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The Long-Term Direct and External Effects of Jewish Expulsions in Nazi Germany[1]

Mevlude Akbulut Yuksel and Mutlu Yuksel

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Abstract: This paper examines the long-term direct and spillover effects of large-scale human capital loss caused by the persecution of Jewish professionals in Nazi Germany. Using region-by-cohort variation in the Jewish population as a quasi-experiment, we find that on average German children who were of school age during the persecutions have fewer years of schooling in adulthood, and are less likely to finish high school or go to college. These results are robust after controlling for regional unemployment and income, wartime destruction, Nazi and Communist Party support, the compulsory schooling reform, migration, urbanization and mortality.

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JEL Classification: I21, I28, J24, N34

An extensive literature on endogenous economic growth has emphasized the role of human capital as a source of long-run differences in income levels across countries. The loss of productive skills due to armed conflicts, ethnic cleansing, natural disasters, diseases and brain drain immigration has become common during the last century (Blatmann and Miguel, 2010; Carrington and Detragiache, 1998; de Walque and Verwimp, 2010). Evidence from macro-level studies suggests that the loss of high-skilled professionals has numerous adverse effects on macroeconomic performance and growth prospects (Klenow and Rodriguez Clare, 2005; Lucas, 1988). More importantly, the lack of highly skilled professionals leaves children and young adults vulnerable, given the age-specific aspect of many human capital investments.

This paper provides causal evidence on long-term direct and spillover effects of the loss of high-skilled professionals on children's outcomes. Specifically, we analyze the long-term consequences of the persecution of Jewish professionals in Nazi Germany on German children's human capital formation. The Nazi Party passed the "Law for the Restoration of the Professional Civil Service" in April 1933 shortly after seizing power.[1] This law allowed the government to purge Jews from the civil service, a vast organization in Germany that included teachers, university professors, judges and many other professionals. In May 1933, this law was amended to include the postal service, railroads, communal health insurance systems, professional associations, unemployment and salaried employees' insurance, miners' guilds, trade guilds and chambers of commerce (Kaplan, 2005). With the civil service law, more than 15 percent of university professors, teachers, doctors, lawyers and other professionals were dismissed from their professions (Strauss, 1983; German Statistical Yearbooks)[2]

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[1]In German, "*Gesetz zur Wiederherstellung des Berufsbeamtentums*"

[2]Starting from 1933, many Jewish professionals including Albert Einstein, Gustav

There was a significant regional variation in the extent of dismissals within Germany. On the one hand, areas with a higher fraction of Jewish population were exposed to a dramatic change in the number of teachers, professors and other professionals compared to regions with a lower fraction of Jews.[3] On the other hand, only individuals who were of school-age during the Nazi Regime would have had their human capital formation affected by the exile of Jewish professionals. This paper therefore uses a difference-in-differences-type strategy where the "treatment" variable is an interaction between the Jewish population in German regions in 1933 and a dummy variable for being school-aged during the Nazi Regime where we always control for region and birth year fixed effects.

We find that the expulsion and exile of Jewish professionals had enduring detrimental effects on German children's educational attainment. Children who were school-aged during the persecutions attained 0.5 fewer years of schooling on average. These children are also less likely to finish high school and attend a technical school or university. More significantly, we find that the dismissal of Jewish professors and schoolteachers had a sizable adverse effect on German children's education decision. During the time period we focus on in our paper, Germany also experienced additional political and economic events. The nationwide effects of these events are captured by the birth year fixed effects in our analysis. We also use a lower level of geographical aggregation than state in our estimation, which allows us to account for potential state-specific events and policies. In addition, we formally test whether the affected cohorts were differentially affected by these events in Section 5. We find that long-term adverse effects of the Jewish

expulsions remain economically and statistically significant even after we account for regional unemployment and income per capita, wartime destruction, the timing of the compulsory schooling reform, migration, mortality and urbanization. Also, our results are robust to the different categorization of the affected and the control cohorts.

This paper makes several contributions. First, our study contributes to the broader literature exploring the association between large negative shocks and children's human capital formation. This strand of literature finds that armed conflicts, natural disasters, political instability and macroeconomic crisis have long-lasting, detrimental effects on children's educational attainment (Ichino and Winter-Ebmer, 2004; Akbulut-Yuksel, 2009; Meng and Gregory, 2007). Blattman and Miguel (2010) point out that the rigorous research on long-term effects of forced high skilled emigration caused by such events is very limited. Therefore, it is critical to understand the long-run effects of the loss of high-skilled professionals and the mechanisms through which it affects children and young adults around the globe. Our paper adds to this literature by quantifying the long-term consequences of systematic persecution of Jewish professionals in Nazi Germany on children's human capital formation.

Our study also contributes to a small but growing literature on the long-term economic and political legacies of the Holocaust (Acemoglu, Hassan and Robinson, 2011; Grosfeld, Rodnyansky and Zhuravskaya, *forthcoming*; Pascali, 2012). Acemoglu, Hassan and Robinson (2011) show that German occupation led to lower postwar growth, long-term GDP per capita and lower average wages in Russian cities and regions with a higher prewar Jewish population. Grosfeld, Rodnyansky and Zhuravskaya (*forthcoming*) and Pascali (2012) provide further evidence on the long-term legacies of the Holocaust in Europe. Using variation in the "Pale settlement," Grosfeld, Rodnyansky and Zhuravskaya (*forthcoming*) find that cities in Pale settlement have stronger support for so-

cialist anti-market parties, lower rates of entrepreneurship and higher levels of trust today. Similarly, Pascali (2012) documents that Italian cities with large Jewish communities in the 15th century currently have complex bank institutions. Our paper adds to these recent studies by estimating the individual-level effects of the Holocaust. In addition, we provide formal evidence on the potential channels through which the purge of Jews affected human capital formation in Germany, such as a loss of professors and teachers. Our final contribution is to combine the newly assembled historical data on regional characteristics and the Jewish population with individual-level data from the German Socio-Economic Panel (GSOEP), which allows us to control for individual and family background characteristics in our analysis in addition to regional characteristics.

This paper is closely related to two recent studies by Waldinger (2012; 2010). Waldinger (2012) finds that the productivity of peer scientists in the departments of dismissed Jewish professors remained virtually unchanged after the dismissals. On the other hand, Waldinger (2010) shows that in mathematics departments, the dismissal of Jewish professors had negative long-term consequences for the PhD students who graduated from these departments. The persecution of Jews from all professions and the associated loss of human capital in Germany at large, however, are likely to have more pervasive long-term effects beyond German peer researchers in the sciences. In our study, we take the potential spillover effects arising from the loss of high skilled professionals into account; hence we estimate the long-term legacies of systematic expulsion of the Jewish population on the entire German population.

The remainder of the paper is organized as follows. Section 2 provides a brief background of the purge of Jewish professionals during the Nazi Regime. Section 3 discusses the identification strategy. Section 4 describes the data used in the analysis. Section 5 presents the main

results, extensions and robustness checks. Section 6 concludes.

2 Background on Dismissal of Jewish Professionals during the Nazi Regime

The 1933 census documented that there were 525,000 individuals with Jewish adherence in Deutsche Reich at that time (Friedlander, 1998; Evans, 2005). Although Jews comprised approximately 0.8 of the population, they had been remarkably successful in German society since their emancipation from legal restrictions in the course of the 19th century (Evans, 2005). The Jewish community gained visibility by gradually concentrating in academia and in professions such as medicine, teaching, law, journalism, banking and finance in large numbers (Kaplan, 2005).

The systematic persecution of the Jews began shortly after Hitler's Nazi Party came into power in 1933. Two turning points took place in the first few months of the Nazi Regime: the boycott of Jewish stores on April 1, 1933, and the exclusion of Jews from the civil service through the "Law for the Restoration of the Professional Civil Service" on April 7 (Evans, 2005). This law allowed the Nazi government to purge Jews from civil service, a vast organization in Germany that included teachers, professors, judges and many other professionals that were not government-controlled in other countries. In May 1933, the civil service law was amended to include supplemental regulations for other status groups, and the extensions of the term "civil service" to include the postal service, railroads, communal health insurance systems, professional associations, unemployment and salaried employees' insurance, miners' guilds, trade guilds and chambers of commerce (Kaplan, 2005).

According to the "Law for the Restoration of the Professional Civil Service," a person was considered non-Aryan if he is descended from

non-Aryan, particularly Jewish parents or grandparents. It was sufficient if one parent or grandparent was of the Jewish faith (Yahil, 1991). With this law, the Nazi government required all employees and officials in the public sector to present the Aryan certificate in order to identify the Jewish population (Heinemann, 1999). The Aryan certificate encompassed the seven birth or baptism certificates (the person, his parents and grandparents) and three marriage certificates (parents and grandparents) or certified proofs thereof. In the event that Aryan descent is questionable, the expert on racial research from the Ministry of Interior was asked to determine the individual's race/ethnicity (Yahil, 1991).

