who were of impressionable age at the time of these changes have lower wages and levels of schooling. Based on models that emphasize political resources, as in Brady, Verba and Schlozman (1995), results also show that exposed individuals have less of the resources that are required to participate in politics (money, civic skills, trust), and fewer opportunities to build civic skills even as adults (volunteering, church attendance). Thus, our results on the lower political interest and participation rates of the cohort who were exposed to the expulsions could be understood to be driven by changes in socioeconomic status and civic skills, as a result of growing up during the expulsions.

6 Conclusion

This paper provides the first evidence of the long-term impacts of social changes in impressionable ages on the adult political attitudes of individuals who were not direct victims of the conflict. We use region-by-cohort variation in the exposure to the Jewish expulsions to show that young Ger-

[22]Note that the lower rates of trust could be either a result or a cause of the lower rates of participation in organizations in which civic skills may be acquired. We do not take a stand on this important issue as it is outside the scope of this paper, but see Alesina and Giuliano (2011) or Putnam (2000) for further discussions.

mans who grew up during the expulsions are significantly less likely to be interested or participate in politics. These results are much stronger for children and young adults for whom more of their impressionable years occurred during the expulsions. We also demonstrate that the negative impacts of the persecutions on political behaviour are not driven by other contemporary events and fixed region characteristics that might have been correlated with the expulsions. We are able to rule out the role of economic factors, and other political factors associated with the Nazi regime. Drawing on theories of the links between the social environment, socioeconomic status, civic skills, and political behaviour, we demonstrate that, all else being equal, young people with a greater exposure to the expulsions have relatively lower socioeconomic status (income, schooling), have fewer civic skills (trust), and are less likely to participate in organizations that help to develop civic skills (volunteering, church attendance).

These results add an important new dimension to our understanding of the formation of political attitudes and behaviour, and provide supportive evidence on the impressionable age hypothesis. First, they help us to understand the long-term impacts of social change on political attitudes of young individuals who are at impressionable age. Furthermore, we are able to demonstrate the links between social changes in impressionable years, civic skills, socioeconomic status, and adult political attitudes, which have been discussed but rarely tested using empirical data. We show that events in childhood and young adulthood do indeed matter for political attitudes and behaviours in adulthood, although we do not find these attitudes to spill over to future generations. We believe that the absence of intergenerational spillovers indicates the importance of the broader social environment, beyond the nuclear family, for learning to participate in politics. Our findings also reveal that conflicts have important consequences for adult political behaviour if they negatively affect the environment in which individuals obtained the social and material resources required to participate in politics.

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Table 1: Descriptive Statistics for Regional and Individual Data

