Antisemitism Affects Households' Investments

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Abstract

We propose historical anti-Jewish sentiment as a proxy for distrust in financial markets. Households in German counties where Jews were persecuted the most as far back as in the Middle Ages are less likely to invest in stocks today. A one-standard-deviation increase in historical anti-Jewish violence leads to a 7.5% to 12% drop in the average stock market participation. For identification, we exploit the forced migrations of Ashkenazi Jews out of the Rhine Valley after the 11^{th} century. The distance of a county from the Rhine Valley serves as an instrument for the existence of a Jewish community during the Black Death (1349) and hence the early emergence of anti-Jewish sentiment. Results are similar when we use the votes for the Nazi party as a proxy for anti-Jewish sentiment. The magnitude of the effect neither changes from 1984 until 2011 nor across cohorts.

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1 Introduction

For centuries, Jews have been associated with financial services. This perception dates back at least to 1049,[1] when Pope Leo IX banned Christians from lending money at interest.[2] In 1882, 3% of the total workforce in Germany was Jewish, while Jews accounted for 23% of the workforce in the financial sector.[3] Strikingly, the association of Jews with financial markets is still vivid today: in 2012, 25% of Germans agreed with the claim that "Jews have too much power in international financial markets."[4] At the same time, Jews have been treated as alien and enemies during several economic crises and pandemics, such as the Black Death of 1349. Pogroms and violence against Jews have been perpetrated across Europe since the Middle Ages, and they were tragically reflected in the *Final Solution* during the Nazi period.

In this paper, we exploit the geographic heterogeneity of historical violence against Jews across German counties (Kreise) to proxy for households' distrust in financial markets, which is a channel for stock market participation theorized by Gennaioli, Shleifer, and Vishny (2013) and Gennaioli, Shleifer, and Vishny (forthcoming). Several measures of Jewish persecution are negatively correlated with current stockholdings. Households in counties with anti-Jewish pogroms during the Black Death of 1349, and in which the number and/or the ratio of Jewish inhabitants deported under the Nazi regime was higher are less likely to invest in stocks today. A one-standard-deviation increase in the persecution of Jews under the Nazi regime is associated with a 1.2-percentage-point lower stock market participation today, which corresponds to 7.5% of the average participation in our sample. Moreover, households in counties that experienced at least one pogrom in

[1] Botticini and Eckstein (2005) and Botticini and Eckstein (2012) document that the human capital accumulated by Jews since the second century determined their voluntary selection into trade and finance occupations well before 1049.

[2] The legal basis for the ban was found in the Gospel of Luke (ch. 6, 34-35). The ban was formalized in the *Corpus Iuris Canonici* of 1150 by Gratianus. He revised the interpretation of usury proposed by Charles the Great in 806, from charging excessive interest rates to requiring any remuneration for lending money or objects: *Usura est, ubi amplius requiritur quam datur: [...] Ecce evidenter ostenditur, quod quicquid ultra sortem exigitur usura est.* – Usury is when one requires more than what he gives: [...] It is therefore obvious that whatever is required in addition shall be usury.

[3] For a breakdown of the German workforce in 1882 by sectors and religious denominations, please refer to Table 1.

[4] See League (2012).

1349 are 2 percentage points less likely to hold stocks today, which is 12% of the average participation.

Providing evidence of a causal effect of historical anti-Jewish violence on current stockholdings is a major challenge because we observe these outcomes several centuries apart.[5] We propose an identification strategy that exploits the forced historical migrations of the first Jewish communities across the German lands. As far back as the ninth century, Ashkenazi Jew communities settled along the Rhine and Mosel valleys, in an area that is still known in Hebrew as Ashkenaz. Archaeologists have found only sparse evidence of contemporaneous Jewish communities in other parts of current Germany. The onset of the Crusades and several European pandemics often led to Jews blamed and forced to leave their hometowns. Over time, Ashkenazi communities have spread widely from the Rhine Valley throughout the German lands and Eastern Europe. Due to transportation costs, this diffusion has taken several centuries (see Figure 2). We exploit the timing of these migrations for a three-stage identification strategy. In the first stage, we use the distance of a German county from the Rhine valley to predict the probability that a Jewish community existed before the Black Death (1349). In the second stage, we use the estimated probability of existence of a Jewish community to predict the extent of historical violence against Jews in each county. The rationale is that in counties with no Jewish communities, the probability of observing acts of violence against Jews during the Black Death must be zero, because no targets for such violence existed. By contrast, wherever a community existed, the probability of violence against Jews due to their mere presence must have been positive.[6] In the third stage, we use the estimated historical persecution against Jews in a county to predict current stockholdings by households in that county. Our three-stage least squares estimates confirm the negative effect of historical anti-Jewish

[5] A direct test for the effect of distrust in the financial sector on stock market participation would exploit a shock to distrust. A stock market crash or a banking sector crisis might represent such a shock. Unfortunately, these shocks also affect other determinants of the decision to invest in stocks (e.g., Guiso et al. (2013)).

[6] This claim does not imply that ex post we will necessarily observe a pogrom in counties where a Jewish community existed.

sentiment on current stockholdings.[7]

The reduced-form and IV results survive a series of robustness and placebo tests, such as excluding the counties where education or GDP per capita were low, or where unemployment was high immediately before the seizure of power by the Nazis in 1933. To corroborate our instrument, we show that placebo distances of a county from towns in the north or south of Germany are not associated with Jewish persecution or current stockholdings.[8]

Ideally, we would like to directly test the hypothesis that anti-Jewish sentiment is correlated with distrust in the financial sector. Unfortunately, however, we cannot measure the level of trust in the financial sector for the households in our sample. Hence we propose a set of placebo analyses to substantiate our argument. First, we consider a supply-side interpretation of our findings. The deportation of Jews may have drastically reduced the human capital needed to run financial institutions and thus reduced the number of financial intermediaries. The lower supply of financial services may determine the observed lower stock market participation. We do not find any effect of historical Jewish persecution on the total number of financial institution branches per capita at the county level, or when we split institutions based on their scope, such as private banks, banks owned by the local governments (*Landesbanken* and *Sparkassen*), or community banks (*Raiffeisenbanken*). Second, we study the effects of historical anti-Jewish violence on the likelihood that households invest in opportunities that are illiquid, such as stocks, but that do not necessarily require any interactions with the financial sector, such as real estate and life insurance products. Third, we look at outcomes that relate to social conservativeness and limited cognitive abilities.[9] We fail to detect economically or statistically significant associations of historical anti-Jewish violence on any of these

[7]To support the exclusion restriction we assume for the validity of the instrument we show that the distance from the Rhine Valley does not predict current outcomes that affect stock market participation, and it has no autonomous association with stock market participation once we add it to the reduced-form analysis together with the measures of historical anti-Jewish sentiment.

[8]In section 5, we propose a series of results to support the exclusion restriction that validates our instrument.

[9]These outcomes include the likelihood that a woman is declared to be in charge of financial decisions within the household, and the likelihood that the head of the household holds a high school degree or higher level of education.

alternative outcomes.

We then analyze the properties of the effect of anti-Jewish violence on households' investment decisions. First, we investigate whether the effect fades away over time. Voigtlaender and Voth (2012) show that anti-Jewish sentiment at the local level has persisted for six centuries. Consistently, the effect of historical anti-Jewish sentiment on the likelihood of holding stocks is stable from 1984 to 2011. Second, we investigate whether the effect differs across cohorts, and find that the effect is similar for all cohorts except those born before 1945 who experienced the economic crisis of the 1930s, the Nazi period, and the post-war denazification process. This result may be driven by the massive antisemitic propaganda of the Third Reich, which may have eliminated the variation of antisemitic beliefs across German counties.[10] At the same time, the experience of a major economic crisis may drive the non-effect (Malmendier and Nagel, 2011). To disentangle these two explanations, we interact a cohort effect with the unemployment rates in counties as of 1933. Results are consistent with the second argument: the non-effect on the cohort born before 1945 is driven by those who live in regions that experienced the highest unemployment during the 1930s.

To better understand the relationship between anti-Jewish sentiment, economic experiences, and current stockholdings, we exploit the geographic heterogeneity of the electoral support for the Nazi Party before the beginning of the Third Reich as an alternative source of variation for the historical anti-Jewish sentiment at the county level. Antisemitism was a major ingredient of the Nazi ideology. But the support for the Nazis was also due to Germans' discontent with their economic situation (de Bromhead, Eichengreen, and O'Rourke (2013)). Hence, the vote share of the Nazi party should be a good proxy for local antisemitic beliefs in counties that were less affected by the economic crisis, whereas it should be a noisier proxy in counties that were affected the

[10]Reconciling this explanation with the effect of the cross-sectional variation in anti-Jewish sentiment on the stockholding of posterior cohorts would be difficult. Moreover, Voigtlaender and Voth (2013) find that Germans who were young during the Nazi period are on average more antisemitic than other Germans today, but they still observe cross-sectional variation in antisemitism within that cohort.

most. Consistently, we find an economically and statistically significant negative effect of votes for the Nazi party in 1933 on current stockholdings in counties with low levels of unemployment in 1933, while there is no effect in counties where unemployment was high.

Finally, we consider two alternative channels through which deep-rooted anti-Jewish sentiment may affect current stock investments. On the one hand, all current households may consciously associate financial markets with the Jewish population. Those who have obtained a higher anti-Jewish sentiment from their ancestors would therefore invest less in stocks than others. On the other hand, a strict connection between the Jewish population and financial services may have been vivid in the past, and it may have promoted two autonomous norms that transmitted across generations: anti-Jewish sentiment, and distrust toward financial markets. While the direct experience of local financial services, such as banks, has reduced the distrust over time, distrust has not faded away for those financial institutions of which households have no direct experience, such as stock markets. Hence, areas where Jews were more disliked in the past would be areas where current households distrust stock markets more, although the association between these two dimensions might not be conscious nowadays. We do find evidence consistent with the latter channel: the likelihood that households invest in stocks is not associated with the ratio of Jews working in the financial sector in a county as of 1882, or with the ratio of Jewish population in a county as of 1933.

To the best of our knowledge, this paper is one of the few that identifies and quantifies a long-run effect of deep-rooted discriminatory beliefs, and specifically anti-Jewish sentiment, on the economic decisions and wealth of the discriminators themselves.[11] Thus, these results may suggest an economic-based paradigm to justify policies against discrimination and hatred beliefs. Starting with Aristotle, research and public debate have condemned hatred beliefs and justified policies against them based on the promotion of

[11]Taste, statistical, and implied discrimination in settings such as the labor market (Bertrand and Mullainathan (2004) and banking relationships have negative effects on the discriminators whenever they choose a second-best employee or counterparty to avoid those they discriminate against. Our results show that the discriminators give up a wealth-increasing opportunity that would not necessarily be enhanced by the discriminated population, but for instance, by non-Jewish borkers.

individual human rights. But several non-Western cultures and institutions do not share the ethical notion of the primacy of individual instances over the collective ones, which hinders the promotion of human rights. To the contrary, promoting broader economic wealth is a universally-accepted aim of collective institutions. Therefore, even cultures that do not recognize the ultimate primacy of the individual over the collective could be persuaded by evidence that hatred beliefs reduce not only the economic wealth of the discriminated, but also the wealth of the discriminators, hence ultimately of the whole society.