As a result of the "Law for the Restoration of the Professional Civil Service," a growing number of Jewish teachers were forced to leave their jobs in the general school system. The German Statistical Yearbooks report that 8.3% of teachers were dismissed by the Nazi government in 1933 in Germany; however there was a substantial variation across German regions in the percentage of teachers that were dismissed. For example, 15% of male secondary school heads in Prussia and 32% of female secondary school heads in Berlin were fired in 1933. Similarly, 7.2% of male senior teachers were dismissed from their jobs in Prussia in 1933 (Evans, 2005). The Jewish teachers who were not fired in April 1933, were compulsorily pensioned off in 1935. Two years later, Jews and "half-Jews" were formally banned from teaching in non-Jewish schools. In 1936, there were 1,335 unfilled posts in elementary schools. The school system needed an additional 8,000 teachers a year to fill the open positions (Evans, 2005). Due to the teacher shortage, the class size on average in all schools had increased to 43 students during this period, compared to 37 in 1927 (Evans, 2005).

With the civil service law, Jewish professors in all fields were also dismissed. Approximately 15 percent (1,200 to 1,500) of university professors of all categories were dismissed and forced to emigrate from Germany after 1933 (Strauss, 1983). When non-university research sci-

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entists and scholars who had begun but not completed their training at the time of emigration are included, this figure becomes approximately 2,000. The replacement of the dismissed Jewish professors took years due to a shortage of qualified researchers as well as slow appointment procedures (Evans, 2005; Waldinger, 2010). Waldinger (2010) also suggests that the positions of the dismissed professors could only be filled if the dismissed professors gave up their pension rights, because the dismissed were originally placed into early retirement. The states were reluctant to pay the salaries for the replacement and the pensions for the dismissed professors at the same time. Therefore the re-appointments took a long time.

Further legislation in 1933 sharply curtailed "Jewish activity" in the medical and legal professions. Within the same year, 16 percent of the lawyers lost their jobs because of the anti-Semitic persecutions (Kondrad, 1986). In July 1933, Jewish doctors lost their patients with substitute health insurance schemes, and private insurance companies reimbursed the fees of Jewish doctors only for their Jewish patients. In 1938, all Jewish doctors lost their medical licenses, and Jewish lawyers lost their admission to the bar. Removal of Jewish doctors from the profession created a large number of vacancies for German graduates to fill (Yahil, 1991).

Taken together, Germany lost a substantial fraction of high-skilled professionals in all occupations within the civil service law. The severity of the human capital lost depended upon the fraction of the Jewish population residing in German regions before the expulsions began in 1933. In our main analysis, we will take the cross-region variation in the percentage of *ex ante* Jewish population in Nazi Germany as exogenous to children's human capital investment after we control for region fixed effects.

3 Identification Strategy

In this section, we describe our strategy for identifying the causal effect of the persecutions on the long-term outcomes of German children. This strategy exploits the plausibly exogenous region-cohort variation in the fraction of Jewish population in Nazi Germany. This is a generalized difference-in-differences strategy where the "treatment" variable is an interaction between the percentage of Jews in German regions in 1933 and a dummy variable for being school-aged during the Nazi Regime.[4] In particular, the proposed estimate of the average treatment effect is given by β in the following baseline region and birth year fixed effects equation:

$$Y_{irt} = \alpha + \beta(JewishFraction_r * AffectedCohort_{it}) + \delta_r + \gamma_t + \pi'\mathbf{X}_{irt} + \epsilon_{irt}$$

(1)

where Y_{irt} is the outcome of interest for individual i in region r born in year t. $JewishFraction_r$ is the percentage of Jewish population in 1933 in region r. $AffectedCohort_{it}$ is a dummy variable that takes a value of 1 if individual i was born between 1910 and 1927 and zero otherwise. δ_r are region-specific fixed effects, controlling for the fact that regions may be systematically different from each other. γ_t are the birth year-specific fixed effects, controlling for the nationwide common shocks. X_{irt} is a vector of individual and household characteristics including gender and urban dummies and parental education. ϵ_{irt} is a random, idiosyncratic error term. The standard errors are clustered by region.

Individuals who were born between 1910 and 1927 form the affected cohorts since they were of primary school and college age in 1933 when the Nazi government forced the Jewish population out of the civil ser-

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[4]This paper provides evidence on the long-term impacts of Jewish expulsions using region-by-cohort variation in *ex ante* Jewish population within Germany; therefore this approach may yield lower bound estimates for the aggregate nation-wide effects of the persecution of Jews.

vice, independent professions and higher education. Thus, their schooling has the potential to be affected by the dismissal of Jewish professionals. [5] On the other hand, individuals born between 1951 and 1960 constitute the control group. These later birth cohorts attained their education after the postwar reconstruction was over in the late 1950s (Akbulut-Yuksel, 2009); therefore their educational attainment has not been affected by the persecution of Jewish professionals.[6]

In order to interpret β as the effect of expulsions, we must assume that had persecutions not occurred, the difference in educational attainment between the affected cohorts and the cohorts born after WWII would have been the same across regions of varying intensity of *ex ante* Jewish population. We assess the plausibility of this assumption below by performing two falsification tests. We first repeat the analysis using only the oldest cohorts who were already beyond school age before the expulsions. Second, we perform an additional falsification test by also including the younger cohorts who started their schooling after the postwar reconstruction period was over in the late 1950s. Moreover, in Figure 2, we present the average years of schooling for birth cohorts residing in regions with high and low fractions of Jewish population. In all of these falsification tests, we find that the parallel trend assumption holds, which lends credence to the difference-in-differences estimation.

4 Data and Descriptive Statistics

The measure of *ex ante* Jewish population we use for our main analysis is from Kessner (1935), who reports the German population statis-

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[5]The estimation results are robust to different age cut-offs and birth years, as reported in Appendix Table 1.

[6]An alternative control group could be individuals born before 1910. Estimation results where cohorts born before 1910 form the control group are presented in Appendix Table 2. We find quantitatively similar results to the baseline specification. Individuals born before 1910, however, are at 76 years of age and older in 1985, leading to very small sample sizes.

tics from the German Population and Occupation Census conducted in 1933. Kessner provides city-level information on the religious affiliation of the German population in 1933. We use the percentage of the population that reported they have Jewish faith as a measure of Jewish presence.[7] Since the dismissal and emigration of German Jews began after the passage of the "Law for the Restoration of the Professional Civil Service" in 1933, we believe that these data give the most accurate available information on the fraction of Jewish population in Germany before the persecutions started. In addition, we compile city-level data on the fraction of Jewish population residing in Germany in 1946 (Einer, 1949). Using both *ex ante* and *ex post* percentage of Jews in Germany, we generate an alternative measure for the loss of Jewish population.

We also assemble detailed information from various years of German Municipalities Statistical Yearbook on regional characteristics, including average income per capita and unemployment rate in 1932, the share of votes the Nazi Party (NSDAP) and the Communist Party (KPD) received in two federal elections in 1932, and area and population in 1933. Second, we collected region-year data on the number of teachers and professors before and after 1933 to assess the school inputs available to the affected cohorts. Finally, we compiled region-level information on the aggregate residential rubble in m^3 per capita in Germany by the end of WWII, which is what we use as a measure of wartime destruction.[8]

The data on individual and household characteristics are from the German Socio-Economic Panel (GSOEP). GSOEP is a household panel survey that is representative of the entire German population residing in private households. It provides a wide range of information on in-

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[7]The purge of Jews was based on racial criteria, not their religious adherence (Evans, 2005). There is no record of the number of Germans who were racial Jews until 1939. In the 1939 German Census, 91.5 percent of the Jewish population had Jewish faith and 8.5 percent were not of Jewish faith (Blau, 1950).

[8]Kaestner (1949) also provides information on the percentage of the residential dwellings destroyed in 1946 in the territory of former West Germany. The correlation between these two measures of WWII destruction is 0.9; thus we present the results with rubble per capita measure.

dividual and household characteristics as well as parental background and childhood environment. GSOEP also asks respondents whether they still live in the city or area where they grew up. This question helps us to identify whether individuals still reside in their childhood city or area. We restrict the empirical analysis to individuals born between 1910 and 1960. These individuals are 25 and older in 1985; thus it is very likely that they have completed their schooling. We dropped individuals born between 1928 and 1950 from the analysis since their human capital formation has been affected by exposure to WWII destruction and postwar reconstruction (Ichino and Winter-Ebmer, 2004: Akbulut-Yuksel, 2009).