	All	High Intensity Regions	Low Intensity Regions	Difference s.e (difference)
	(1)	(2)	(3)	(4)
	Summary of Regional Characteristics			
Percent Jewish in 1933	1.190	1.955	0.715	1.240***
	(0.875)	(0.960)	(0.280)	(0.348)
Percent Jewish in 1946	0.156	0.291	0.073	0.219
	(0.283)	(0.381)	(0.148)	(0.135)
WWII Destruction	15.627	17.122	14.683	2.439
	(7.480)	(9.019)	(6.137)	(3.083)
Unemployment Rate	9.931	9.929	9.933	-0.004
	(2.405)	(2.151)	(2.551)	(0.766)
% Votes to Nazi Party	31.867	33.821	30.658	3.163
	(7.298)	(6.231)	(7.642)	(2.266)
% Votes to Communist Party	17.593	17.432	17.693	-0.261
	(6.211)	(4.977)	(6.864)	(1.952)
% Turnout in 1932 Elections	80.838	79.767	81.513	-1.746
	(5.106)	(5.530)	(4.700)	(2.082)
Urban Share	0.604	0.592	0.611	-0.019
	(0.157)	(0.118)	(0.176)	(0.047)
% of Catholics in 1933	43.646	44.157	43.330	0.827
	(28.933)	(28.109)	(29.437)	(11.072)
Income per Capita in 1932 (in RM)	474.900	504.368	456.303	48.064
	(103.711)	(70.599)	(116.233)	(32.679)
Population Density in 1933	1,563	1,771	1,435	336
	(734.551)	(842.193)	(625.865)	(318.086)
Area in km^2 in 1933	264.329	317.347	231.523	85.824
	(211.083)	(251.813)	(173.516)	(89.037)
	Summary of Individual Characteristics			
Interest in Politics	0.361	0.363	0.3600	0.003
	(0.480)	(0.481)	(0.480)	(0.020)
Political Participation	0.082	0.084	0.081	0.003
	(0.275)	(0.277)	(0.273)	(0.012)
Importance of Political Activity	0.233	0.201	0.252	-0.051
	(0.423)	(0.401)	(0.434)	(0.021)
Volunteer	0.206	0.205	0.206	-0.001
	(0.404)	(0.404)	(0.405)	(0.017)
Trust	0.622	0.653	0.605	0.048
	(0.485)	(0.477)	(0.489)	(0.035)
Church Attendance	0.270	0.290	0.258	0.031
	(0.444)	(0.454)	(0.438)	(0.022)
Years of Schooling	11.320	11.435	11.248	0.187
	(2.311)	(2.391)	(2.258)	(0.105)
Employment	0.616	0.642	0.599	0.043
	(0.487)	(0.480)	(0.490)	(0.023)
Ln(wage)	8.944	8.974	8.924	0.050
	(0.969)	(0.912)	(1.005)	(0.062)
Age	47.275	47.280	47.272	0.008
	(18.458)	(18.347)	(18.533)	(0.776)
Female	0.537	0.534	0.539	-0.005
	(0.499)	(0.499)	(0.499)	(0.021)
Rural	0.587	0.577	0.592	-0.015
	(0.493)	(0.494)	(0.492)	(0.021)
Observations Baseline Max.	2399	917	1482	2399

Notes: The data are from the 1985 GSOEP, and the means presented are weighted by population. High-intensity regions are defined as regions with above average Jewish population shares in 1933, and low-intensity regions are defined as regions with below-average Jewish population shares. WWII destruction is defined as the volume of rubble per capita. All other data are as described in the text. Standard deviations and errors are given in parentheses. Standard errors for mean differences are clustered at the region-level. Asterisks denote significance levels (* = 0.10, ** = 0.05, *** = 0.01).

Table 2: Estimated Effects of Social Change during Expulsions on Political Attitudes

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Discrete Measure of Treatment in Impressionable Age					
	General Interest in Politics (Mean = 0.361)			**Participation in Local Politics (Mean = 0.082)**		
Percent Jewish in '33 × Impressionable Age	−0.0392**	−0.0462**	−0.0443**	−0.0184**	−0.0187**	−0.0179*
	(0.0174)	(0.0173)	(0.0193)	(0.0074)	(0.0087)	(0.0096)
Mother Education		0.2329***	0.1464***		0.0746***	0.051
		(0.0398)	(0.0534)		(0.0248)	(0.0364)
Father Education			0.1433***			0.050
			(0.0444)			(0.0347)
R^2	0.113	0.144	.152	0.055	0.074	0.077
Observations	2389	2064	2009	2357	2041	1987
	Panel B: Continuous Measure of Treatment in Impressionable Age					
	General Interest in Politics (Mean = 0.361)			**Participation in Local Politics (Mean = 0.082)**		
Percent Jewish in '33 × Length of Exposure	−0.0026**	−0.0036**	−0.0033**	−0.0011*	−0.0020***	−0.0019**
	(0.0013)	(0.0014)	(0.0015)	(0.0006)	(0.0007)	(0.0008)
Mother Education		0.2362***	0.1663***		0.0756***	0.053
		(0.0326)	(0.0445)		(0.0260)	(0.0350)
Father Education			0.1236***			0.0490*
			(0.0376)			(0.0279)
R^2	0.110	0.141	0.150	0.044	0.059	0.063
Observations	3515	3048	2974	3466	3014	2941