2 Related Literature

This paper contributes to the literature on the effects of culture and social stereotypes on economic and financial outcomes. Banfield (1958) and Putnam (1993) argue that the social capital a population accumulates over time affects its long-run social and economic development. Tabellini (2010) finds that culture affects regional economic development in Europe, whereas Algan and Cahuc (2006) show that the inherited component of trust by second-generation immigrants affects current economic growth in the United States. Guiso et al. (2004) exploit the cross-sectional variation of social capital across Italian provinces to show that social capital positively affects stock market participation, illiquid investments, and the use of checks as a household's means of payment. Inspired by the seminal work of Akerlof and Kranton (2000), a nascent strand of the finance literature focuses on the effects of identity and social stereotypes on financial decision making: Han and Hirshleifer (2013) model the effect of visibility bias on consumption and saving decisions, whereas D'Acunto (2013a) studies the effect of gender identity stereotypes on individual financial decisions. We contribute to these fields by proposing a proxy for households' distrust in financial markets based on the sterotypical association of Jews with financial services, and by testing for its causal effect on investment decisions. We argue that deep-rooted anti-Jewish sentiment has produced distrust in financial markets, which has transmitted across generations and persisted over time.

This paper also relates to the limited stock market participation puzzle. Paxson (1990)

and Guiso et al. (1996) argue theoretically and empirically that households' background risk may explain their limited participation in stock markets. Other determinants include social interactions (Hong et al., 2004), awareness of investment opportunities (Guiso and Jappelli, 2005), generalized trust (Guiso et al., 2009), financial literacy (van Rooij et al., 2011), macroeconomic experiences (Malmendier and Nagel, 2011), and labor income risk (Betermier et al. (2012), Berk and Walden (2013)). In this paper, we show that deep-rooted and persistent anti-Jewish sentiment reduces the likelihood that households invest in stocks.

Moreover, our paper belongs to the recent literature that uses historical natural experiments to explain current economic outcomes recently surveyed by Spolaore and Wacziarg (2013) and Nunn (forthcoming). For the case of financial decisions, Chitu, Eichengreen, and Mehl (2013) document that the foreign bond holdings by US investors in the early 20^{th} century predict current holdings, whereas D'Acunto (2013b) documents that historical literacy predicts the current education level of the blue-collar workforce in European regions, and studies its effect on current corporate policies. This strand of literature differs from the field Economic History insofar as the former *directly* explains current-day economic and financial decisions using natural experiments from the past and persistent social phenomena, whereas the latter studies the effect of past shocks and institutions on past outcomes. To stress this difference, an appropriate label for this nascent field would be "History & Economics", and for financial outcomes "History & Finance."

Finally, our paper adds to the recent literature on the evolution of anti-Jewish sentiment and on the consequences of Jewish persecution on economic outcomes. In their seminal work, Voigtlaender and Voth (2012) show that the variation in anti-Jewish violence across German towns in the Middle Ages predicts the intensity of Jewish persecution during the Nazi period. In the reduced-form analysis, we compute the authors' measures of Jewish persecution in the Middle Ages and during the Nazi period at the level of German counties. Our work differs in two respects. First, we employ an identification strategy for the local emergence of early anti-Jewish sentiment, which exploits plausibly exogenous variation in the existence of Jewish communities across German counties before the Black

Death. Second, we investigate the effect of anti-Jewish sentiment on households' current investments, and thus on their economic wealth.

Acemoglu, Hassan, and Robinson (2011) find that areas where the deportations of Jews were more extensive are less economically developed today. Jews were overrepresented in the middle and upper classes, which acted as the engine of economic growth after the Second World War. Pascali (2012) finds that Italian towns where Jews were expelled have a less developed banking system today. He argues that in towns where Jews persisted and provided financial services, the Catholic Church founded financial institutions to compete with them. These latter institutions constituted the backbone of the current Italian banking system. In this paper, we provide evidence of an effect of anti-Jewish *sentiment* on economic decision making. Our channel is therefore not related to societal composition or to the evolution of financial institutions.[12] Grosfeld, Rodnyansky, and Zhuravskaya (forthcoming) exploit the Pale of Settlement to show that households in towns with a higher Jewish population before the Holocaust have stronger antisemitic beliefs, are less supportive of the market economy, and more supportive of socialism. We build on their argument that antisemitic beliefs become stronger in the case of direct contact with Jews to instrument for the early diffusion of antisemitism across German counties.

The rest of the paper is organized as follows. Section 2 describes the measures of Jewish persecution in the past. Section 3 documents the correlation between Jewish persecution and current stock market participation. In Section 4, we exploit the early settlements of Jews along the Rhine Valley and their subsequent forced migrations to identify the effect of antisemitism on financial decisions. In Section 5 we corroborate our interpretation of anti-Jewish beliefs as distrust in the financial sector, whereas Section 6 investigates the properties of the effect of antisemitism on current outcomes. Section 7 concludes.

[12]In Table 8, we show that the number of bank branches per capita in German counties is unrelated to our measures of antisemitism. In addition, there is no evidence in Germany that financial institutions were founded by the church or others as a direct means of competing with Jews in providing financial services.

3 Measuring Historical Anti-Jewish Violence

Inspired by Voigtlaender and Voth (2012) and Voigtlaender and Voth (2013), we propose four measures of historical anti-Jewish sentiment at the level of German counties.

Black Death pogroms. The first measure, *Pogrom 1349*, is a dummy that equals 1 if a county experienced at least one anti-Jewish pogrom during the Black Death, that is, around 1349. The Black Death was arguably the worst pandemic in human history: up to one third of the European population may have been eliminated. It was followed by a period of deep economic and social crises throughout Europe. Historians refer to this period as one of the darkest eras in the history of humanity. Unsubstantiated theories on the origins and the diffusion of the pandemic diffused all over Europe. Accusations against the Jews, the most influential minority group in 14^{th}-century Europe, were common and often led to mass murders, persecution, and the elimination of entire communities, especially in the German lands. Consequently, as Voigtlaender and Voth (2012) argue, the severity of the Jewish persecution during the Black Death is a relevant measure of the origin of localized anti-Jewish sentiment.

Intensive and extensive margins of persecution. The second and third measures relate to the persecution of Jews during the Nazi period in Germany. The aim of these measures is to capture an extensive margin and an intensive margin of Jewish persecution. The *extensive* margin, namely, the overall extent of Jewish persecution, is the number of Jews from each German county who were deported to a Nazi concentration or extermination camp after 1933. This measure depends on the size of the Jewish communities within a county. By construction, it tends to be higher for counties with large cities and cities with Jewish ghettos. The *intensive* margin, namely, the relative extent of Jewish persecution, is the ratio of deported Jews during the Nazi regime over the total Jewish population in a county as of 1933, the year of the last elections in Germany before the beginning of the Third Reich. Jews started to hide and live in captivity after the Crystal Night of 1938, the largest single anti-Jewish pogrom in history. To locate hiding Jews, the SS relied on delation by locals, and households in areas where anti-Jewish sentiment was higher were presumably more likely to delate. This willingness enabled the

Nazis to retrace and deport high fractions of the Jewish population in those areas.

Principal component of measures. Finally, we also compute the first principal component of the three measures described above. The rationale is to isolate the common variation across the three alternative margins of persecution. Doing so may give a more precise account of the phenomenon of Jewish persecution in the past, and hence of the anti-Jewish sentiment across German counties.

Summary Statistics. We collect data from four sources. First, we obtain the characteristics of German households from the *Socio-Economic Panel* (SOEP) run by the *Deutches Institut fuer Wirtschaftsforschung Berlin* (DIW). The SOEP has conducted interviews on a yearly basis since 1984.[13] For each wave, the SOEP includes households that have been interviewed in previous waves as well as new households. Because we are interested in the cross-sectional association of anti-Jewish sentiment and stockholdings, we only include non-repeated observations when running the main analysis. Second, we collect data on Jewish persecution in the Middle Ages and during the Nazi period from Voigtlaender and Voth (2012), which also include medieval controls from Cantoni and Yuchtman (2012). We average socio-demographics as of 1933 at the current county level. Third, we obtain county-level historical socio-demographic characteristics from the *Ifo Prussian Economic History Database*, a project by CESifo Group Munich that collects historical information on Prussian counties in the 19^{th} century. We average the data at the current county level. Finally, we collect several current county-level socio-demographic controls from various sources: the average GDP per capita in the county from *DeStatis*, the index of land quality at the county level of Ramankutty et al. (2002), and the latitude and longitude of the centroid of each county and the ratio of college graduates over total population from Eurostat.

Table 2 reports summary statistics for the variables used in the analysis at the household level.[14] We collect them in three categories: measures of Jewish persecution, household-level characteristics from the SOEP, and county-level characteristics from the sources mentioned above. The left Panel reports statistics for all available observations, whereas in the right Panel , we restrict the sample to households with non-missing information.

[13] Researchers can access the county of residence of households after signing a non-disclosure agreement.

[14] For county-level variables, we attribute the same values to all households living in a given county.

The far-right column reports p-values for testing the null hypothesis that the means across the two samples are equal. The full sample of households in the SOEP county-level dataset includes 29,680 observations. The county of residence, which is crucial to our analysis, is not reported for 2,655 households. Moreover, we are missing the county-level historical information for 9,207 households in current counties that do not include any of the towns for which we have historical data.[15] The remaining missing data are due to missing answers to household-level demographics, such as the age, sex, and marital status of the household head.

Measures of Jewish persecution do not differ between the whole sample and the subsample of households without missing information. On average, 57% of households live in counties where at least one pogrom happened during the Black Death of 1349. The average ratio of Jews deported during the Nazi period is 34%. This ratio may be higher than 100%, because Jews may have migrated across counties between 1933 and 1945.[16] To compute this measure, we need to exclude all households in counties where the Jewish population in 1933 was reported to be zero.

Several household- and county-level characteristics differ across the two samples. Households in the running sample are more likely to own the house they live in (42% vs. 39%), and they have lower self-reported income (30,500 euros vs. 31,500 euros). Moreover, they are more likely to be mononuclear households, are less likely to be headed by a woman, and to hold a high school degree or higher education.[17]

Moreover, households in the running sample live in counties that had as of 1933 lower unemployment and a lower proportion of blue-collar workers, and a higher proportion of Catholics in 1925. They are also less likely to live in Eastern Germany, and they live in counties with a lower ratio of college graduates as of 2005.

The average ratio of stock-owning households across counties and over time is about 16%

[15] All results are similar if we attribute values of historical variables from neighboring counties to those for which we miss information. Because any rules for these manual inputs would be arbitrary, we simply discard households in counties for which we are missing historical information.

[16] The ratio exceeds one only in 1 county (Viersen, in North Rhein-Westphalen). If we exclude Viersen from the analysis, all results are virtually identical.

[17] Respondents report the gender of the person who usually makes financial decisions within the household. Individual-level variables refer to the same person, who need not be identified as the head of the household.

in both samples. The average age of the person who makes financial decisions is about 49 years in both samples. The two samples do not differ in terms of the geographic characteristics of the counties. They are similar with respect to the average population size, and no statistically significant difference is present in the average number of Jews in 1933.

4 Historical Anti-Jewish Violence and Current Stockholdings

In the baseline analysis, we estimate the association between the historical anti-Jewish violence and current stock market participation by German households in the period from 1984 until 2011. The following is our most general specification:

$$Pr(HoldsStocks_{ik}|X_{ik}, K_{ik}) = \Phi(\alpha + \beta \times Persecution_k + X'_{ik} \times \gamma + K'_{ik} \times \delta + \eta_t + \epsilon_{ik}) \quad (1)$$

where $HoldsStocks_{ik}$ is a dummy that equals 1 if household i holds any stocks, $Persecution_k$ is one of our four measures of historical anti-Jewish violence at the county k level; X_i and K_{ik} are sets of controls at the household and county levels, respectively, and Φ is the normal cdf. η_t are a set of time fixed effects, each capturing a group of four adjacent years.[18] We include time fixed effects in the analysis to capture the variation in average stock market participation over time.[19] Equation 1 provides reduced-form evidence about the correlation between historical anti-Jewish violence and the likelihood that households invest in the stock market. We compute standard errors of average marginal effects using the delta method. In all estimations, we allow for correlation of unknown form across residuals at the county level.