We consider the effects of the Jewish expulsions at the smallest representative geographical units ("ROR" or "region") provided in GSOEP .[9] We obtain our historical dataset by digitalizing the city-level data on Jewish population and other historical variables from the 1930s German Municipalities Statistical Yearbooks. We then aggregate these variables according to the 1985 German regional (ROR) boundaries. This aggregation is possible since every city reported in the yearbooks belongs to only one region. We finally merge this aggregated ROR-level historical data with the 1985 wave of GSOEP by an individual's ROR. We choose this year because this is the earliest date for which both households' ROR information and individual and parental characteristics are available. Furthermore, the 1985 wave of GSOEP is only available for former West Germany.

Table 1 presents the descriptive statistics for the percentage of Jewish population and regional characteristics before the Nazi Regime. Table

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[9]These units are called *Raumordnungsregionen (RORs)* and are determined by the Federal Office for Building and Regional Planning (*Bundesamt fuer Bauwesen und Raumordnung, BBR*). West Germany has 75 different *Raumordnungsregionen* (see Figure 1 for detailed information on RORs). RORs are "spatial districts" based on economic interlinkages and commuting flows of areas. RORs encompass the aggregation of *Landkreise* and *kreisfreie Staedte* (administrative districts, which are analogous to counties in the US.) and represent the center of the local labor market and surrounding small towns and rural areas (Jaeger, et al., 2010).

1 shows that, on average, 1.2 percent of the population was Jewish in former West Germany in 1933.[10] However, there was a sizable variation across regions in the density of Jews; the Jewish percentage in regions with an above-average Jewish population was almost three times that in regions with a below-average Jewish population. For example, almost 4 percent of the population in Berlin and Frankfurt was Jewish. In contrast, the Jewish population constituted only 0.4 percent of the population in Bremen and Essen. Table 1 also highlights that regions with a higher Jewish population are larger in area and have a higher population and average income per capita in 1932. This underlines the fallacy of relying only on cross-regional variation in the Jewish population to identify the long-term educational effects of Jewish expulsions. Therefore the difference-in-differences strategy we propose uses within-region, cross-cohort variation to identify the effects of purges and controls for differences between birth cohorts that are common across German regions. There may be concern that observed differences in regional population and per capita income suggest possible differences in pre and post trends in children's outcomes. In Section 5, we assess whether there are differential trends by completing falsification tests and find no evidence for pre and post cohort-specific trends.

Table 2 shows the descriptive statistics for the outcomes and the main individual-level control variables we use in our estimation. One of the main outcomes of interest is years of schooling completed. The GSOEP asks respondents about their highest educational attainment; then in the data files generates the individual's years of schooling using these attainment categories.[11] While most of our regression analysis is with the years-of-schooling measure, we also present results using the highest educational attainment categories. These outcomes are mea-

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[10]The percentage of Jewish population is slightly higher in Table 1 than the aforementioned average of 0.8 because our measure of Jewish presence only includes former West Germany.

[11]We adjust the generated years of schooling variable in GSOEP for the cohorts who were affected from the compulsory schooling reform.

sured five decades after the persecution of Jewish population and reflect the outcomes of German-natives who lived to 1985 or later.

5   Estimation Results

Table 3 reports the results of estimating Equation (1) where the dependent variable is completed years of schooling. Each column is from a separate regression that controls for region and birth year fixed effects along with female and rural dummies. The difference-in-differences estimate,β, is reported in the first row. It is negative and significant at 99 percent level of confidence in every specification. Column (1) displays the difference-in-differences estimate for the entire population. Column (1) has an estimated β of -0.43 which suggests that school-aged children and young adults in a region with average *ex ante* Jewish population attain 0.5 fewer years of schooling. This is the difference-in-differences coefficient β (-0.43) multiplied by the average Jewish percentage in 1933 (1.19%) presented in Table 1. To gain a better understanding on the magnitude of β, we can also compare the educational attainment of school-aged children who were in Frankfurt (3.25 percent of its population was Jewish) to children who were in Bremen (0.4 percent of its population was Jewish) during the Nazi Regime. Using this comparison, Column (1) suggests that children in Frankfurt had 1.2 fewer years of schooling compared to children in Bremen as a result of the Jewish expulsions. It is likely that the loss of Jewish professionals had adverse effects on the quality of education in addition to the quantity of education. Therefore, the analysis presented in Table 3 provides a lower bound estimate for the potential negative consequences of the purge of Jewish population on children's long-term educational attainment.

Columns (2)-(4) of Table 3 present the results incorporating family background characteristics, such as father's and mother's educational attainment, which may also serve as a proxy for parents' economic sta-

tus. The first row in these columns reports the estimation results for children whose parents had a basic school degree (*Hauptschule*) or less. Results summarized in Columns (2)-(4) reveal that children with less educated parents had a greater reduction in their educational attainment (first row). On the other hand, the point estimates of the interaction terms suggest that the negative effect of expulsions is mitigated for children whose parents have more than basic education (second and third rows).[12] This differential effect may work literally through parental education (e.g., more educated parents value education more and so ensure their children are educated too even if negative shocks occur) or through other channels correlated with parental education such as family income or wealth (e.g., wealthy families can afford to educate their children and can hire private tutors or send children to boarding schools when necessary).

Table 3 shows that the reduction in the Jewish population decreases children's years of schooling by approximately 0.5 years. Does the effect in Table 3 come from a reduction in middle school, secondary school, college or graduate school completion? To assess at what level of education the adverse effect of expulsions is more profound, we estimate the following difference-in-difference specification using OLS:

$$Y_{irtm} = \alpha + \beta_m(JewishFr_r * AffectedCoh_{it}) + \delta_r + \gamma_t + \pi' \mathbf{X}_{irtm} + \epsilon_{irtm}$$

(2)

where the outcome of interest, Y_{irtm} is a dummy variable that indicates whether the individual i born in year t, in region r, completed m years of schooling or more. β_m, for m=9 to 18, is the estimated effect of the persecution of the Jews on the probability of completing each

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[12]As robustness, we estimate the same specifications as in Table 3 using the decline in Jewish population between 1933 and 1946 as a measure of loss of Jewish population. Results are presented in Appendix Table 3. We find quantitatively similar results with this alternative measure.

levels of education. The estimation results for difference-in-differences estimates using OLS are plotted in Figure 3 (the 95% intervals are also shown). Each point in Figure 3 is from a separate regression where the outcome is a dummy variable that takes a value of 1 if an individual completed m years of schooling or more. Figure 3 shows that the purge of Jewish professionals disrupts an individual's educational attainment in all levels of education; the point estimates are always negative. Figure 3 also reports that the affected cohorts in a region with an average Jewish population are 3 percentage points less likely to finish basic high school (9 years of schooling) and 5-6 percentage points less likely to complete technical high school or Gymnasium (12-13 years of schooling). Moreover, these cohorts are 4 percentage points less likely to have a college degree (16-17 years of schooling) and a graduate degree (18 years of schooling) because of the dismissal of Jewish professionals.

5.1 Robustness Checks and Potential Confounding Factors

Results presented in Table 3 and Figure 3 rest on the parallel trend assumption which assumes that the affected and the control cohorts would have had parallel trends in educational attainment in the absence of the Jewish persecutions. That is, the coefficient for interaction between dummy for being born 1910-1927 and regional *ex ante* Jewish population would be zero in the absence of the expulsions. To assess the validity of the identifying assumption, we perform two falsification tests in Table 4. First, we restrict the empirical analysis to the oldest cohorts who would have completed their schooling before the outset of the Nazi Regime to analyze whether RORs with varying *ex ante* Jewish population exhibit differential cohort specific trends before the expulsions. We code the oldest cohorts (i.e. those born between 1900 and 1904) as the "Placebo" affected cohort and the cohorts born between 1905 and 1909 as the "Placebo" control cohort, although there is obvi-

ously no true treatment here. If there are no differential trends, then the difference-in-differences estimates should be zero, which is indeed what we find (see Panel A of Table 4).