Notes: Standard errors, clustered by regions, are shown in parentheses. Asterisks denote significance levels (* = 0.10, ** = 0.05, *** = 0.01). Impressionable age is defined as individuals born between 1910 and 1927. Mother education is an indicator variable that equals 1 if the mother has more than basic education, and father education is similarly defined. The control group is individuals born between 1951 and 1960. The second panel of the table shows the estimated impact of Jewish expulsions using a continuous measure of exposure to expulsions in childhood and young adulthood. We define exposure as having lived during the expulsions and the Nazi Regime. We assume that the child was affected by the Nazi Regime if they were between 6 and 23 years of age at any time during the period 1933 to 1945. All regressions control for region and birth-year fixed effects, along with gender and rural dummies. The point estimates are also allowed to vary by parental characteristics.

Table 3: Falsification Tests

	Interest (1)	**Participation** (2)	**Interest** (3)	**Participation** (4)
Percent Jewish in '33 × Born btw. 1951–1960	0.0165	0.0052	0.0264	−0.0019
	(0.0226)	(0.0120)	(0.0283)	(0.0134)
Placebo Control Group	Born in 1961–1970		Born in 1900–1909	
R^2	0.103	0.039	0.118	0.059
Observations	2250	2229	1481	1466

Notes: Standard errors, clustered by regions, are shown in parentheses. Asterisks denote significance levels (* = 0.10, ** = 0.05, *** = 0.01). The placebo treated group are individuals born between 1951 and 1960, and the placebo control groups are older and younger untreated individuals. Each column is from a separate regression and controls for region and birth-year fixed effects, along with gender and rural dummies.

Table 4: Robustness to Different Definitions of Impressionable Ages

	Born btw. 1910–1923 (1)	**Born btw. 1910–1933** (2)	**Born btw. 1910–1938** (3)	**Born btw. 1910–1945** (4)	**Born btw. 1915–1927** (5)	**Born btw. 1908–1927** (6)
	General Interest in Politics (Mean = 0.361)					
Percent Jewish in '33 × Birth Year Band	-0.0489***	-0.0366**	-0.0290*	-0.0275*	-0.0407**	-0.0383**
	(0.0176)	(0.0155)	(0.0166)	(0.0159)	(0.0190)	(0.0173)
R^2	0.111	0.106	0.111	0.111	0.117	.108
Observations	2019	2952	3515	4340	2123	2488
	Participation in Local Politics (Mean = 0.082)					
Percent Jewish in '33 × Birth Year Band	-0.0122*	-0.0151**	-0.0099	-0.0026	-0.0202**	-0.0166**
	(0.0071)	(0.0067)	(0.0073)	(0.0061)	(0.0081)	(0.0077)
R^2	0.053	0.047	0.045	0.04	0.053	.055
Observations	1992	2906	3466	4280	2098	2454

Notes: The table shows the estimated impact of the Jewish expulsions using a range of definitions of "impressionable years." Standard errors, clustered by region, are shown in parentheses. Asterisks denote significance levels (* = 0.10, ** = 0.05, *** = 0.01). The control group is individuals born between 1951 and 1960. All regressions control for region and birth-year fixed effects, along with gender and rural dummiesF.

Table 5: Robustness to Different Impressionable Ages and Control Groups

	Born btw. 1910–1923 (1)	**Born btw. 1910–1927** (2)	**Born btw. 1910–1933** (3)	**Born btw. 1910–1938** (4)	**Born btw. 1910–1945** (5)	**Born btw. 1915–1927** (6)	**Born btw. 1908–1927** (7)
	General Interest in Politics (Mean = 0.361)						
Percent Jewish in '33 ×	-0.0448***	-0.0335**	-0.0310**	-0.0243*	-0.0235*	-0.0328**	-0.0326**
Birth Year Band	(0.0154)	(0.0152)	(0.0128)	(0.0146)	(0.0137)	(0.0163)	(0.0157)
R^2	0.11	0.111	0.106	0.109	0.109	0.114	.108
Observations	2574	2944	3507	4070	4895	2678	3043
	Participation in Local Politics (Mean = 0.082)						
Percent Jewish in '33 ×	-0.0107	-0.0176**	-0.0146*	-0.0111**	-0.0039	-0.0188**	-0.0157**
Birth Year Band	(0.0091)	(0.0084)	(0.0075)	(0.0055)	(0.0062)	(0.0089)	(0.0083)
R^2	0.049	0.051	0.045	0.043	0.04	0.048	.051
Observations	2536	2901	3450	4010	4824	2642	2998