Nazi Persecution of Jews. Table 3 reports the average marginal effects for our baseline specification. In columns (1)-(3), $Persecution_k$ is the first principal component of the three measures of past Jewish persecution. In column (1), we include only historical socio-demographic controls at the county level, and hence variables that are measured contemporaneously to the Jewish persecution. We estimate that a one-standard-deviation

[18] Results do not change if we make the survey-wave fixed effects coarser or finer.

[19] Recall that we exclude repeated observations across survey waves; that is, we have no time variation in stockholdings at the household level.

increase in the principal component (1.016) is associated with 1.22-percentage-point lower stock market participation. This effect is statistically significant and economically large, because it represents 7.5% of the average participation.

In column (2), we add individual-level controls, a dummy that equals 1 for households that live in counties that were part of Eastern Germany, current socio-demographic controls at the county level, and wave-group fixed effects, which control for the time variation in average stock holdings from 1984 until 2011. The effect of antisemitism on stock holdings is similar to column (1) in terms of both magnitude and statistical significance. As expected, households with higher income and singles are more likely to invest in the stock market. Moreover, households in the former East Germany invest less in stocks. We do not find a statistically significant association of gender, age, or having at least a high school degree on household-level stockholdings after we control for county-level historical economic outcomes. Finally, in column (3), we estimate the marginal effect in a linear probability model. The parametric assumptions do not drive our results.

In columns (4)-(7), we use the two measures of the extensive and intensive margins of persecution. In columns (4) and (5), a one-standard-deviation increase in the logarithm of the overall number of Jews deported in a county (2.31) is associated with a 1.16-percentage-point lower likelihood that a household invests in stocks. The magnitude of the effect is similar to that of the principal component. In columns (6) and (7), a one-standard-deviation increase in the ratio of deported Jews over the total Jewish population in 1933 (0.192) is associated with a 0.61-percentage-point drop in the likelihood that a household invests in stocks. Statistical significance is weaker when we look at the intensive margin of persecution. Moreover, the sample is smaller, because we exclude households in counties with no reported Jewish population as of 1933. The county with the highest ratio of deported Jews over total Jewish population is Viersen, in North Rhein-Westphalen. A thought experiment that moves a household from Viersen to one of the 23 counties with a ratio of zero, keeping everything else constant, would increase by about 5 percentage points the likelihood that the household participates in the stock market. The average stock market participation of households in Viersen is 14.3%.

Pogroms in 1349. In columns (8) and (9), we measure historical anti-Jewish violence at

the county level as the occurrence of one or more pogroms during the Black Death (1349). Voigtlaender and Voth (2012) find this measure predicts the extent of Jewish persecution during the Nazi period at the town level. Both in the probit and in the linear probability specifications, households that live in counties with at least one pogrom in 1349 are on average 2 percentage points less likely to participate in the stock market. The size and statistical significance of the effect of individual- and county-level observables on current stockholdings are comparable to our previous specifications.

In summary, all different measures of deep-rooted antisemitism are negatively associated with the likelihood that a German household invests in the stock market today. The cross-sectional variation in antisemitism across German counties helps explain the heterogeneity in stock market participation across households living in different areas of Germany.

5 Early Jewish Migrations and Antisemitism

A major concern with the analysis so far is that some unobservable characteristics of German counties may have jointly determined the occurrence of pogroms in the Middle Ages as well as current stockholdings. Controlling for historical socio-demographics and for geographical characteristics of counties alleviates this concern, but it is not sufficient to establish a causal effect of anti-Jewish violence on current financial decisions. In this section, we develop an identification strategy. The ideal identification would exploit some plausibly exogenous variation in the extent of historical violence against Jews across German counties. Because the geographic heterogeneity of anti-Jewish sentiment has persisted since the Middle Ages, ideally this exogenous variation should be measured before the Black Death of 1349.

We propose a way to isolate quasi-exogenous variation in the likelihood that a Jewish community existed in a county before the Black Death and its massive anti-Jewish pogroms. Figure 2 depicts the core idea of our strategy. We exploit the forced migrations of Ashkenazi Jew communities out of the Rhine Valley since the First Crusade (1096). Archaeologists provide evidence of the earliest presence of Jews in the German lands in the cities of Trier, along the Mosel, and Cologne, along the Rhine. They date this

presence to the ninth century. Similar evidence of Jewish communities in the ninth and 10^{th} centuries has been documented along the entire Rhine Valley, mainly in the current states of Rheineland-Palatinate and North Rhine-Westphalen.[20] By contrast, the Jewish population in other areas of current Germany was sparse before the 11^{th} century, and it mainly comprised isolated households or merchants (Engelman (1944)). At the onset of the Crusades in the 11^{th} and 12^{th} centuries, early Jewish communities in the Rhine Valley were subject to persecution by Christian knights traveling to the Holy Land. Moreover, several cities expelled Jews from their territories, causing massive migratory movements of Jews towards Eastern Europe, as well as Northern and Southern Germany. Evidence of the existence of large Jewish communities, as inferred by the dimension of synagogues, dates back to the late 13^{th} and 14^{th} centuries in Munich (south) and Berlin (east), respectively. Migrations accelerated with the Black Death of 1349, when Jews were commonly attacked as being responsible for the devastating pandemic. Only in the 15^{th} century did Ashkenazi Jews merge with Jewish communities of Khazar origin who had moved from the Black Sea to current Poland.[21] Consistent with this pattern, in Figure 2 the darker a county is, the older the first Jewish community documented in that county.[22] The Rhine river is sketched as a light blue line in the western part of Germany. The red straight lines are *isodistance curves*, that is, points that lie at the same geographic distance from the Rhine Valley. The red arrows emphasize that the average age of the first documented Jewish community in a county increases as one moves towards the Rhine Valley.

We argue that the distance from the Rhine Valley is a proxy for the existence of early Jewish communities at the time of medieval persecutions, and hence of the emergence of anti-Jewish sentiment across the German lands. Specifically, our identification strategy includes three steps. In the first step, we use the distance of a county from the Rhine Valley to predict the probability that a Jewish community existed in a German county before the Black Death. In the second step, we use the predicted component of the probability of the existence of early communities to predict the extent of Jewish persecution. The

[20]For a recent and comprehensive economic history of European Jews in the Middle Ages, we refer to Toch (2012).

[21]See van Straten (2004) for archaeological evidence and Elhaik (2012) for genetic-based evidence.

[22]Blank counties are those for which we have no available data.

rationale is as follows: in counties with no Jewish communities before the Crusades, violence against Jews cannot have emerged, because no targets for such violence existed. Instead, in counties where early Jewish communities existed, the probability of a historical pogrom against the local Jews is strictly positive. In the third step, we use the predicted component of anti-Jewish violence at the county level to predict the current likelihood of stock investments at the household and county levels.

We base this argument on two crucial assumptions: (i) anti-Jewish sentiment had not spread widely outside areas without Jewish communities before the 10^{th} century;[23] (ii) the paths followed by Ashkenazi Jew communities forced to move out of the Rhine Valley were not systematically determined by unobservables that also determine current stockholdings. Below, we discuss some tentative evidence to support the exclusion restriction that makes our instrument valid.

Distance from the Rhine Valley. We consider three alternative measures of the distance of a county from the Rhine Valley. All three measures are the *residuals* from a regression of the geographic distance between the centroid of a county and a point in the Rhine Valley on observable proxies of economic activity at the county level in the Middle Ages:

$$DistanceRhine_k = \alpha + K_k' \times \delta + \epsilon_k, \tag{2}$$

where K is a set of dummy variables for whether the county hosted a monthly market, a bishop siege, any free imperial city, whether it was part of the Hanseatic League, whether it was incorporated before 1349, and geographic characteristics (latitude, the land quality index of Ramankutty et al. (2002), whether a navigable river existed in the county, whether the county lies in eastern Germany). To measure the distance from the Rhine Valley, we first compute the minimal Euclidean distance of the centroid of a county from the city of Trier, on the Mosel. The shortest distance (for the county that includes Trier) is about 2 km, whereas the greatest distance is about 1100 km. Second, we use the minimal Euclidean distance from the town of Emmerich at the north end of the river Rhine in

[23]This gradual diffusion would not be true in more recent periods, and especially in the 19^{th} century and during the Nazi period, when mass media and propaganda could easily spread antisemitic beliefs in areas more or less populated by Jews.

current Germany. As a third measure, we use the minimum of these two distances.

Exclusion restriction. Our identification argument builds on Grosfeld et al. (forthcoming), who show that areas with the largest Jewish communities in the Pale of Settlement were those where anti-Jewish beliefs were stronger in the past and are stronger today. Moreover, the staggered diffusion of Jewish communities during the Middle Ages across southern and eastern German counties due to transportation costs is consistent with historical evidence.[24] Two major concerns with our specific distance-based instrument are that (i) some unobserved confounders may have determined both the deep-rooted anti-Jewish sentiment and past conditions that explain current stock market participation across Germany, and (ii) an economic shock in the Rhine Valley posterior to the Black Death may explain our results. In both cases, the instrument would not affect current stock market participation through anti-Jewish sentiment only, and hence it would be invalid. We propose tentative evidence to support the exclusion restriction. In Table 4, we estimate OLS regressions of several county- and individual-level observables that are known to correlate with stock market participation on the residual distances estimated in Equation 2.[25] Different from customary reporting, we list the predicted outcome variables in the left column, and report the regressor at the top of each column. Each entry refers to a different regression, and it reports the estimated coefficient on the distance residuals. In odd columns, we use the residuals for the three distance measures regressed on medieval characteristics of counties as in Equation 2, whereas in even columns, we obtain the residuals for regressing the distances also on the whole set of economic observables at the county level as of 1933, that is, before the start of the Third Reich. Most distance residuals are not significantly associated with any of the current county- and individual-level observables. In most cases, the size of the estimated coefficients is small, which suggests the imprecision of our estimates does not drive the non-results. We find two exceptions: (i) the ratio of college graduates at the county level is positively associated with the residuals on the distance from Emmerich, but note

[24]For instance, Toch (2012) dates the first documented Jewish community in Munich to 1229, and in Berlin to 1295.

[25]Observables include current income and education at the county level, and income, education, gender, age, marital status, and homeownership status of individuals in the SOEP sample.

that the estimated coefficient is negative when we use the distance from Trier; (ii) the coefficients when age is the outcome variable have a non-negligible size, although we detect no statistical signficance.

In Table 5, for each measure of distance, we estimate the reduced-form effect of the distance from the Rhine on the ratio of households that own stocks within each county when the distance measures enter as regressors in lieu of anti-Jewish violence in the past, and when both the distances and one of the violence measures enter separately. All the coefficients refer to standardized variables. In columns (1), (4), and (7), indeed all three distances are positively associated with the likelihood that a household currently holds stocks. Interestingly, the size of the estimated coefficients for standardized distances are similar to those for anti-Jewish violence in Table 3. Once one of the anti-Jewish violence measures enters the specification together with each of the distance measures, we note two patterns: (i) the estimated autonomous association of distance with current stockholdings drops significantly in magnitude; and (ii) the estimated standard errors for each coefficient are virtually identical across all specifications. At the same time, the estimated associations between the violence measures and current stockholdings are similar to those in Table 3 in terms of magnitude and statistical significance. If one is still concerned that distances affect current stockholding through unobservable channels different from the historical anti-Jewish violence, such channels would hardly explain the whole effect we document, unless they were highly correlated with our measures of Jewish persecution.

In the Appendix, we provide two additional pieces of evidence to support the exclusion restriction. First, we derive the conditions for identifying our three-stage OLS system presented below, and we show evidence consistent with the non-correlation of our instrument with the residuals from the structural equations in the system. Second, we extend the analysis that exploits the voting pattern for the Nazi party across counties described in section 8 to alternative observables at the individual level, and we show that Nazi votes do not explain these alternative outcomes.