Second, in Panel B of Table 4, we focus on the oldest cohorts who completed their education before the Nazi Party came into power in 1933 and the youngest cohorts who began their human capital formation after the postwar reconstruction period was over in the late 1950s. In this control experiment, the oldest cohorts (i.e. those born between 1900 and 1909) are treated as the "Placebo" affected cohorts and the youngest cohorts who were born between 1951 and 1960 are treated as the "Placebo" control cohorts (this is our original control group) although there is no true treatment here either. As reported in Panel B, the difference-in-difference estimates in this specification are also statistically insignificant and close to zero. This finding supports the parallel trend assumption since it shows that differences in educational attainment between the oldest and the youngest cohorts are similar across regions. This suggests that the educational attainment of cohorts born between 1951 and 1960 (i.e. our control group) has not been affected by the persecution of the Jewish population. Thus, the results summarized in Table 4 lend credence to the identification assumption in Equation (1) and support the interpretation of the difference-in-difference estimates due to the exclusion of the Jewish population as opposed to some *ex ante* or *ex post* region-specific cohort trends.

Figure 2 presents an additional analysis on the validity of the parallel trend assumption. More specifically, in Figure 2, we present the educational achievement by cohort in regions with a high and low fraction of Jewish population in 1933. For the analysis presented in Figure 2, we group the birth cohorts into 5-year groups starting from 1900 in order to increase statistical precision. The vertical lines indicate our affected cohorts (i.e., born between 1910 and 1927). This figure also helps us to test whether our results are confounded by the differential postwar edu-

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cation policies. Germany, like many other countries, experienced a substantial increase in educational attainment after WWII. Hence the educational achievement was higher in urban areas before WWII; the rural areas had more scope to increase educational achievement. Jews were more likely to live in relatively more urban areas; therefore our analysis might overestimate the effects of persecutions if the postwar expansion in educational attainment is negatively correlated with the fraction of the Jewish population in 1933. Figure 2 shows that the expulsion of the Jewish population has negatively affected the educational attainment of the affected cohorts in regions with a high fraction of Jewish population. Figure 2 also illustrates that regions with varying *ex ante* Jewish population exhibit parallel trends in educational attainment before the expulsions and after WWII, which further supports our identifying assumption. In addition, Figure 2 shows that regions with a high or a low percentage of Jews experienced a similar increase in educational attainment in the postwar era; therefore our results are not confounded by the differential expansion in educational attainment across regions.

Another potential confounding factor for results summarized in Table 3 and Figure 3 is the probability of the nonrandom internal migration across regions. It is well documented, however, that Germany has historically low levels of geographic mobility in comparison to the U.S. and the U.K., and that mobility is particularly low during childhood and early adulthood (Pischke and von Wacher, 2005; Rainer and Siedler, 2005; Hochstadt, 1999). Therefore, internal migration should yield at most minor consequences for our estimates. Nonetheless, as robustness, we test whether individuals' migration decision is based on the regional Jewish population using the probability of moving as the dependent variable. Results are reported in Table 5, Panel A. Individuals are coded as movers if they report that they no longer reside in their childhood city or area in 1985. The affected and the control groups for this specification are the same as in Table 3. The difference-

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in-differences estimates for probability of moving are close to zero and statistically insignificant in every specification. This finding bolsters our confidence that individuals did not choose their final destination according to the fraction of the Jewish population of the region in 1933.

Panel B of Table 5 provides further evidence on the lack of systematic internal migration. In Panel B, the analysis is restricted to individuals who still live in the city or area where they grew up. We find that the difference-in-differences estimates for non-movers are very similar to the estimates for the entire population.[13] These results further support our previous findings and suggest that non-random internal migration is unlikely to be a concern for the results presented in Table 3 and Figure 3.

Another potential concern is the probability of differential mortality across regions with varying intensity of *ex ante* Jewish population. Panel C presents the mortality results for the affected cohorts. For this analysis, we take advantage of the panel structure of GSOEP, which enables us to analyze the mortality of the respondents between 1985 and 2011. The mortality variable refers to a dummy variable that takes a value of 1 if an individual has a recorded death year sometime between 1985 (the beginning of our sample) and 2011, and zero otherwise.[14] All the difference-in-differences estimates in Panel C are statistically insignificant and close to zero; therefore it is unlikely that our analysis is confounded by the differential mortality rates across regions.

Finally, analyses presented in Appendix Table 1 bolster our confidence that our results are robust to the choice of affected cohorts. In Appendix Table 1, we estimate Equation (1) where we define the affected cohorts using different birth year cut-offs. In Column (1), the affected cohorts encompass individuals born between 1910 and 1923.

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[13]the difference-in-differences estimates for the entire population and non-movers lie within each other's 95% confidence intervals

[14]Information on an individual's death year in GSOEP comes from official vitality records

This is the most conservative definition of the affected group; hence these cohorts were beyond compulsory schooling age on the onset of WWII so their schooling has not been affected by WWII destruction. In Column (2), we present the difference-in-differences estimates with the affected cohorts used in the main analysis in Table 3. In Column (3), we extend the affected cohorts to individuals born between 1910 and 1933. These cohorts were 6 and older on the onset of WWII in 1939; therefore their education might have been potentially affected from the persecution of Jewish population until 1939. The last column presents the difference-in-differences estimates, where individuals born between 1910 and 1938 constitute the affected group. This is the most inclusive definition of the affected group, where at least one year of the educational attainment of these cohorts had been potentially interrupted by the loss of Jewish population. Individuals born in 1910 may have been in the last year of their college education when the dismissals started in 1933. On the other hand, individuals born in 1938 were in the first year of the primary school just before the end of the Nazi Regime in 1945. Thus they are potentially the youngest cohorts that have been affected by the persecution of Jewish population in Nazi Germany. We find quantitatively similar results to the baseline specification in all columns in Appendix Table 1. This lends credence to our estimation results and suggests that our results are robust to a different categorization of the affected group.[15]

5.2 Potential Historical Confounding Events

During the time period we focus on in our paper, Germany also experienced other political and economic events, including World War II. Moreover, compulsory schooling laws changed in German states starting from the late 1940s. The nationwide effects of these events are cap-

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[15]We find similar results when we extend the college finishing age to 25 years of age.

tured by the birth year fixed effects in our analysis. We also use a lower level of geographical aggregation than state in our estimation, allowing us to account for potential state-specific policies. However, it is possible that regions with a higher Jewish population may have been differentially impacted by these events, which may raise potential concerns on the interpretation of our analyses. In this subsection, we formally test whether our results are confounded by these events; results are presented in Table 6. The first row in Table 6 reports the difference-in-differences estimate for the *ex ante* Jewish population. In all columns in Table 6, we also control for state-cohort trends.

One of the potential concerns is the state-specific policies, which might differentially affect the affected cohorts in regions with a higher fraction of Jewish population. The extent of such potential bias is largely mitigated because we use a lower level of geographical aggregation than state in our analysis to allow us to explore within-state variation. Moreover, we formally test whether our results are sensitive to the inclusion of state-cohort trends in Column (1) of Table 6. We find quantitatively similar difference-in-differences estimate in Column (1), which suggests that our results are not confounded by state-specific policies.

In addition, the compulsory schooling has increased from 8 to 9 years in Germany after the late 1940s. The timing of the reform was determined by state governments and varied at the state level. To address this potential concern, we first adjust the individuals' generated years of schooling variable reported in GSOEP. Following Pischke and von Wachter (2005) and Cygan-Rehm and Maeder (2013), we replace the years of schooling of individuals who completed Hauptschule before the introduction of the reform with 8 years. We use this adjusted years of schooling for all of our empirical analysis. Second, we control for the interaction between state dummies and birth year dummies that were affected by the compulsory schooling law along with linear state trends in Column (2). The difference-in-differences estimates re-

main statistically and quantitatively similar to the baseline specification in Column (2), contributing to our confidence that our results are robust to the compulsory schooling reform.

Another potential confounding factor is the differential exposure to WWII. The WWII destruction experienced by the region might have been associated with the Jewish population residing in the region during the Nazi Regime. To address whether school-aged children had been differentially affected by WWII destruction, we estimate the baseline specification controlling for the interaction between regional wartime destruction measure (rubble in m^3 per capita) and an indicator for being in the affected group. Column (3) shows that the difference-in-differences estimate for WWII destruction is very small and statistically insignificant, suggesting that there was no variation in exposure to wartime destruction across regions of varying *ex ante* Jewish population.

A related concern is refugees or people who fled to Germany from former parts of Germany and Soviet Zone/GDR after WWII.[16] These refugees might have settled in regions with higher *ex ante* Jewish population to fill the positions vacated from Jewish professionals. As an attempt to address this potential concern, we use the official 1961 regional-level refugee data provided by Redding and Sturm (2008). This is the earliest data we are able to access on refugees that serves as a good proxy for percentage of refugees residing in German regions by the end of WWII. We include this measure interacted with being in the affected cohort as a control into our baseline specification; results are summarized in Column (4). We find no evidence for differential impacts of refugees.