Notes: The table shows the estimated impact of the Jewish expulsions using a range of definitions of "impressionable years." Standard errors, clustered by region, are shown in parentheses. Asterisks denote significance levels (* = 0.10, ** = 0.05, *** = 0.01). **The control group is now individuals born between 1946 and 1960**. All regressions control for region and birth-year fixed effects, along with gender and rural dummies.

Table 6: Robustness to Differences in Sample Selection

	Non-Movers Only				
	Interest in Politics (1)	**Participation in Politics** (2)	**Internal Migration** (3)	**Mortality** (4)	**Cohort Size** (5)
Percent Jewish in '33	−0.0404*	−0.0209*	−0.0262	−0.0078	−0.1435
× **Impressionable Age**	(0.0269)	(0.0110)	(0.0177)	(0.0088)	(0.2058)
R^2	0.138	0.077	0.108	0.527	0.518
Observations	1308	1290	2348	2354	2354

Notes: Standard errors, clustered by regions, are shown in parentheses. Asterisks denote significance levels (* = 0.10, ** = 0.05, *** = 0.01). Our baseline definition of impressionable age consists of individuals born between 1910 and 1927. Each column is from a separate regression, controlling for region and birth-year fixed effects, along with gender and rural dummies. Individuals are coded as movers if they report that they no longer live in their childhood region. Mortality is computed by exploring the panel nature of the data.

Table 7: Expulsions and Political Behaviour: Robustness to Alternative Historical Events

	Base Results (1)	State Trends (2)	WWII Destruction (3)	Unemp. Rate (4)	Pop. Density & Income (5)	Party Support (6)	Voter Turnout (7)	Catholic Share (8)	Urban Share (9)	All (10)
					General Interest in Politics (Mean = 0.361)					
Percent Jewish in '33	-0.0392**	-0.0337*	-0.0409**	-0.0384**	-0.0345*	-0.0392**	-0.0364**	-0.0335**	-0.0394**	-0.0380**
× Impressionable Age	(0.0174)	(0.0172)	(0.0175)	(0.0159)	(0.0180)	(0.0154)	(0.0175)	(0.0156)	(0.0180)	(0.0191)
WWII Destruction			0.0018							0.0016
× Impressionable Age			(0.0023)							(0.0027)
Unemployment Rate in 1932				0.0101						-0.0004
× Impressionable Age				(0.0088)						(0.0125)
Population Density in 1933					-0.0001					-0.0001
× Impressionable Age					(0.0002)					(0.0002)
Income per Capita in 1932					-0.0001					-0.0002
× Impressionable Age					(0.0002)					(0.0002)
% of Votes to Nazi Party						0.0003				0.0024
× Impressionable Age						(0.0026)				(0.0041)
% of Communist Vote						0.0048				0.0064
× Impressionable Age						(0.0041)				(0.0069)
% Voter Turnout							-0.0037			-0.0029
× Impressionable Age							(0.0029)			(0.0065)
% Catholic								0.0005		0.0001
× Impressionable Age								(0.0006)		(0.0013)
Urban Share									0.1319	-0.0792
× Impressionable Age									(0.1242)	(0.1663)
R^2	0.113	0.118	0.113	0.114	0.113	0.114	0.113	0.114	0.113	.114
Observations	2389	2389	2360	2389	2360	2389	2364	2389	2389	2335
					Participation in Local Politics (Mean = 0.082)					
Percent Jewish in '33	-0.0184**	-0.0191**	-0.0200**	-0.0180**	-0.0211**	-0.0197***	-0.0182**	-0.0124*	-0.0184**	-0.0166*
× Impressionable Age	(0.0074)	(0.0074)	(0.0082)	(0.0073)	(0.0080)	(0.0066)	(0.0078)	(0.0071)	(0.0074)	(0.0084)
WWII Destruction			0.0005							0.0001
× Impressionable Age			(0.0013)							(0.0016)
Unemployment Rate in 1932				0.0067						0.0017
× Impressionable Age				(0.0042)						(0.0061)
Population Density in 1933					-0.0001					-0.0001
× Impressionable Age					(0.0002)					(0.0002)
Income per Capita in 1932					-0.0001					0.0001
× Impressionable Age					(0.0002)					(0.0002)
% of Votes to Nazi Party						0.0012				0.0005
× Impressionable Age						(0.0015)				(0.0020)
% of Communist Vote						0.0027				-0.0002
× Impressionable Age						(0.0018)				(0.0032)
% Voter Turnout							0.0020			0.0034
× Impressionable Age							(0.0018)			(0.0030)
% Catholic								-0.0001		0.0005
× Impressionable Age								(0.0003)		(0.0005)
Urban Share									0.1441**	0.1390
× Impressionable Age									(0.0590)	(0.1055)
R^2	0.055	0.057	0.055	0.056	0.055	0.056	0.056	0.057	0.055	0.058
Observations	2357	2357	2328	2357	2328	2357	2332	2357	2357	2303