3SLS results. Table 6 reports results for estimating the three stages described above in

a three-stage OLS framework (see Becker and Woessmann (2009)) at the county level[26]:

$$Community1096_k = \alpha + \beta \times LogDistanceRhine_k + K_k' \times \delta + \epsilon_k \quad (3)$$

$$Persecution_k = \alpha + \beta \times \hat{Community1096_k} + K_k' \times \delta + \epsilon_k \quad (4)$$

$$RatioStockhold_k = \alpha + \beta \times \hat{Persecution_k} + K_k' \times \delta + \epsilon_k, \quad (5)$$

where $\hat{Community1096_k}$ and $\hat{Persecution_k}$ are the predicted values for county k when estimating the system of three simultaneous equations. Columns (1) to (3) of Table 6 report the coefficient on $LogDistanceRhine_k$ when estimating Equation 3 for the residuals of Equation 2 using the three alternative measures of distance from the Rhine valley. In column (1), a one-standard-deviation increase in the log residuals of the distance from Trier reduces by 12 percentage points the likelihood that an early Jewish community existed in a county. This figure is about 36% of the average likelihood that a county hosted a Jewish community before the Black Death, and specifically in 1096 (33%). Estimated magnitudes when using the distance from Emmerich and the minimal distance are similar: -15 and -13 percentage points, respectively.[27] Hence the farther a German county is from the Rhine, the less likely a Jewish community is to have existed there in 1096. In columns (4)-(6) of Table 6, we report results for the second stage. A one-standard-deviation increase in the likelihood of an early Jewish community increases the principal component of the persecution measures by 0.53, which is about one half of a standard deviation.[28] Again, the magnitudes are similar when we predict the likelihood of a Jewish community with the other measures of distance. Finally, columns (7)-(9) of Table 6 report results for the third stage. We use the predicted component of Jewish persecution from the second stage to predict the current ratio of stock-owning households in a county. Consistent with the reduced-form results, an increase in the instrumented persecution of Jews significantly

[26]Results are similar if we estimate the system at the individual level. Because the first and second stages involve outcomes that vary only at the county level, we find it more conservative to report results for variables aggregated at the county level in all stages.

[27]The standard deviations of the log distances from Trier, Emmerich, and the minimal distance are 0.69, 0.62, and 0.77, respectively.

[28]We look at the principal component measure of persecution because it captures the common variation across all three measures of persecution.

reduces stock market participation at the county level. The implied magnitudes of the IV estimates are larger than in the reduced form at the county level (see column (1) of Table 7). We expect this pattern due to the plausible selection of Jewish communities in areas where financial services were in greater demand in the past. This selection would bias the magnitude of the reduced-form coefficients downwards; that is, our estimated effect of anti-Jewish sentiment on current stockholdings in the reduced-form analysis would be lower than the true effect.

Robustness. In Table 7, we further test the robustness of our main results. In columns (1)-(3), we report coefficients for estimating the baseline specification at the county level, where household-level observations are averaged at the county level. This test addresses the concern that individual observations in a given county may be spatially correlated in an alternative way than clustering the standard errors at the county level. The magnitude and significance of coefficients are similar to the household-level analysis when we use the principal component of Jewish persecution measures. In column (2) of Table 7, we also report results for a reduced-form regression in which we use the minimal distance from Trier and Emmerich as a measure of early antisemitic beliefs. Again, a larger distance implies lower historical antisemitism and higher stock market participation. The magnitude of the coefficient is similar to the household-level analysis, although statistical significance is minor.

We then run the reduced-form analysis on three subsamples of households. First, one may be concerned that religious and racial stereotypes only develop in areas where education is low. In columns (4)-(6), we exclude all households that live in counties in the bottom third of the distribution by the share of inhabitants with a college degree or higher levels of education in 2005. Second, counties with weak economic fundamentals may drive the effects. In columns (7)-(9), we exclude households living in counties in the bottom third of the distribution by GDP per capita in 2005. Estimated effects are similar to the analysis for the full sample. A third concern is that the economic conditions at the time we measure antisemitism, and especially during the economic crisis of the 1930s, may drive the effect. We repeat the analysis excluding all households from counties in the top third of the distribution of unemployment in 1933. The estimated effect of Jewish persecution on

current stockholdings is slightly lower than that in Table 3, but the statistical significance is unchanged. The effect of the distance from the Rhine Valley is similar to the main estimate, whereas the effect of having experienced at least one pogrom in 1349 becomes statistically insignificant.

In columns (13) and (14) of Table 7, we propose a placebo test of our instrument. If the distance from the Rhine Valley indeed captures the emergence of early antisemitic beliefs, we should find no effect on current stockholdings if we use the distance of a county from towns far away from the Rhine Valley. We compute the distance of each county from the city of Ulm, in Eastern Baden-Wuerttemberg (south). Column (10) shows that this distance has no effect on the likelihood that households own stocks. Similarly, in column (11), we employ the distance from Flensburg, in Schleswig-Holstein (north). We again find no effect of this pseudo distance on current stockholdings.[29]

6 Antisemitism and Distrust in the Financial Sector

So far we have documented that deep-rooted antisemitic beliefs at the county level affect the decision of current households to invest or not in the stock market. We argued that antisemitic beliefs proxy for a specific form of distrust, that is, distrust in the financial sector. Unfortunately, we cannot test this claim in our data. Ideally, we would want to ask households whether they trust the financial sector. Then we would test if, after controlling for individual- and county-level characteristics, the historical anti-Jewish violence is positively correlated with this direct measure of distrust in the financial sector. To corroborate our interpretation, we propose a placebo analysis: we test whether anti-Jewish violence has any effect on outcomes that, under our interpretation, should not be affected by it. We verify that no correlation exists between anti-Jewish violence and these alternative outcomes.

Table 8 reports the results of this placebo analysis. First, we test if a change in the skill structure of counties after the persecution of Jews drives our findings. Acemoglu et al. (2011) show that Jews were overrepresented within the upper middle class in Russia. Areas where more Jews were eliminated during the Nazi period grew less after the war

[29] Placebo results are similar if we measure the distance from other northern or southern German towns.

due to the scarcity of members from this dynamic social class during the reconstruction period. In our case, because a large share of finance workers were Jewish, persecution of Jews in the past may have hindered the development of local financial services at the county level. This possibility may explain why the current inhabitants of those counties invest less in stocks. In this case, the availability of financial services today should be lower in counties where persecution of Jews was higher in the past. In Panel A of Table 8, we look at the effect of three persecution measures on current private bank branches per capita at the county level.[30] We find no effect of persecution on bank branches per capita in reduced-form regressions (columns (1)-(6)) or in the IV results (columns (7) and (8)). If anything, we find a slightly positive effect of persecution on the availability of bank branches before we control for covariates. This result would suggest that areas with higher persecution were those where the demand for financial services was higher, which would bias the size of our reduced-form coefficient in Table 3 downward. Second, we test whether anti-Jewish violence affects the decision to invest in real estate. Like stocks, real estate is an illiquid investment. Contrary to stocks, households do not necessarily need to interact with financial sector operators when buying a house.[31] In Panel B of Table 8, we estimate Equation 1 using a dummy variable that equals 1 if the household owns the house in which they live. None of the estimated effects is economically or statistically significant (columns (1)-(6)). In columns (7) and (8), we estimate Equation 3 using homeownership as a dependent variable. Again, we do not find any effect of historical anti-Jewish violence on the current German households' decisions to invest in real estate.

In Panel C of Table 8, we look at the decision of households to invest in life insurance products. Life insurance is an illiquid investment. Contrary to lending, the Church did not ban Christians from providing insurance in the past: the premium an insuree pays is justified by risk, which is transferred to the insurer. Hence insurance does not involve the remuneration of time, as is the case in lending. Under our interpretation, we should

[30] All results are very similar if we consider the ratio of government-owned bank branches (*Sparkassen, Landesbanken*) or cooperatives (*Raiffeisenbanken*) or the ratio of all bank branches over population.

[31] One could argue that most households apply for mortgages when buying a house. Note that the realtor usually mediates the mortgage negotiations between the household and the bank. In addition, around 70% of mortgages are provided by cooperatives and government-owned banks and are therefore unrelated to international financial markets. Yet, to address this point, we alternatively look at the decision to invest in life insurance.

see no effect of antisemitism on the propensity of households to invest in life insurance, and indeed we do not: we find no economically or statistically significant effect of our measures of deep-rooted anti-Jewish sentiment on current German households' decision to buy insurance.[32]

Anti-Jewish sentiment may correlate with a broader distrust of households toward the market economy and capitalism (Grosfeld et al., forthcoming). Households in areas we identify as antisemitic may, for ideological reasons, dislike accumulating wealth, which would also explain why they invest less in stocks. We check if our measures of anti-Jewish violence affect the logarithm of households' incomes today. The rationale is that if a household does not want to be part of the capitalistic economy, it would be content with a job that pays less on average. In Panel D of Table 8, we estimate an OLS version of Equation 1, where the dependent variable is the logarithm of households' income. We do not find any significant effect of antisemitism on the income of households.

Moreover, one might think the most antisemitic counties are also the most socially conservative ones. Social conservatism may have made households suspicious of non-traditional forms of investing over time. To investigate this aspect, we estimate the effect of anti-Jewish violence on the likelihood that a female member of the household is in charge of financial decisions.[33] We do not find any effect of Nazi persecution of Jews, or the likelihood of pogroms in 1349, on the likelihood that a woman is declared to be in charge of financial decisions within the household (Panel E). The reduced-form coefficient for the distance of a county from the Rhine (Emmerich) is positive and statistically significant in column (5); once we add county- and individual-level controls, though, the magnitude of the estimated coefficient drops, and we fail to reject the null of no effect of antisemitism at any plausible level of significance. We do not find any effect of antisemitism on the likelihood of a woman being in charge of financial decision-making once we estimate an IV specification in columns (7) and (8).

Finally, one might think a high level of antisemitism in a county is due to lower cognitive

[32]Note that investing vehicles such as the 401(k) plans in the United States did not exist in Germany until the early 2000s. Their effect on our outcomes are captured by the time fixed effects.

[33]When surveyed, households declare who is in charge of financial decisions within the household. More socially conservative households would arguably tend to name the most senior men within the household as being in charge, notwithstanding who is indeed in charge of such decisions.

abilities of households living in those counties. This possibility could make households less aware of the opportunities offered by stock markets or less financially literate. Ideally, we would like to test households' cognitive abilities using standardized tests, but doing so is infeasible. We instead look at their education level. In Panel F, we test for an effect of historical anti-Jewish violence on the likelihood that the household head has at least a high school degree. Across all our measures, and when instrumenting persecution with the distance of a county from the Rhine, we fail to detect any effect of anti-Jewish violence on the education level of household heads.

7 Properties of the Effect of Antisemitism on Stockholdings

In the previous sections, we have established that deep-rooted antisemitism reduces German households' stock market participation. In the following, we aim to better understand the properties of this effect.

Effect over time. First, we study whether the magnitude of the effect of antisemtism on stock market participation varies over time. Voigtlaender and Voth (2012) show that the cross-sectional variation of antisemitic beliefs is remarkably stable over time. This observation suggests that as long as these beliefs have an effect on current investment decisions, such an effect should be stable as well over the years. At the same time, periods of higher or lower salience of antisemitism may induce a higher or lower effect, respectively. Alternatively, the effect might simply fade away over time, if households become more financially literate or stop relying on deep-rooted beliefs and cultural cues when making their investment decisions.

The SOEP data allow us to observe stock market participation from 1984 until 2011, that is, for more than 25 years. In those years, stock market participation has varied substantially. Moreover, the German stock market has witnessed periods of booms and busts: the crash of 1987, the bull markets during the "dot-com bubble" and the subsequent burst, and the financial crisis of 2008.