In Column (5), we consider the macroeconomic conditions German regions were in during early 1930s. The macroeconomic conditions and the employment opportunities in general might have had differential impacts in regions with a higher percentage of Jews, altering the in-

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[16]See Burchardi and Hassan (2013) for a discussion of the settlement of refugees after WWII in Germany.

centives for human capital investment among German children in these regions. Under unfavorable macroeconomic conditions, these children might have chosen to work in lower skill jobs instead of investing in higher education. Alternatively, adverse labor market conditions may have induced children to stay at school longer and acquire more education. As an attempt to address this potential concern, in Column (5), we use regional unemployment rate in 1932 as a proxy for the macroeconomic conditions in German regions. The difference-in-differences estimate for *ex ante* Jewish population remains virtually unchanged when we control for the interaction between unemployment rate in 1932 and an indicator for being in the affected group. Therefore, children's educational attainment was not differentially affected by differences in regional unemployment rate.

An additional concern is the differential Nazi Party(NSDAP) and the Communist Party(KPD) support across German regions. Regions with higher *ex ante* Jewish populations might have had a stronger support for Nazi Party and Anti-Semitic policies implemented during the Nazi Regime. Moreover, members of the Communist party were also expelled from civil service in Germany with the civil service law. Deichmann (2001) and Waldinger (2010) suggest that only a small fraction of the professionals were dismissed because of being "politically unreliable"; thus it is unlikely that our results are confounded by differential Communist Party support across German regions. Nevertheless, as robustness, we assembled region-level data on the percentage of votes the Nazi Party and the Communist Party of Germany received in the federal elections in 1932 to address this potential concern. Using election results from the first federal election of July 1932, we control for the interaction between the percentage of votes received by the Nazi Party and the Communist Party and being in the affected cohort. Estimation results summarized in Column (6) mimic our previous findings.[17]

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[17]We find quantitatively similar results when we use the election results from the second federal election that took place in November 1932.

Nazi policies and potentially other policies might have differentially affected individuals in larger and more prosperous regions. In addition, RORs may differ in terms of the urban population they encompass. In Column (7), we control for the interaction of the regional population and income per capita in 1932 with an indicator for being in the affected group.[18] Similarly, in Column (8), we include the interaction between the percentage of urban population in each region and being in the affected cohort to account for potential difference in urbanization across regions. The difference-in-differences estimates in Columns (7) and (8) reveal that there is no variation in children's educational attainment across regions of varying income per capita, population and urbanization.

In the last column of Table 6, we control for all of the potential confounding factors. The difference-in-differences estimate for the *ex ante* Jewish population is still economically and statistically significant in this specification. Therefore, analyses presented in Table 6 suggest that the dismissal of Jewish professionals has detrimental long-term effects on German children's educational attainment even after we account for state-specific policies, regional macroeconomic conditions, Nazi and Communist Party support, WWII, urbanization and the compulsory schooling reform.

5.3   Heterogeneity and Channels

In this subsection, we provide formal evidence on the heterogeneity and the potential mechanisms through which persecution of the Jewish population may have affected school-aged children. The results are reported in Table 7. Women constitute 56 percent of our affected cohorts; therefore it is of interest to test whether our results are similar across gender.

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[18]As an alternative, we also estimate a specification where we control for the interaction of 1932 regional population and income per capita with birth year dummies. We find quantitatively similar results.

The first and second columns present the difference-in-differences estimates for only female and male samples, respectively. Findings summarized in these columns show that the difference-in-differences estimate is larger in magnitude for the male sample; however it is not statistically different than the difference-in-differences estimate for the female sample. These gender-specific analyses also suggest that the main education results presented in Table 3 are not an artifact of the higher number of women in the 1985 GSOEP. Columns (3) and (4) consider whether the long-term effects of purges differ by an individual's urban status. We find that the adverse educational effect of persecutions is statistically similar to the difference-in-difference estimates for the rural population. Therefore, analyses summarized in columns (3) and (4) show that our results are not driven by the urban areas.

Additionally, one may expect the effects of the persecutions to be non-linear, e.g., if an *ex ante* fraction of Jews surpasses a certain level, then the detrimental effects become especially large, otherwise the effects are modest or negligible. To explore whether the negative effects of expulsions are more pronounced in regions with the highest *ex ante* Jewish population, we divide the Jewish population intensity measure into quartiles. The estimation results from this specification show that the adverse effects of the Holocaust are larger in regions that lost a higher fraction of their population due to the expulsions. Column (5) shows that children in the top quartile attain 0.7 fewer years schooling relative to the control group; this effect is twice as large as for the bottom quartile.

In Column (6), we replace the affected group dummy with a continuous measure of the number of school-age years an individual was potentially affected by the persecutions.[19] We find that children in a re-

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[19]To generate this variable, we assume that the school-starting age is 6 in Germany and individuals finish their university education at the age of 23, 5 years after the high school graduation. Since the Nazi Party was in power between 1933 and 1945, this new variable takes a value of 1 if the individual was born in 1910 or 1938, a value of 2 if an individual was born in 1911 or 1937, a value of 3 if an individual was born

gion with an average *ex ante* Jewish population attained 0.44 fewer years of schooling if they were potentially affected from the purge of the Jewish population during the entire duration of the Nazi Regime. This is the difference-in-differences coefficient β (-0.0316) multiplied by the average population-weighted Jewish population in 1933 (1.19%) in Table 1 and the duration of the Nazi Regime (12 years). This additional analysis shows that the estimation results presented in Table 3 also hold when the affected cohort dummy is replaced with a continuous measure of the number of school-age years a cohort was exposed to persecutions.[20]

Having shown that school-aged children have lower educational attainment due to the expulsion of Jewish population, it is of interest to explore the channels underlying this causal association. For example, the absence of school teachers and professors might have adversely affected the education decision of German children. Similarly, the declining teacher quality and bigger class sizes would have increased the number of early drop-outs. Moreover, the dismissal of Jews from all civil service jobs might have provided new employment opportunities, thereby changing the incentives for human capital investment among German children and young adults. Figure 4 illustrates the association between the percentage of dismissed professors and the Jewish population in each region.[21] Similarly, Figure 5 displays the relationship between the percentage of school teachers who were dismissed in 1933 and the Jewish population. These figures show that regions with higher *ex ante* Jewish population had a greater decline both in the number of

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in 1912 or 1936, a value of 4 if an individual was born in 1913 or 1935 and a value of 5 if an individual was born in 1914 or 1934, a value of 6 if an individual was born in 1915 or 1933. Similarly, this variable is 7 if an individual was born in 1916 or 1932, 8 if an individual was born in 1917 or 1931, 9 if an individual was born in 1918 or 1930, 10 if an individual was born in 1919 or 1929 and 11 if an individual was born in 1920 or 1928. Finally, this variable takes a value of 12 if an individual was born between 1921 and 1927 and zero otherwise.

[20]We find similar results when we extend college graduation age to 25

[21]For the number of dismissed science professors, we use figures from Waldinger (2011).

professors and teachers after the Nazi Party seized power in 1933. In addition, Columns (7) and (8) of Table 7, we formally test how the dismissal of Jewish professors and school teachers affected children's educational attainment. These analyses reveal that school-aged children in regions with a higher fraction of *ex ante* Jewish population received less schooling because schools and universities were defunct due the expulsion of Jewish professors and schoolteachers.

6   Conclusion

This paper provides first causal evidence on the long-term direct and spillover effects of the expulsion of the Jewish population. Using region-by-cohort variation in the Jewish population of Nazi Germany, we find that children who were school-aged during the persecutions completed fewer years of schooling and had a lower probability of finishing a technical and academic high school, or university. The reduction in educational attainment is borne disproportionately by girls and boys living in regions with the highest fraction of Jewish population and whose parents were less educated. We also find that school-aged children received less schooling because schools and universities are defunct due to the expulsion of Jewish professors and schoolteachers.

Findings in this paper may shed light on the potential long-term legacies of large-scale human capital loss that could be caused by the economic and the political turmoil experienced in many countries around the globe. Our findings show that regions that experienced a dramatic decline in the number of highly skilled professionals not only experience deterioration in macro-level outcomes as shown in the previous literature, but such large-scale human capital loss has more substantial and enduring consequences along human dimensions. Hence, the detrimental effects of the Jewish persecutions in Nazi Germany are still present five decades after the Holocaust. Our findings underline the im-

portance of policies primarily targeting school-aged children following these large-scale negative human capital shocks.

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Figure 1 Map of *Raumordnungsregionen* (RORs) in Former West Germany

Source: Source: Federal Office for Building and Regional Planning (*Bundesamt fuer Bauwesen und Raumordnung*, BBR). There are 75 spatial planning regions (RORs) in former West Germany.