Notes: Standard errors, clustered by regions, are shown in parentheses. Asterisks denote significance levels (* = 0.10, ** = 0.05, *** = 0.01). Our baseline definition of impressionable age consists of individuals born between 1910 and 1927. The control group is individuals born between 1951 and 1960. All regressions control for region and birth-year fixed effects, along with gender and rural dummies.

Table 8: Social Change and Political Behaviour: Potential Channels

	Income (1)	**Employment** (2)	**Schooling** (3)	**Importance of Pol. Activity** (4)	**Volunteer** (5)	**Trust** (6)	**Church** (7)
% Percent Jewish in '33	-0.1334***	0.022	-0.4074***	-0.0556***	-0.0306**	-0.0491*	-0.0312*
× Impressionable Age	(0.0400)	(0.0211)	(0.0706)	(0.0144)	(0.0143)	(0.0270)	(0.0185)
R^2	0.280	0.327	0.181	0.081	0.074	0.1030	0.174
Observations	1026	1896	2385	1713	2350	852	1721

Notes: Standard errors, clustered by regions, are shown in parentheses. Asterisks denote significance levels (* = 0.10, ** = 0.05, *** = 0.01). The control group is individuals born between 1951 and 1960. All regressions control for region and birth-year fixed effects, along with gender dummies and rural dummies. We test the ability of various channels to explain the estimated patterns of political activity and interest according to the resource-based socioeconomic status model described by Brady, Verba and Schlozman (1995).

Appendix: Additional Tables and Variable Definitions

Additional Tables

Table A1: Estimated Effects of Social Change Due to Expulsions Across Cohorts

	(1)	(2)	(3)	(4)
	General Interest in Politics (Mean = 0.361)		**Participation in Local Politics (Mean = 0.082)**	
Percent Jewish in '33	-0.0382	-0.0382	-0.014	-0.0125
× Born 1910-1914	(0.0309)	(0.0314)	(0.0098)	(0.0099)
Percent Jewish in '33	-0.0349*	-0.0333*	-0.0202**	-0.0181**
× Born 1915-1927	(0.0185)	(0.0180)	(0.0080)	(0.0079)
Percent Jewish in '33		-0.0179		-0.0038
× Born 1928-1938		(0.0167)		(0.0084)
R^2	0.113	0.111	0.055	0.044
Observations	2389	3515	2357	3466

Notes: Standard errors, clustered by regions, are shown in parentheses. Asterisks denote significance levels (* = 0.10, ** = 0.05, *** = 0.01). Table presents results by cohort. All regressions control for region and birth-year fixed effects, along with gender and rural dummies, along with parental characteristics.