In Table 9, we replicate the estimation of Equation 1, adding a full set of interactions between each measure of persecution of Jews and time dummies capturing periods of

four years.[34] All other controls are the same as in Table 3. The omitted category is the most recent group of waves, from 2008 to 2011. If the effect of anti-Jewish violence on financial decision-making fades away over time, we should find that the coefficients on the interaction terms are significantly negative. Moreover, their magnitude will decrease monotonically over time. If the effect follows periods of higher or lower salience of antisemitism, and if antisemitism is higher during economic crises, we will observe positive interaction coefficients for non-crisis years, because the omitted category captures the financial crisis period after 2008 and the subsequent Great Recession.[35] If the effect is stable over time, we will find that the coefficients attached to the interaction terms are indistinguishable from zero, that is, the effect is the same across all time periods as it is for the omitted time category.[36] Across all measures of historical anti-Jewish sentiment, results in Table 9 are consistent with the latter hypothesis. In columns (1) and (2), the effect of the principal component in the period 2008-2011, as captured by the baseline coefficient, is similar to the baseline effect in Table 3 in terms of magnitude and statistical significance. At the same time, none of the coefficients attached to the interaction terms differ significantly from zero.[37] Results are similar when we consider the extensive margin (columns (3) and (4)), the intensive margin (columns (5) and (6)), and pogroms in 1349 (columns (7) and (8)).

Effect across cohorts. All results so far refer to the average stock market participation of Germans across all cohorts that have been interviewed yearly by the SOEP. But different cohorts have been exposed to vastly different macroeconomic conditions throughout their lives. Moreover, individuals born before the rise of Hitler directly experienced the massive antisemitic propaganda and the anti-Jewish policies of the Nazi regime.

In Panel B of Table 9, we test whether and why the impact of antisemitism on financial decision-making has different magnitudes across cohorts. We divide the sample of

[34]Results do not change if we change the number of years covered by each dummy.

[35]One may also think the peace process in the Middle East shapes current antisemitism. We might observe stronger effects during periods of turmoil in Israel and Palestine, such as during the First Intifada (1987 to 1993) and the Second Intifada (2000 to 2005).

[36]This claim does not mean that the wave-group dummy variables in level should have zero coefficients. In fact, average stock market participation varies dramatically over time.

[37]Note that the effect in the period 1984-1987 is non-negligible, but statistically indistinguishable from zero.

households into three cohorts based on the year the head of the household was born: the *Nazi cohort* includes households whose head was born before 1945;[38] the *Postwar cohort* includes those born between 1945 and 1965; the *Post 1965 cohort* includes those born after 1965. Averaging out time effects in this analysis is crucial because observations in the Nazi cohort are concentrated in the first half of the SOEP waves, whereas those in the Post 1965 cohort are concentrated in the second half of our sample, and the average stock market participation has varied substantially between these periods.

We replicate the estimation of Equation 1 and add interactions of measures of Jewish persecution with a cohort dummy. The Postwar cohort is the omitted category. The effects we estimate for the Postwar cohort are similar to the baseline estimates in Table 3 in terms of both magnitude and statistical significance. Interestingly, the effect seems to disappear for households in the Nazi cohort across all our measures. Two explanations seem plausible. On the one hand, the massive antisemitic propaganda under the Nazi regime may have represented a permanent shock to the level of anti-Jewish sentiment within German counties, and therefore it may have eliminated its cross-sectional variation across counties. This explanation would be inconsistent with the 600-year-long persistence in the heterogeneity of anti-Jewish sentiment documented by Voigtlaender and Voth (2012). On the other hand, the Nazi cohort witnessed periods of social disruption and major economic crisis, especially before the rise of Hitler. Malmendier and Nagel (2011) show that cohorts that experienced the Great Depression are less likely than others to invest in the stock market. Under this explanation, the effect of antisemitism on individual financial decision making could be stable across cohorts, but it would be of second order when interacted with personal experiences of large business-cycle fluctuations. To disentangle the two hypotheses, we exploit the cross-sectional variation in the depth of the economic crisis in the 1930s at the county level. Specifically, we look at the stock market participation of Nazi cohort households sorted by the county-level unemployment rate in 1933. We divide counties into three groups with low, medium, or high levels of unemployment in 1933. In untabulated results, we estimate triple interactions of persecution with the households'

[38] One might be concerned that individuals who were too young in the early 1940s were not exposed to the massive antisemitism of the Nazi regime. For instance, the Nazi cohort might better be constituted by those born before 1930. If we define the cohorts using different time cuts, results are very similar.

cohort and with dummies for the severity of unemployment in 1993. We do find that the reversal of the effect of antisemitism for the Nazi cohort is driven by households in counties where the level of unemployment was high: the estimated coefficient on the interaction between the Nazi cohort and persecution is significantly positive (0.021, s.e. 0.008), but the coefficient on the triple interaction of Nazi cohort, persecution, and low unemployment in 1933 is significantly negative (-0.029, s.e. 0.008). Because the baseline effect is negative (-0.017, s.e. 0.006), Nazi cohort households that lived in areas with a less dramatic economic crisis during the 1930s are negatively affected by historical anti-Jewish violence, similar to households in other cohorts. When we look at triple interactions of other persecution measures with cohorts and unemployment in 1933, the estimates are noisy, and we do not detect any statistically significant interaction terms.

Overall, the evidence suggests the effect of antisemitism on financial decision making is stable across time and across cohorts, but it may be of second order to the effect of dramatic economic lifetime experiences.

Effect by education levels. One might wonder if our measures of anti-Jewish violence capture the households' generalized trust toward others. Guiso et al. (2004) show that households that live in areas where generalized trust toward others is lower are less likely to hold stocks. Generalized trust is especially important for households whose education level is low and that have difficulties understanding the content of financial contracts. These households will only sign financial contracts if they trust their counter-party. By contrast, highly educated individuals understand the content of financial contracts and do not need to rely on trust. In Panel C of Table 9, we interact our baseline measures of persecution with dummies capturing counties in the top and the bottom third of the distribution based on the ratio of college graduates in the population. If antisemitism is a proxy for generalized trust, we should find that our baseline effects are driven by households in counties where the average level of education is lower. This prediction is not consistent

with the results.[39] When we use the principal component of the measures of persecution, we find that, if anything, the effect of antisemitism is lower for households in counties with lower education, as compared to those in the middle and top of the distribution. We do not find any heterogeneity in the baseline effect across education levels when using the other measures of historical anti-Jewish sentiment. Hence we conclude that the effect of antisemitism on current stockholdings is distinct from generalized trust.

8 Anti-Jewish Ideology, Nazi Votes, and Stockholdings

So far, we have used indirect measures of historical anti-Jewish sentiment based on past violence against the Jews. But these measures may capture incentives other than ideology for individuals to attack Jews. Individuals may have hoped to obtain rewards based on the number of Jews they helped the Nazis locate. As for pogroms, individuals may have hoped to seize Jewish property if they took part in the attacks against Jews.

In contrast to these measures, voting is unobservable and not verifiable; hence it was unlikely to raise the expectation of rewards by the Nazis before the start of the Third Reich. Voting also has undesirable properties, though. First, the Nazi ideology also included elements different from antisemitism, for instance, economic and political nationalism and the rejection of established religions. Moreover, the Nazis rose to power during a long and deep economic crisis: hyperinflation was a major concern and high unemployment rates plagued several areas. The democratic institutions of the Weimar Republic were perceived as ineffective. Most voters are likely to have supported the Nazi party (which had not been in power before 1933) in the hope of improving their economic conditions. de Bromhead et al. (2013) look at the voting patterns for extreme-right parties in the 1920s and 1930s in European countries. They indeed find that persistently

[39]We also run a more direct test adding a measure of generalized trust to the baseline equation. Our measures of anti-Jewish sentiment are not correlated with generalized trust, and their association with current stockholdings is virtually identical when we add trust as a covariate. We do not report these results in the main text for two reasons. First, our measures of generalized trust from the World Value Survey and the SOEP are not necessarily representantive at the county level. Second, consistent with Buggle (2013), trust increases with the distance from the Rhine Valley. He attributes this pattern to the Code Napoleon, which was mainly imposed to households living in the western part of Germany. Since this shock is posterior to our period of estimation of the anti-Jewish sentiment, currently-observed trust is probably different from the generalized trust in the Middle Ages.

depressed economic conditions are a strong predictor of the electoral support of right-wing, anti-system parties. Thus, voting for the Nazi party in 1933 should be a valid proxy for anti-Jewish sentiment in areas where unemployment in 1933 was low. The economic crisis was less severe in those areas, and votes for the Nazis are more likely to capture the local support for their ideological and political platform. By contrast, votes for the Nazis should be a noisier proxy for anti-Jewish sentiment in counties where unemployment was high, that is, where voters were likely driven by economic motives when voting for the Nazis. We therefore expect a negative association between Nazi vote shares in 1933 and current stockholdings in counties where unemployment was low in 1933, and a less negative effect in other counties. We find exactly this pattern. In Figure 3, the left vertical axis reports the average marginal effect of the Nazi county-level vote share in 1933 elections on current stockholdings, and is associated with the histograms. The horizontal axis indicates the percentile of the distribution of counties by the unemployment rate in 1933. We estimate our baseline specification for cumulative percentiles of this distribution. For instance, the histogram labeled "20" reports the average marginal effect for estimating Table 3 only for households that live in counties in which the unemployment rate in 1933 was below the 20th percentile. The right vertical axis reports standard errors attached to each marginal effect, which are clustered at the county level. They are associated with the black line. Dark brown histograms are marginal effects that are significant at the 1% level or lower; orange histograms are significant at the 5% level; white histograms are not significant at any conventional level. The average marginal effect of Nazi vote shares in 1933 is significantly negative in counties where unemployment in 1933 was low, up to the 45th percentile of the distribution. The magnitude of the effect and its statistical significance are still high but lower up to households in the 80th percentile of the unemployment distribution in 1933. Instead, the estimated effect becomes economically small and statistically insignificant once we add households in counties above the 85th percentile of the distribution for unemployment in 1933.

In Figure 4, we propose a placebo analysis to corroborate our interpretation of the evidence. If we sort households by the unemployment rates across counties, the share of votes for the Nazi party in a county is not significantly associated with current

outcomes. In particular, we do not find patterns similar to Figure 3 for the likelihood that households invest in life insurance products or real estate, and for education, gender, and age of the household head. The pattern for household income seems compatible with the one for holding stocks, although none of the estimated effects is statistically significant. Importantly, Adena et al. (2013) show that the radio had opposite effects on the vote shares of the Nazis based on who controlled the aired content: when the content was anti-Nazi (1929-1932), the radio reduced the vote share of the Nazis, while once the Nazis obtained the control of the radio, it had a large positive effect on their vote share (March 1933). In untabulated results we find a virtually identitcal pattern as in Figure 3 when we look at the Nazi votes in the elections of September 1930, for which we have detailed data at the county level, and which were held after the beginning of the economic crisis but in a time when the radio had a negative effect on the Nazi vote share.

9 Channels

Our analysis so far was silent on the channels through which deep-rooted anti-Jewish sentiment in a county may affect the likelihood that current inhabitants invest in stocks. Plausible channels should link past and current anti-Jewish sentiment to be consistent with the evidence of Voigtlaender and Voth (2013). We discuss two potential channels that are consistent with our evidence. On the one hand, current households across all counties may consciously associate financial markets, but not other financial services, with the Jewish population. Those who have obtained a higher anti-Jewish sentiment from their ancestors would therefore invest less in stocks because they have a negative view of the Jewish population and everything the Jews stereotypically represent.