Figure 2 Average years of schooling
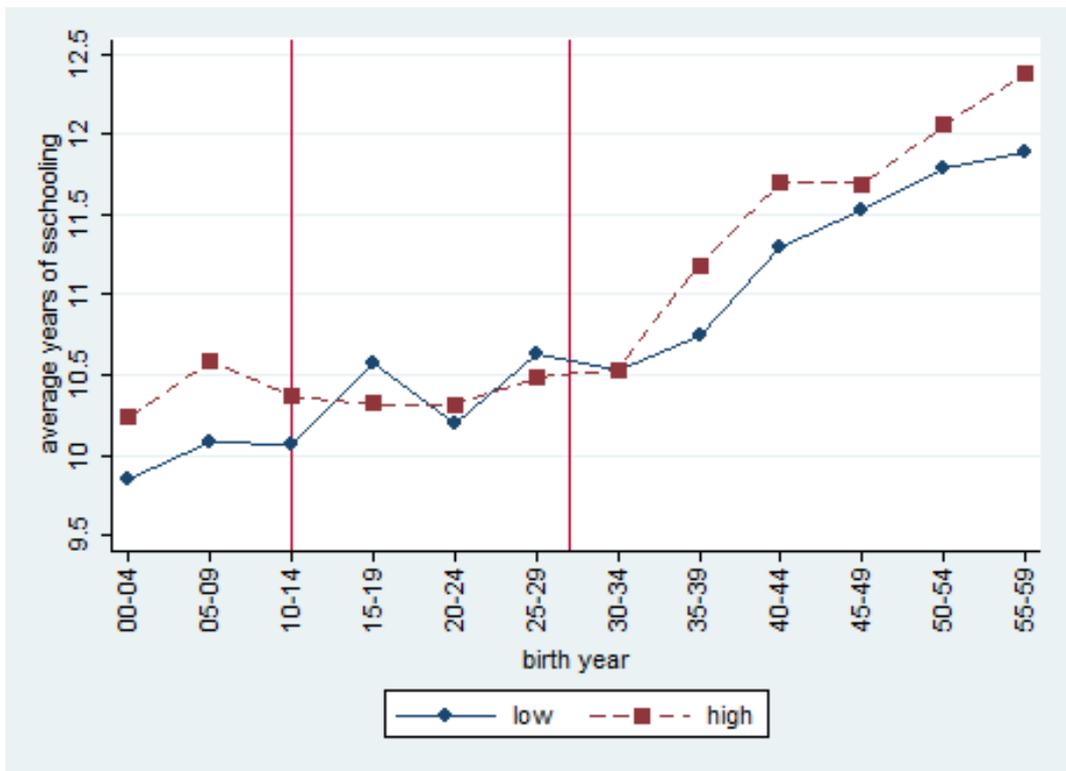
Notes: Own calculations.

Figure 3 Estimated Effect of Jewish Expulsions on Full Distribution of Education
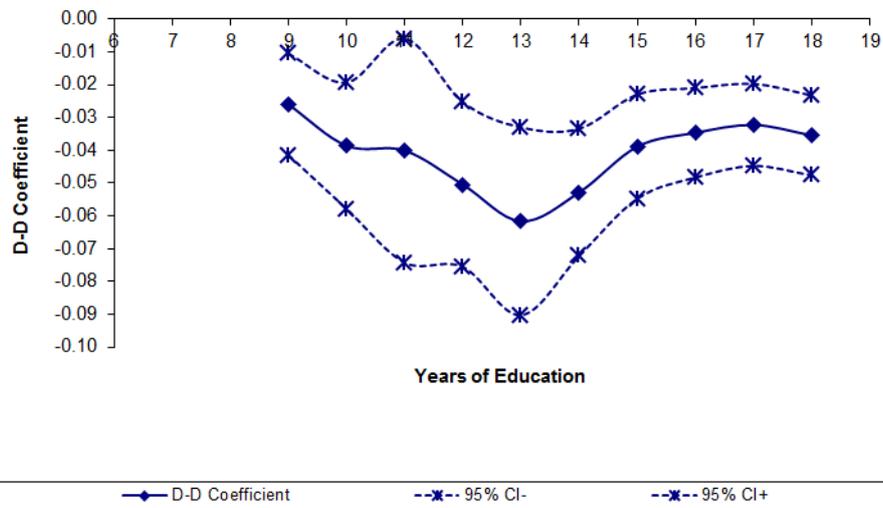
Notes: Each point in this figure is the difference-in-difference estimate from a separate regression where the outcome is a dummy variable that takes a value of 1 if an individual completed m years of schooling or more and zero otherwise.

Figure 4 Percentage of Dismissed Science Professors and the Jewish Population in German Cities
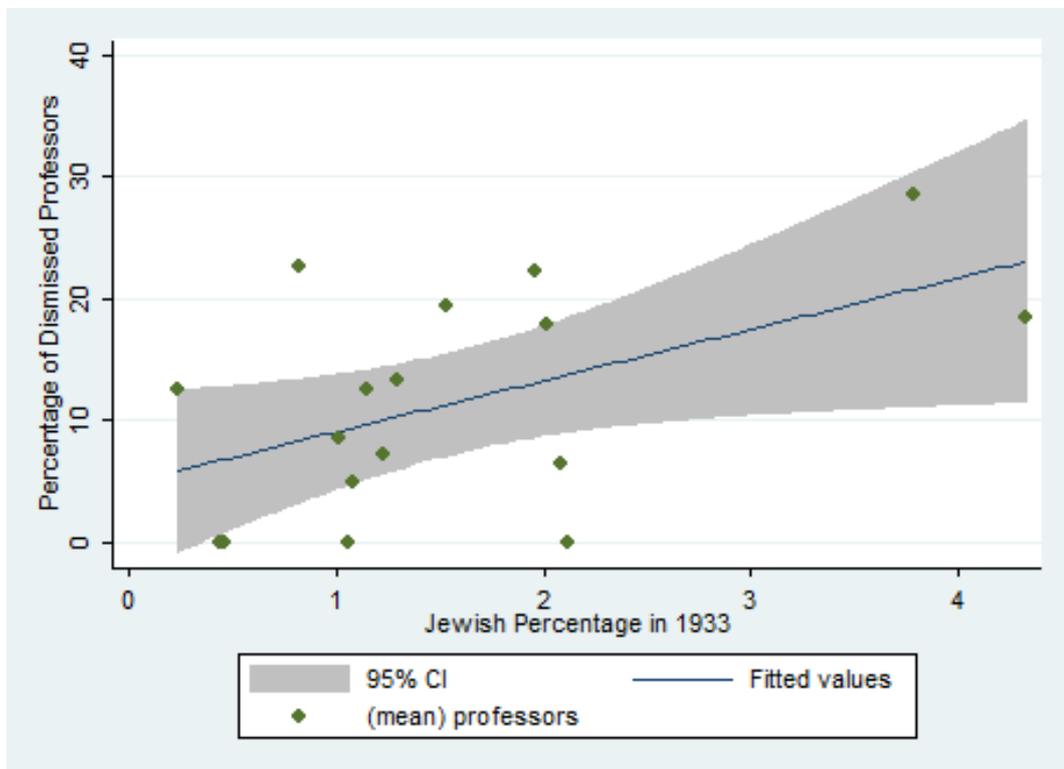
Waldinger (2012).