Definitions of Some Variables from SOEP

Interest in Politics: This comes from Question Bp75 of SOEP: "First of all in general: How interested are you in politics?" We classify an individual as interested if he responds as having strong or very strong interest, and non-interested if weak or no interest. Missing values are left as missing in all cases.

Participation in Local Politics: This comes from the time use data in SOEP: "Which of the following activities do you do in your free time? Please enter how often you practice

Table A2: Impact of Expulsions on Party Support

	Support a Party	**Social Democrats**	**Christian Democratic U.**	**Left-Leaning (SPD,Greens)**	**Right-Leaning (CDU, CSU, FDH)**
	(1)	(2)	(3)	(4)	(5)
Percent Jewish in '33	0.0192	0.0516**	−0.0152	0.0091	−0.0039
× **Born btw. 1910–1927**	(0.0257)	(0.0249)	(0.0274)	(0.0241)	(0.0217)
R^2	0.071	0.107	0.192	0.1480	.150
Observations	2377	1478	1478	1478	1478

Notes: Standard errors, clustered by regions, are shown in parentheses. Asterisks denote significance levels (* = 0.10, ** = 0.05, *** = 0.01). Each column is from a separate regression. Each column controls for region and birth-year fixed effects. Other controls in each regression are gender and rural dummies. The table estimates the impact of an exposure to the expulsions on both general party support and support for particular major parties.

Table A3: Correlation between Possible Channels and Political Attitudes

	Interest in Politics	**Participation in Politics**
	(1)	(2)
Income	0.208	0.076
Employment	0.096	0.101
Years of Schooling	0.282	0.171
Importance of Political Activity	0.204	0.180
Volunteer	0.126	0.312
Trust	0.084	0.050
Church	-0.040	0.005

each activity. Bp0707: Participation in citizen initiatives, parties, community politics." Individuals are classified as participants if they participate "less frequently" than monthly,

or more frequently, i.e., monthly and weekly. Individuals are classified as not participating if they never participate.

Length of Exposure: Length of exposure is calculated as the length of time that an individual would have lived under the Nazi regime (1933–1945) between the ages of 6 and 23 (impressionable years). If we call the length of exposure "Treat", it is generated as below:

Table A4: Calculation of the length of Exposure

Treat	**Birth year**
1	1910 or 1938
2	1911 or 1937
3	1912 or 1936
4	1913 or 1935
5	1914 or 1934
6	1915 or 1933
7	1916 or 1932
8	1917 or 1931
9	1918 or 1930
10	1919 or 1929
11	1920 or 1928
12	Between 1921 and 1927 (inclusive)
0	Before 1910 or after 1938

Importance of Political Activity: This comes from the question in SOEP: "Different individuals find different things in life important. How important are the following things to you today? Gp0209: To be politically/socially involved." Individuals are classified as viewing politics as important if they say politics is "important" or "very important". Individuals are classified as not viewing politics as important if they indicate it is "not very important" or "unimportant."

Trust: Our measure of trust comes from Question Tp0301: "On the Whole Trust People." Individuals are classified as trusting if they respond that they "Agree" or "Agree Slightly," and not trusting if they respond that they "Disagree Slightly" or "Totally Disagree."

Church Attendance: This comes from the SOEP: "Now some questions about your free time. How frequently do you do the following activities? Gp0408: Go to church or religious institutions." Individuals are classified as going to church if they go at least monthly.

Volunteerism: This comes from Gp0407 of the SOEP: "Now some questions about your free time. How frequently do you do the following activities? Gp0408: Volunteer work in clubs, associations, or social services." Individuals are classified as volunteering if they participate at least monthly.

Party Support: This comes from Question Bp7901 of the SOEP: "Many people in the Federal Republic of West Germany are inclined to a certain political party, although from time to time they vote for another political party. What about you: Are you inclined—generally speaking—to a particular party?" Individuals are classified as supporting a party if they answer "yes."

If the response above is "yes," they are asked "Which Party?" We classify CDU, CSU and FDH as right-wing, and SDP and Greens as left-wing, as is standard in the literature.