On the other hand, a strict connection between the Jewish population and financial services may have been vivid in the past, and it may have promoted two autonomous norms that transmitted across generations within counties: anti-Jewish sentiment, and distrust towards financial services. While the direct experience and familiarity with local financial services, such as banks, should have reduced the distrust towards banks over time, distrust may have not faded away for those financial institutions households have no

direct experience of, such as stock markets. Hence, areas where Jews were more disliked in the past would be areas where current households distrust stock markets the most, although the association between these two dimensions needs not be explicit nowadays.

It is hard to disentangle these two channels, because it is impossible to obtain household-level evidence on past beliefs and past investments. At the same time, any survey that asked current households for their motivations to invest in stocks, and in particular whether it relates to their level of antisemitism, would hardly elicit any reliable information. An ideal test would manipulate implicit discrimination cues in a controlled environment, for instance with IAT tests as proposed by Bertrand et al. (2005). Unfortunately, the decision process of investing in stocks does not satisfy criteria that are thought to be important for implicit discrimination to arise, such as time pressure, considerable ambiguity of whether investing in stocks is good or bad, or a simple nonverbal outcome for the decision task. We attempt to provide some tentative evidence on the channels by looking at how households' stock investments vary across counties based on the fraction of workers of Jewish religion in the financial sector in the near past.

The rationale is as follows: in the Middle Ages the entirety of financial services were run by Jews due to the ban on Christians to lend money at interest. Over time, after the ban was eliminated, the ratio of Jews in finance must have declined, and it may have declined heterogeneously across different German counties. The ratio must have tragically dropped toward zero after the Nazi period. This interpretation is consistent with the evidence of Table 1, which shows that in 1882, the ratio of German Jews in finance was larger than that of the overall Jewish population but well below 100%. If the association between Jews and distrust in finance was still vivid today, we would expect that the likelihood to invest in stocks today is negatively correlated with the share of Jews in finance as of 1882, similar to what we find for the votes for the Nazis in the previous section. A similar argument can be made for the share of finance employees of Jewish religion in a county as of 1933, which we proxy with the share of Jews in the population.[40] On the contrary, if the association was vivid in the very past and it created two *autonomous* norms over time

[40]There is variation in both shares across counties. The mean ratio of Jews in finance in 1882 is 23.1%, with a standard deviation of 29.1% and a range of [0%,100%]. The mean ratio of Jews in the population in 1933 is 1.9%, with a standard deviation of 7.9% and a range of [0%,54%].

- anti-Jewish sentiment and distrust in financial markets - we would find that deep-rooted anti-Jewish sentiment explains current stockholdings, while the share of Jews in finance in the recent past does not. We document a pattern that is consistent with the latter hypothesis. We obtain the residuals from the following regression:

$$HoldsStocks_{ik} = \alpha + X'_{ik} \times \gamma + K'_{ik} \times \delta + \eta_t + \epsilon_{ik} \tag{6}$$

where $HoldsStocks_{ik}$ is a dummy that equals 1 if household i holds any stocks, X_{ik} and K_{ik} are a set of controls at the household and county levels, respectively, and η_t are a set of time fixed effects, each capturing a group of four adjacent years.

In Panel A of Figure 5 we plot the mean of the estimated residuals from Equation 6, $\hat{\epsilon}_{ik}$, across households in counties that belong to the same decile of the distribution of counties based on the ratio of financial sector workers who were Jewish in 1882. We do not find a negative association between the share of Jews in finance and current stockholdings in this non-parametric setting. Similarly, when we estimate the mean of the residuals from Equation 6 across deciles of households based on the Jewish population in their county as of 1933, we do not find any negative association between stockholdings and the presence of Jews. This tentative evidence is consistent with two autonomous norms that have been transmitted across generations at the county level: anti-Jewish sentiment, and distrust in financial markets.

10 Conclusions

We exploit the variation in historical anti-Jewish violence across German counties to proxy for households' distrust in financial markets. A one-standard-deviation increase in the past persecution of Jews reduces by 1.2 percentage points the likelihood that a German household invests in stocks, which is 7.5% of the average participation rate. Households in counties that experienced at least one pogrom in 1349 are 2 percentage points less likely to invest in stocks than other households. We confirm these effects when we instrument for early anti-Jewish violence with the distance of a county from the early settlements of Ashkenazi Jewish communities. The magnitude of the effect is stable from 1984 until

2011 and across cohorts, except for those who experienced the Nazi period. We show that this non-result is due to their direct experience of the economic crisis in the interwar period. When we look at education, we find the effect is similar in counties with high and low current education levels. Hence the channel we capture differs from generalized trust in others. Finally, we replicate our results when proxying anti-Jewish sentiment with the electoral support for the Nazis. We argue that the ratio of Nazi votes is a good proxy in counties with a low unemployment rate during the 1930s, but not for counties with high unemployment, where economic rather than ideological reasons presumably drove the support for the Nazis.

Because anti-Jewish sentiment is a cultural trait transmitted across generations, these results contribute to the interdisciplinary debate on hatred beliefs and their consequences on individuals and societies. To our knowledge, these results are among the few to imply that policies designed to correct deprecable stereotypes and norms, especially in the case of racial and religious hatred, should not only be inspired by ethical considerations and the promotion of human rights, but also by economic wealth of the broader population. Anti-Jewish sentiment reduces stock market participation. Therefore, given the historically large equity risk premium, deep-rooted antisemitism arguably reduces the financial wealth of households, but it also lowers the funds available for productive investments at the firm level, and hence both individual as well as societal welfare.

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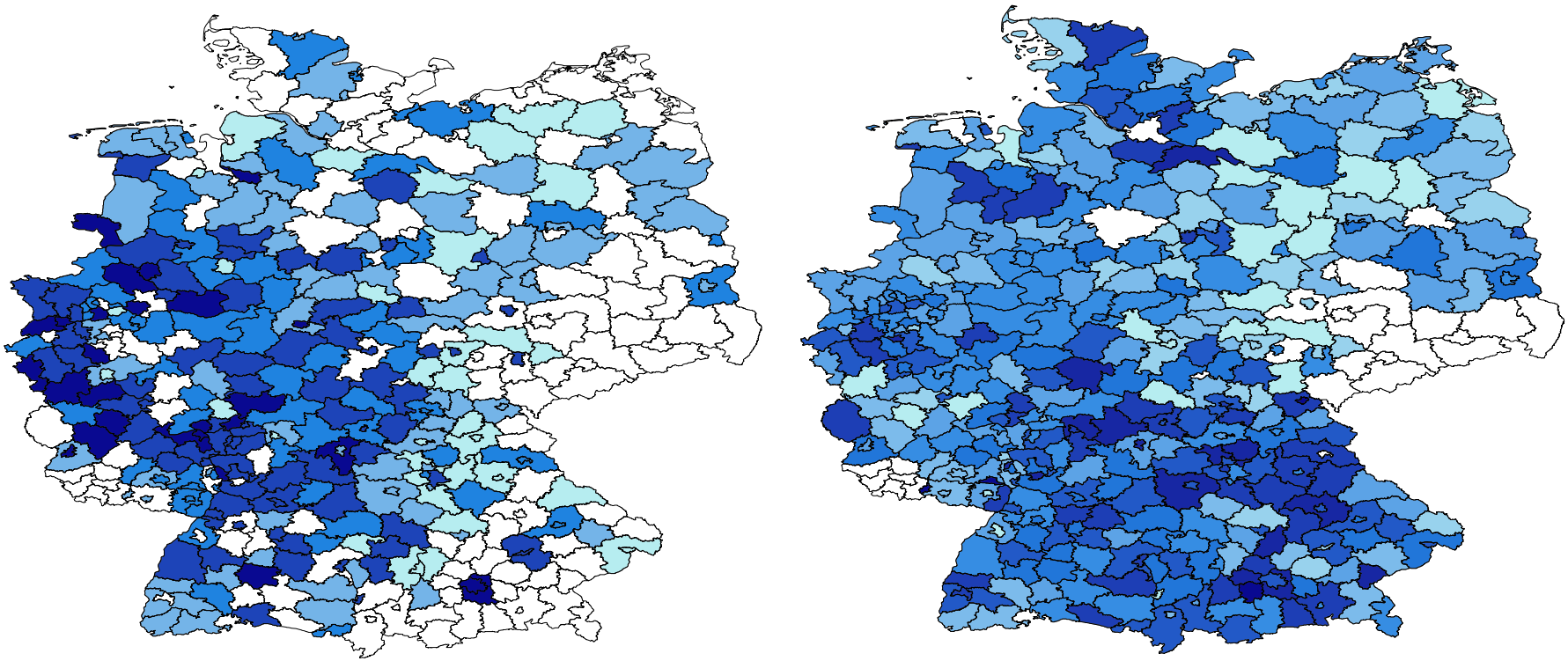
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Figure 1: **Jewish Persecution and Current Stockholdings**

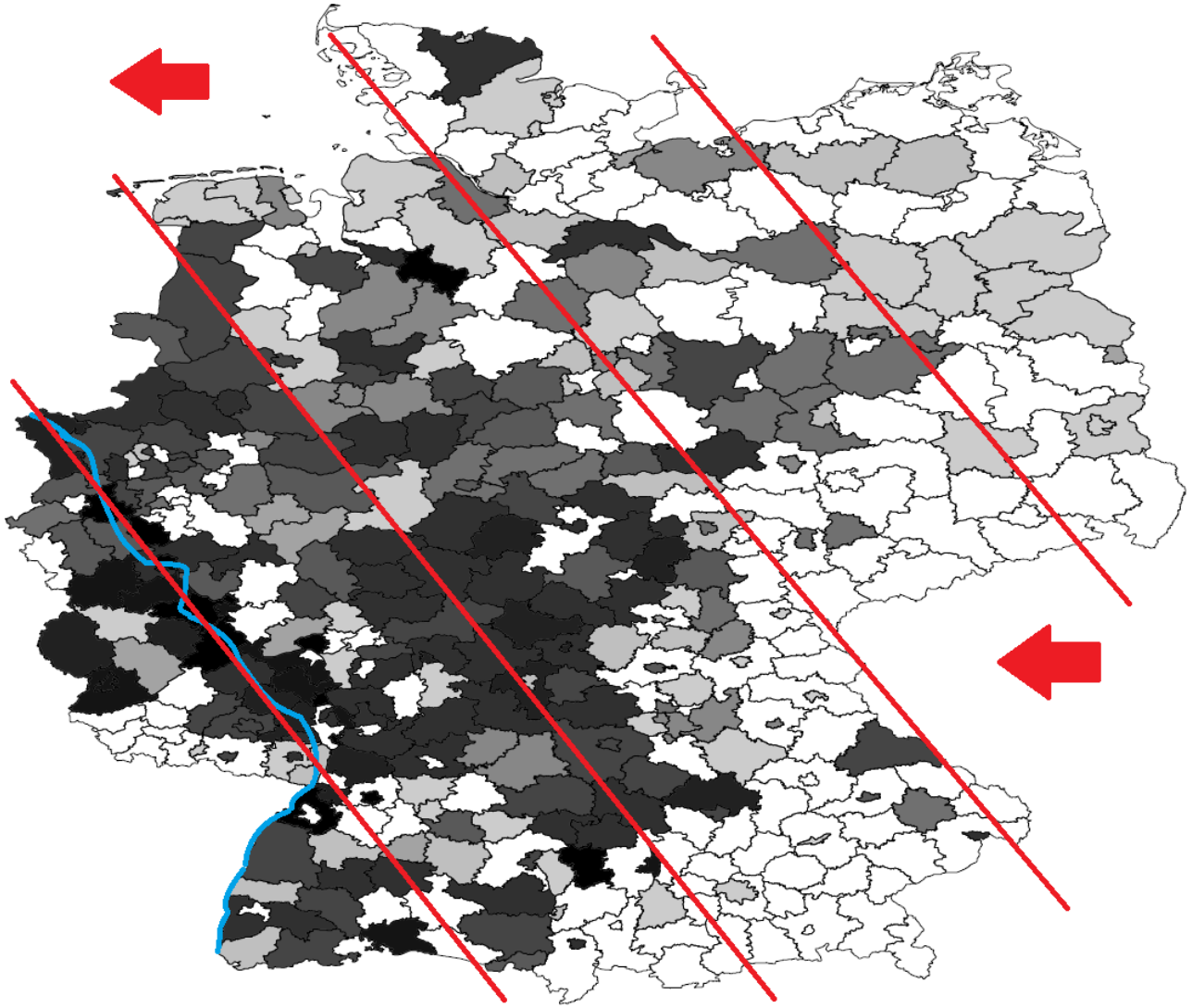
(a) Jewish Persecution Nazi Period

(b) Current Stock Market Participation



In both maps, the darker a county is, the higher the value of the depicted variable. Blank counties are those for which the data are not available. The map on the left in Figure 1 depicts the principal component of the dummy for pogroms against the Jews in a county during the Black Death (1349), the extensive margin of persecution (log of deported Jews), and the intensive margin of persecution (ratio of deported Jews over Jewish population in 1933) against the Jews during the Nazi period. The map on the right in Figure 1 depicts the average yearly ratio of households who have invested in stocks from 1984 to 2010 across German counties.