Figure 5 Percentage of Dismissed Teachers and the Jewish Population in German Cities
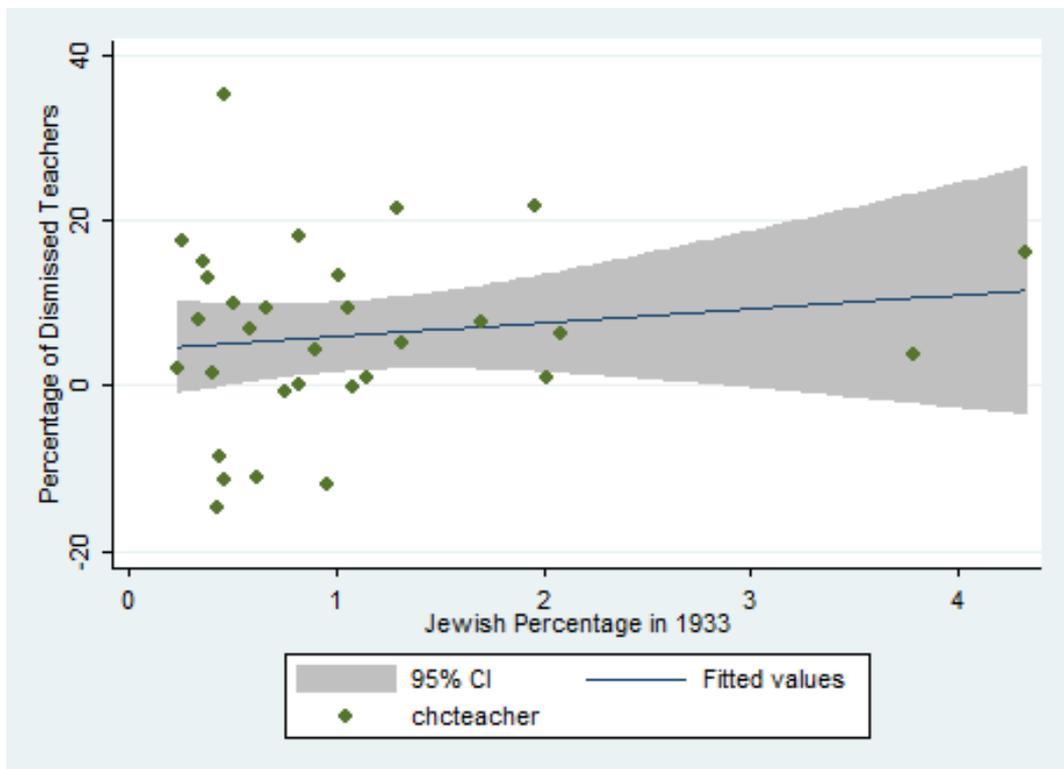
Source: 1930s German Municipalities Statistical Yearbook.

Table 1 Descriptive Statistics for Jewish Population in Germany

	Cities with below avg. Jewish Population	Cities with below avg. Jewish Population	All	difference s.e.(difference)
	(1)	(2)	(3)	(4)
Percentage of Jews in 1933	1.957	0.715	1.189	1.242***
	(0.961)	(0.280)	(0.875)	(0.027)
Percentage of Jews in 1946	0.289	0.0727	0.155	0.217***
	(0.379)	(0.148)	(0.282)	(0.011)
Percentage of Dismissed Professors	14.45	6.512	10.74	7.935***
	(6.161)	(7.796)	(8.017)	(0.409)
Percentage of Dismissed Teachers	14.85	3.869	6.958	10.98***
	(7.428)	(10.43)	(10.86)	(0.520)
Area in km2 in 1933	317.8	231.4	264.4	86.35***
	(252.3)	(173.7)	(211.4)	(8.696)
Population in 1933	519,058.3	341,805.0	409460.7	177,253.3***
	(342606.3)	(350043.2)	(357676.1)	(14,579.030)
Income per Capita in 1932 (in RM)	504.1	456.3	474.8	47.85***
	(70.60)	(116.2)	(103.7)	(4.262)
N max.	913	1,479	2,392	2,392

Notes: The means presented are weighted by population in the 1985 GSOEP. Standard deviations are in parentheses.

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Table 2 Descriptive Statistics, GSOEP Data

	RORs with below avg. Jewish Population	RORs with below avg. Jewish Population	All	difference s.e.(difference)
	(1)	(2)	(3)	(4)
Years of Schooling	11.27	11.08	11.15	0.185
	(2.582)	(2.445)	(2.499)	(0.105)
Has Basic Education	0.594	0.588	' 0.590	0.007
(Hauptschule)	(0.491)	(0.492)	(0.492)	(0.021)
Has More than Basic	0.162	0.159	0.160	0.003
Education	(0.369)	(0.366)	(0.367)	(0.015)
Mother with Basic Education	0.873	0.892	0.885	-0.019
	(0.333)	(0.310)	(0.319)	(0.014)
Father with Basic Education	0.815	0.834	0.827	-0.018
	(0.388)	(0.373)	(0.379)	(0.017)
Age in 1985	47.30	47.29	47.29	0.0101
	(18.35)	(18.54)	(18.46)	(0.776)
Female	0.535	0.538	0.537	-0.005
	(0.499)	(0.499)	(0.499)	(0.021)
Rural	0.410	0.391	0.398	0.0194
	(0.492)	(0.488)	(0.490)	(0.021)
N max	913	1,479	2,392	2,392

Notes: Data are from the 1985 GSOEP. The sample consists of individuals born between 1910 and 1960. Individuals born between 1928 and 1950 are dropped from the analysis since they were exposed to WWII destruction. Standard deviations are presented in parentheses.

Table 3 Effects of Jewish Expulsions on Years of Schooling

	(1)	(2)	(3)	(4)
% of Jews in 1933 X Born btw.1910-1927	-0.4292***	-0.3460***	-0.3241***	-0.3125***
	(0.0742)	(0.0748)	(0.0725)	(0.0698)
% of Jews in 1933 X Born btw.1910-1927 X Mother has more than Basic Education		0.1790		-0.3945
		(0.2628)		(0.3914)
% of Jews in 1933 X Born btw.1910-1927 X Father has more than Basic Education			0.5379*	0.7227**
			(0.2678)	(0.3264)
R^2	0.216	0.304	0.331	0.352
N	2,385	2,061	2,037	2,006

Notes: Standard errors clustered by regions are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01). The control group is individuals born between 1951 and 1960. Each column is from a separate regression where the main treatment effect varies by parental education in Columns (2)-(4). Each column controls for region and year of birth fixed effects. Columns (2)-(4) also control for the main effects of parental human capital. Other controls in each regression are gender and rural dummies.

Table 4 Falsification Tests

	(1)	(2)	(3)	(4)
Panel A: Only Old Cohorts				
% of Jews in 1933 X Born btw.1900-1904	-0.0018	-0.0725	-0.0088	-0.1006
	(0.2483)	(0.3497)	(0.2760)	(0.3097)
% of Jews in 1933 X Born btw.1900-1904 X Mother has more than Basic Education		-0.1778		0.6041
		(0.3332)		(0.6144)
% of Jews in 1933 X Born btw.1900-1904 X Father has more than Basic Education			-0.4115	-0.9271
			(0.3190)	(0.6017)
R^2	0.293	0.406	0.446	0.447
N	302	229	229	228
Panel B: Old and Young Cohorts				
% of Jews in 1933 X Born btw.1900-1910	-0.1059	0.0021	-0.0317	-0.0233
	(0.1018)	(0.0984)	(0.0936)	(0.0948)
% of Jews in 1933 X Born btw.1900-1910 X Mother has more than Basic Education		-0.6302		-0.9871***
		(0.3831)		(0.3418)
% of Jews in 1933 X Born btw.1900-1910 X Father has more than Basic Education			0.4386	0.7129*
			(0.4224)	(0.3713)
R^2	0.189	0.278	0.294	0.318
N	1,479	1,276	1,257	1,243

Notes: Standard errors clustered by regions are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01). In Panel A, sample consists of older cohorts who were born between 1900 and 1909 and who would have completed their schooling before 1933. In Panel A, "Placebo" affected group is individuals born between 1900 and 1904 and "Placebo" control group is individuals born between 1905 and 1909. In Panel B, "Placebo" affected group is individuals born between 1900 and 1909 and "Placebo" control group is those born between 1951 and 1960. Each column is from a separate regression where the main treatment effect varies by parental education in Columns (2)-(4). Each column controls for region and year of birth fixed effects. Columns (2)-(4) also control for the main effects of parental education. Other controls in each regression are gender and rural dummies.

Table 5 Robustness Checks

	(1)	(2)	(3)	(4)
Panel A: Probability of Moving				
% of Jews in 1933 X Born btw.1910-1927	-0.0240	-0.0186	-0.0117	-0.0100
	(0.0183)	(0.0212)	(0.0222)	(0.0214)
% of Jews in 1933 X Born btw.1910-1927 X Mother has more than Basic Education		0.0061		0.0016
		(0.0561)		(0.0693)
% of Jews in 1933 X Born btw.1910-1927 X Father has more than Basic Education			-0.0218	-0.0192
			(0.0507)	(0.0599)
R^2	0.111	0.132	0.136	0.138
N	2,379	2,056	2,033	2,002
Panel B: Non-movers Only				
% of Jews in 1933 X Born btw.1910-1927	-0.3853***	-0.2587**	-0.3667***	-0.2887***
	(0.1396)	(0.1078)	(0.0751)	(0.0717)
% of Jews in 1933 X Born btw.1910-1927 X Mother has more than Basic Education		0.0318		-0.5600
		(0.6352)		(0.6974)
% of Jews in 1933 X Born btw.1910-1927 X Father has more than Basic Education			0.5996	0.7664*
			(0.3686)	(0.4141)
R^2	0.226	0.304	0.319	0.345
N	1,303	1,120	1,108	1,094
Panel C: Mortality				
% of Jews in 1933 X Born btw.1910-1927	-0.0044	0.0028	0.0082	0.0070
	(0.0095)	(0.0093)	(0.0095)	(0.0092)
% of Jews in 1933 X Born btw.1910-1927 X Mother has more than Basic Education		-0.0787*		-0.0011
		(0.0397)		(0.0564)
% of Jews in 1933 X Born btw.1910-1927 X Father has more than Basic Education			-0.1104***	-0.1080**
			(0.0309)	(0.0442)
R^2	0.521	0.533	0.528	0.531
N	2,385	2,061	2,037	2,006

Notes: Standard errors clustered by regions are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01). The control group is individuals born between 1951 and 1960. In Panel C, the mortality measure is a dummy variable that takes a value of 1 if individual has recorded death year from 1985 until 2011 and zero otherwise. Each column is from a separate regression where the main treatment effect varies by parental education in Columns (2)-(4). Each column controls for region and year of birth fixed effects. Columns (2)-(4) also control for the main effects of parental human capital. Other controls in each regression are gender and rural dummies. Individuals are coded as movers coded as movers if they report that they no longer reside in their childhood city or area.

Table 6 Historical Controls

	State Trends	Compulsory Schooling	WWII Destruction	Refugees	Unemployment Rate	NSDA and KPD Party Support	Population Size and Income pc.	Urban Share	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
% of Jews in 1933 X Born btw.1910-1927	-0.4364*** (0.0701)	-0.2696** (0.1203)	-0.4211*** (0.0683)	-0.3757*** (0.0897)	-0.4349*** (0.0701)	-0.4478*** (0.0678)	-0.4450*** (0.0755)	-0.4563*** (0.0666)	-0.3824*** (0.1178)
Rubble per Cap X Born btw.1910-1927			-0.0112 (0.0153)						-0.0124 (0.0177)
% of Refugees X Born btw.1910-1927				2.2108 (1.8057)					1.6070 (2.2421)
Unemployment Rate in 1932 X Born btw.1910-1927					0.0351 (0.0464)				0.0732 (0.0786)
% of Votes to Nazi Party X Born btw.1910-1927						0.0118 (0.0150)			-0.0014 (0.0213)
% of Votes to Communist Party X Born btw.1910-1927						-0.0145 (0.0164)			-0.0300 (0.0288)
Population Size in 1933 X Born btw.1910-1927							-0.0000 (0.0002)		-0.0001 (0.0002)
Income per Capita in 1932 X Born btw.1910-1927							0.0001 (0.0015)		0.0006 (0.0016)
Urban Share X Born btw.1910-1927								-0.5007 (0.5679)	-0.1437 (0.9193)
R^2	0.220	0.223	0.221	0.221	0.221	0.221	0.220	0.221	0.222
N	2,385	2,385	2,356	2,385	2,385	2,385	2,356	2,385	2,356

Notes: Standard errors clustered by regions are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01). The control group is individuals born between 1951 and 1960. Each column controls for region and year of birth fixed effects. Other controls in each regression are gender and rural dummies and state-cohort trends.

Table 7 Heterogeneity in the Effects of Jewish Expulsion on Years of Schooling

	Female Only	Male Only	Urban Only	Rural Only	Top Quartile Jewish population	Number of Years Affected	Dismissed Professor(%)	Dismissed Teachers (%)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
% of Jews in 1933 X Born btw.1910-1927	-0.3548*** (0.1130)	-0.5188*** (0.1386)	-0.4527*** (0.1049)	-0.3882*** (0.1098)	-0.6752* (0.3575)			
% of Jews in 1933 X Number of Years effected						-0.0316*** (0.0056)		
% of Dismissed Professors X Born btw.1910-1927							-0.0457** (0.0179)	
% of Dismissed Teachers X Born btw.1910-1927								-0.0265** (0.0122)
R^2	0.275	0.135	0.184	0.305	0.215	0.203	0.220	0.213
N	1,277	1,108	1,435	950	2,385	3,509	1,158	1,713

Notes: Standard errors clustered by regions are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01). The control group is individuals born between 1951 and 1960. In Column (5), "Top Quartile Destruction" is a dummy variable which takes a value of 1 for interaction of being in regions with highest Jewish fraction during the Nazi Regime and being born between 1910 and 1927. Each column controls for region and year of birth fixed effects. Other controls are gender and rural dummies.

Table A-1 Effects of Jewish Expulsion on Years of Schooling by Different Cohorts

	Born between 1910-1923	Born between 1910-1927	Born between 1910-1933	Born between 1910-1938
	(1)	(2)	(3)	(4)
% of Jews in 1933 X Cohort Dummy	-0.4088***	-0.3824***	-0.3621***	-0.3098***
	(0.1405)	(0.1178)	(0.1098)	(0.1138)
Rubble per Capita X Cohort Dummy	-0.0026	-0.0124	-0.0106	-0.0119
	(0.0179)	(0.0177)	(0.0158)	(0.0152)
% of Refugees X Cohort Dummy	0.8991	1.6070	2.0190	1.3938
	(2.4377)	(2.2421)	(2.0279)	(2.0072)
Unemployment Rate in 1932 X Cohort Dummy	0.0338	0.0732	0.0454	0.0482
	(0.0822)	(0.0786)	(0.0692)	(0.0717)
% of Votes to Nazi Party X Cohort Dummy	0.0123	-0.0014	0.0085	0.0158
	(0.0249)	(0.0213)	(0.0193)	(0.0189)
% of Votes to Communist Party X Cohort Dummy	-0.0202	-0.0300	-0.0023	0.0121
	(0.0338)	(0.0288)	(0.0252)	(0.0238)
Population Size in 1933 X Cohort Dummy	0.0001	-0.0001	-0.0002	-0.0002
	(0.0002)	(0.0002)	(0.0001)	(0.0001)
Income per Capita in 1932 X Cohort Dummy	-0.0002	0.0006	0.0010	0.0009
	(0.0014)	(0.0016)	(0.0013)	(0.0013)
Urban Share X Cohort Dummy	-0.3731	-0.1437	-0.5234	-1.0831
	(0.8200)	(0.9193)	(0.9734)	(0.8436)
R^2	0.223	0.222	0.221	0.208
N	1,991	2,356	2,911	3,467

Notes: Standard errors clustered by regions are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01). The control group is individuals born between 1951 and 1960. Each column controls for region and year of birth fixed effects. Other controls in each regression are gender and rural dummies and and state-cohort trends.

Table A-2 Effects of Jewish Expulsions on Years of Schooling

	(1)	(2)	(3)	(4)
% of Jews in 1933 X Born btw.1910-1927	-0.3129**	-0.2916**	-0.2853**	-0.2942**
	(0.1238)	(0.1174)	(0.1102)	(0.1136)
% of Jews in 1933 X Born btw.1910-1927		0.0635		-0.3918
X Mother has more than Basic Education		(0.3244)		(0.5126)
% of Jews in 1933 X Born btw.1910-1927			0.4390	0.6461
X Father has more than Basic Education			(0.3542)	(0.4694)
R^2	0.226	0.309	0.358	0.365
N	1,480	1,221	1,214	1,197

Notes: Standard errors clustered by regions are shown in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01). The control group is individuals born between 1900 and 1909. Each column is from a separate regression where the main treatment effect varies by parental education in Columns (2)-(4). Each column controls for region and year of birth fixed effects. Columns (2)-(4) also control for the main effects of parental human capital. Other controls in each regression are gender and rural dummies.

Table A-3 Effects of Jewish Expulsions on Years of Schooling using the Change in the Jewish Population between 1946 and 1933

	(1)	(2)	(3)	(4)
Decline in Jewish population X Born btw.1910-1927	-0.4151***	-0.3428***	-0.3100***	-0.3105***
	(0.0821)	(0.0784)	(0.0721)	(0.0669)
Decline in Jewish population X Born btw.1910-1927 X Mother has more than Basic Education		0.3148		-0.2514
		(0.2742)		(0.4415)
Decline in Jewish population X Born btw.1910-1927 X Father has more than Basic Education			0.6647**	0.7785**
			(0.2904)	(0.3568)
R^2	0.216	0.305	0.332	0.352
N	2,385	2,061	2,037	2,006

Notes: Standard errors clustered by regions are shown in parentheses. Asterisks denote significance levels (*=.10,**=.05, ***=.01). The control group is individuals born between 1951 and 1960. Each column is from a separate regression where the main treatment effect varies by parental education in Columns (2)-(4). Each column controls for region and year of birth fixed effects. Columns (2)-(4) also control for the main effects of parental human capital. Other controls in each regression are gender and rural dummies.