Figure 2: **Distance from the Rhine Valley and Jewish Communities**



In this map, the darker a county is, the earlier a Jewish community was documented in that county. Blank counties are those for which the data are not available. The light blue line on the left sketches the Rhine river. The intersection of the Rhine and Mosel Valleys roughly corresponds to the current German states of Rheineland-Palatinate and North Rhein-Westphalen. The red straight lines are *isodistance curves*, that is, points that lie at the same distance from the Rhine Valley. The red arrows emphasize that the average age of the first documented Jewish community in a county increases as one moves towards the Rhine Valley.

Figure 3: **Nazi Votes, Economic Crisis, and Stock Market Participation**



Figure 3 plots the average marginal effects of the variable $VoteShareNazi1933$ computed after after estimating the following probit specification in subsamples of households sorted by the unemployemnt rate in their county of residence as of 1933:

$$Pr(HoldsStocks_{ik}|X_i, K_{ik}) = \Phi(\alpha + \beta \times VoteShareNazi1933_k + X_i' \times \gamma + K_{ik}' \times \delta + \eta_t + \epsilon_{ik})$$

Each observation is a German household interviewed by the SOEP any time between 1984 and 2011. The left vertical axis reports the average marginal effect of $VoteShareNazi1933$, and it is associated with the histograms. The horizontal axis indicates the percentile of the distribution of counties by the unemployment rate in 1933 below which the estimation is performed. For instance, the histogram labeled "20" reports the average marginal effect for estimating the probit model only for households who live in counties were the unemployment rate in 1933 was below the 20th percentile; the histogram labeled "30" reports the average marginal effect for estimating the probit model only for households who live in counties were the unemployment rate in 1933 was below the 30th percentile. The right vertical axis reports standard errors attached to each marginal effect and clustered at the county level. They are associated with the black line. Dark brown histograms are marginal effects that are significant at the 1% level or lower; orange histograms at the 5% level; white histograms are not significant at any conventional level.

Figure 4: **Nazi Votes and Placebo Outcomes**

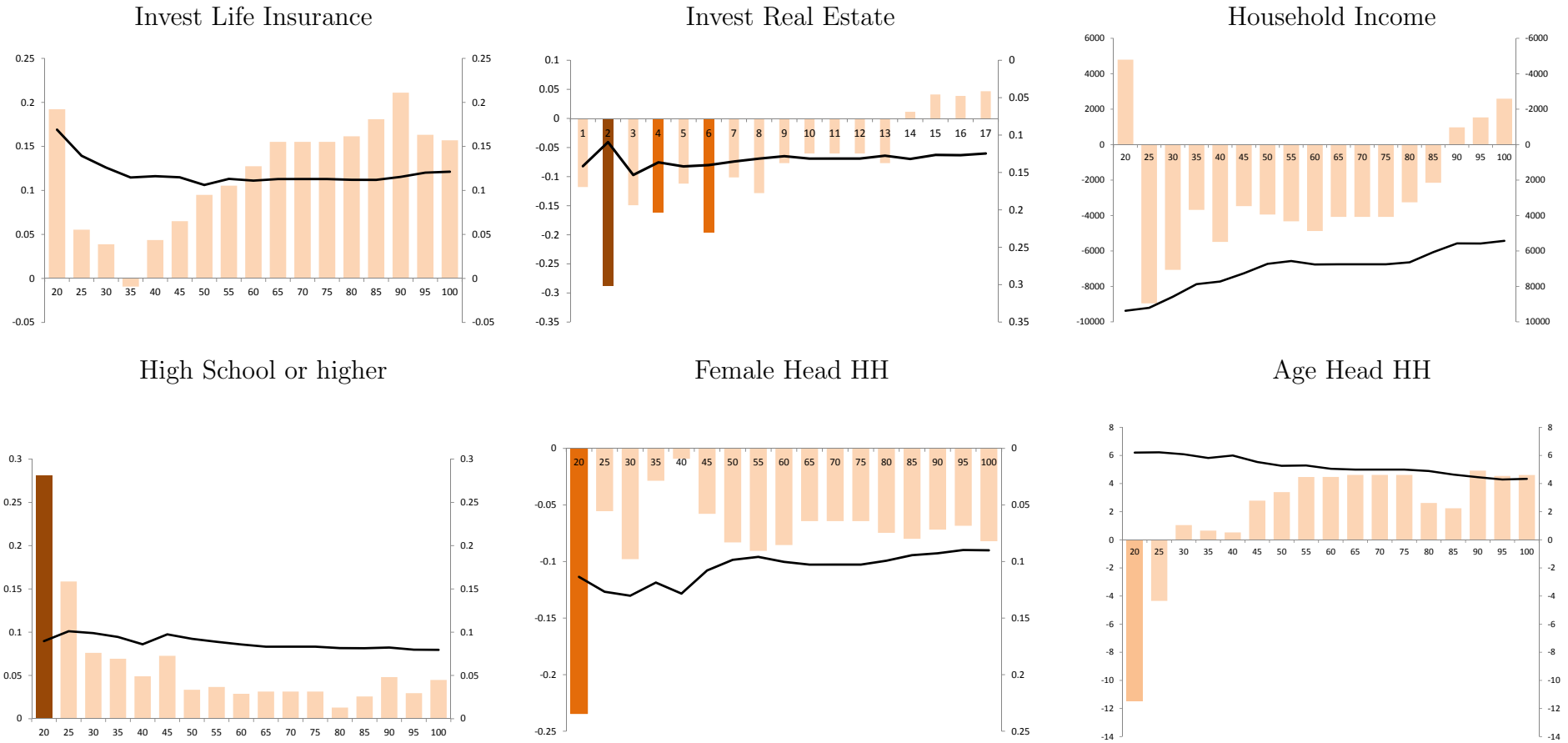


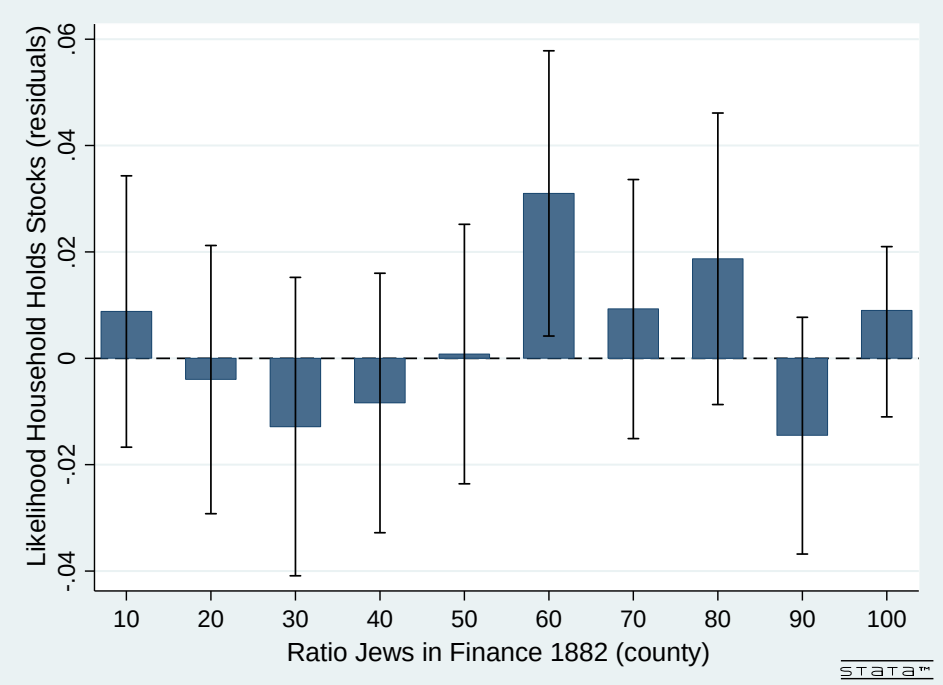
Figure 4 plots the coefficent on the variable $VoteShareNazi1933$ computed after after estimating the following OLS specifications in subsamples of households sorted by the unemployemnt rate in their county of residence as of 1933:

$$DepVar_{ik} = \alpha + \beta \times VoteShareNazi1933_k + X_i' \times \gamma + K_{ik}' \times \delta + \eta_t + \epsilon_{ik}$$

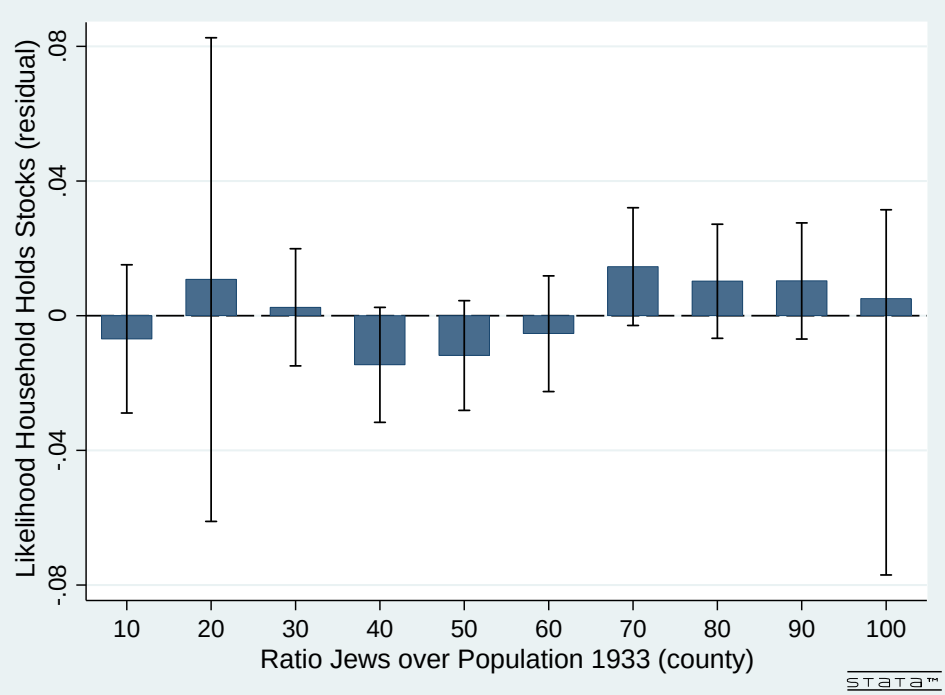
Each observation is a German household interviewed by the SOEP any time between 1984 and 2011. $DepVar$ is indicated on top of each graph. In each graph, the left vertical axis reports the OLS coefficient on $VoteShareNazi1933$, and it is associated with the histograms. The horizontal axis indicates the percentile of the distribution of counties by the unemployment rate in 1933 below which the estimation is performed. For instance, the histogram labeled "20" reports the OLS coefficient for estimating the OLS specification only for households who live in counties were the unemployment rate in 1933 was below the 20th percentile; the histogram labeled "30" reports the average marginal effect for estimating the OLS specification only for households who live in counties were the unemployment rate in 1933 was below the 30th percentile. The right vertical axis reports standard errors attached to each coefficient and clustered at the county level. They are associated with the black line. Dark brown histograms are marginal effects that are significant at the 1% level or lower; orange histograms at the 5% level; white histograms are not significant at any conventional level.

Figure 5: **Jews in Finance and Current Stockholdings**

A. Ratio of Jews in Finance in 1882 and Current Stockholdings



B. Ratio of Jews in 1933 and Current Stockholdings



The two graphs plot the average residuals from the following OLS equation:

$$HoldsStocks_i = \alpha + X_i' \times \delta + K_{ik}' \times \delta + \eta_t + \epsilon_i$$

across ten decile of the distribution of the ratio of financial employees of Jewish religion in a county as of 1882 (Panel A), and of the ratio of Jews over the total German population in a county as of 1933 (Panel B).

Table 1: **Distribution of Workers across Sectors, 1882**

	Jews	Total sector	Jews/Total sector (%)	Total sector/Overall Workers (%)
Total working population	357546	11037320	3.239	100
Unemployed			2.598	6.116
<u>Agriculture, Manifacturing, Trade</u>				
Agriculture	1641	4625893	0.035	41.911
Forestry and Fishing	65	66455	0.098	0.602
Mining and metal transf.	1255	866794	0.145	7.853
Chemical	266	28908	0.920	0.262
Textile	1724	385565	0.447	3.493
Food and Beverage	9239	363837	2.539	3.296
Building and Construction	1312	533925	0.246	4.837
Printing	555	35352	1.567	0.320
Retail trade	70175	466249	**15.051**	4.224
Bookshops, art dealers	485	9580	**5.063**	0.087
<u>Services</u>				
Engineering Services	591	146650	0.403	1.329
Health Care	1108	40883	2.710	0.370
Transportation	421	128136	0.329	1.161
Hotels and restaurants	3654	147061	2.485	1.332
Household services	692	278927	0.248	2.527
Military	918	25860	0.355	2.343
Administration	1093	119140	0.917	1.079
Finance	3042	13324	**22.986**	0.120
Insurance	223	6655	3.351	0.060

This Table reports the absolute number and percentages of workers for the municipalities included in the Prussian census of 1882. The first two columns report the absolute number of Jews and of overall workers in each sector. The third column reports the ratio of Jewish workers over that of total workers for each sector. The fourth column reports the ratio of the overall German population which is employed in each of the sectors.

Table 2: **Summary Statistics**

	All available observations					Non-missing information					p-value
	Obs.	Mean	St. dev.	Min.	Max.	Obs.	Mean	St. dev.	Min.	Max.	
Persecution of Jews											
Pogrom 1349	17818	0.576	0.494	0	1	13870	0.570	0.495	0	1	0.358
Log deported Jews	17818	4.956	2.311	0	10.93	13870	4.950	2.295	0	10.93	0.881
Ratio deported Jews	17477	34.06	19.61	0	163.5	13599	34.34	19.81	0	163.5	0.158
Principal component persecution	17477	-0.223	1.106	-3.082	3.571	13599	-0.222	1.112	-3.082	3.571	0.980
Household characteristics											
Holds Stocks	26761	0.163	0.369	0	1	13870	0.158	0.364	0	1	0.092
Homeowner	27064	0.391	0.488	0	1	13870	0.417	0.493	0	1	0.009
Has life insurance	26761	0.469	0.499	0	1	13870	0.462	0.499	0	1	0.198
Income	26761	31522	26614	-36	986400	13870	30514	27061	-36	986400	0.000
Age	21981	48.62	17.64	17	97	13870	48.49	17.58	17	96	0.371
Female	21982	0.490	0.500	0	1	13870	0.473	0.499	0	1	0.000
Single	27064	0.178	0.382	0	1	13870	0.218	0.413	0	1	0.000
High School or higher	27079	0.766	0.423	0	1	13870	0.718	0.450	0	1	0.000
County characteristics											
Log Population 1933	17818	11.57	1.371	7.94	14.29	13870	11.56	1.368	7.94	14.29	0.638
Log Jews 1933	17818	6.271	2.047	0	11.99	13870	6.261	2.041	0	11.99	0.708
Percentage unemployed 1933	17818	18.21	8.768	2.618	40.52	13870	18.01	8.838	2.618	40.52	0.028
Percentage blue collars 1933	17818	44.85	10.56	16.49	72.40	13870	44.67	10.68	16.49	72.40	0.023
Percentage self employed 1933	17818	20.01	4.211	9.096	32.74	13870	20.08	4.265	9.096	32.74	0.064
Percentage Catholics 1925	17818	40.11	32.71	0.502	98.77	13870	41.27	32.53	0.517	98.77	0.006
Eastern Germany	27079	0.129	0.335	0	1	13870	0.071	0.258	0	1	0.000
Latitude	24959	50.69	1.727	47.95	54.03	13870	50.65	1.642	47.95	54.03	0.643
Land quality index	24959	0.562	0.149	0.306	0.870	13870	0.566	0.149	0.306	0.870	0.628
Income p.c. 2005	22748	18033	2301	12846	27253	13870	18099	2220	13115	25027	0.452
College graduates 2005	24766	24.40	4.929	17.60	34.60	13870	23.55	4.463	17.60	34.60	0.010

This Table reports summary statistics for measures of the historical persecution of Jews in the German lands, and for the characteristics of households and counties where households live. Each observation is a German household interviewed by the SOEP any time between 1984 and 2011. For each variable, the table reports the number of observations for which the variable is observed, its mean, standard deviation, minimal, and maximal values. The left Panel reports statistics for households interviewed by the SOEP (repeated observations are excluded) and whose county of residence has been collected. The right Panel reports statistics for the sample of households for which we do not miss any household or county-level characteristics. The latter is the sample we will use throughout the analysis. The column on the far right reports p-values for tests of the difference in the means across the two samples.

Table 3: **Deep-rooted Antisemitism and Stock Market Participation**

	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		P.C. intensive, extensive margins of persecution			Log deported Jews *(extensive margin)*		Ratio deported Jews *(intensive margin)*		Pogroms 1349	
	Probit	Probit	Probit	OLS	Probit	OLS	Probit	OLS	Probit	OLS
Persecution of Jews		-0.011	-0.012	-0.013	-0.005	-0.006	-0.031	-0.031	-0.020	-0.020
(see columns heading)		*0.005***	*0.004****	*0.004****	*0.002****	*0.002****	*0.018**	*0.018**	*0.009***	*0.009***
Percentage unemployed 1933		-0.279	0.070	0.083	0.119	0.137	0.103	0.114	0.086	0.072
		*0.155**	*0.118*	*0.115*	*0.108*	*0.098*	*0.117*	*0.153*	*0.108*	*0.099*
Percentage Catholics 1925		0.022	0.005	0.003	-0.005	-0.003	0.003	0.001	0.001	-0.003
		*0.013**	*0.012*	*0.012*	*0.011*	*0.012*	*0.012*	*0.013*	*0.011*	*0.012*
Income	0.019		0.028	0.028	0.028	0.028	0.027	0.028	0.028	0.028
	*0.001****		*0.003****	*0.002****	*0.003****	*0.002****	*0.003****	*0.002****	*0.003****	*0.002****
Age	0.011		-0.001	-0.002	-0.002	-0.002	-0.001	-0.002	-0.002	-0.002
	*0.005***		*0.001*	*0.001*	*0.001*	*0.001**	*0.001*	*0.001*	*0.001*	*0.001**
Female	-0.016		-0.005	-0.005	-0.006	-0.006	-0.005	-0.005	-0.006	-0.006
	0.027		*0.006*	*0.006*	*0.006*	*0.006*	*0.006*	*0.006*	*0.006*	*0.006*
Single	0.184		0.054	0.050	0.053	0.050	0.054	0.050	0.054	0.050
	*0.044****		*0.010****	*0.010****	*0.010****	*0.009****	*0.010****	*0.010****	*0.010****	*0.009****
High School or higher	0.025		0.012	0.014	0.011	0.012	0.012	0.013	0.011	0.013
	0.031		*0.008*	*0.008**	*0.008*	*0.008*	*0.008*	*0.008*	*0.008*	*0.008*
Eastern Germany	-0.210		-0.031	-0.026	-0.040	-0.034	-0.032	-0.027	-0.030	-0.024
	*0.064****		*0.021*	*0.019*	*0.021**	*0.019**	*0.021*	*0.019*	*0.020*	*0.018*
Income p.c. 2005			0.039	0.044	0.028	0.046	0.042	0.047	0.033	0.038
			*0.016***	*0.016****	*0.003****	*0.016****	*0.017****	*0.017****	*0.017***	*0.017***
College graduates 2005			0.132	0.120	0.173	0.162	0.133	0.123	0.100	0.080
			0.140	*0.139*	*0.138*	*0.136*	*0.143*	*0.143*	*0.125*	*0.123*
Other Individual, historical controls	X	X	X	X	X	X	X	X	X	X
Wave groups f.e.			X	X	X	X	X	X	X	X
Current regional controls			X	X	X	X	X	X	X	X
Observations	13,870	13,599	13,599	13,599	13,870	13,870	13,599	13,599	13,870	13,870
N. of clusters	270	261	261	261	270	270	261	261	270	270
(Pseudo-) R^2	0.06	0.01	0.10	0.08	0.10	0.08	0.10	0.08	0.10	0.08

This Table reports average marginal effects computed after estimating the following probit spefication:

$$Pr(HoldsStocks_{ik}|X_i, K_{ik}) = \Phi(\alpha + \beta \times Persecution_k + X_i' \times \gamma + K_{ik}' \times \delta + \eta_t + \epsilon_{ik})$$

Each observation is a German household interviewed by the SOEP any time between 1984 and 2011. In all columns, the dependent variable is a dummy which equals one if the household holds stocks. The main covariate of interest, *Persecution*, is the measure of persecution of Jews in the past described at the top of each column. S.e. are clustered at the county level. Significance is as follows: *10%, **5%, ***1%.

Table 4: **Exclusion Restriction 1: Distance Residuals and Observables**

Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)
	Distance Trier		Distance Emmerich		Minimal distance	
Log County Income p.c.	0.009	0.011	-0.007	-0.008	0.006	0.009
	0.008	*0.009*	*0.011*	*0.013*	*0.007*	*0.008*
% College graduates	-0.096	-0.224	0.718	0.795	0.230	0.147
	0.293	*0.303*	*0.341***	*0.439**	*0.250*	*0.276*
Land Quality Index	-0.003	-0.003	-0.005	-0.005	-0.003	-0.003
	0.017	*0.020*	*0.014*	*0.019*	*0.014*	*0.016*
Female	0.001	-0.006	0.006	-0.003	0.004	-0.002
	0.012	*0.012*	*0.011*	*0.014*	*0.010*	*0.010*
Single	0.009	0.000	0.015	0.008	0.009	0.002
	0.012	*0.014*	*0.010*	*0.014*	*0.010*	*0.011*
Home owner	-0.006	-0.012	0.016	-0.000	-0.002	-0.013
	0.020	*0.023*	*0.020*	*0.025*	*0.017*	*0.019*
Log Income	0.006	0.019	-0.000	0.010	0.007	0.019
	0.023	*0.025*	*0.019*	*0.024*	*0.018*	*0.021*
Age	-0.401	-0.124	-0.630	-0.228	-0.434	-0.179
	0.634	*0.787*	*0.463*	*0.717*	*0.501*	*0.661*
High School or higher	-0.008	-0.005	-0.004	0.000	-0.007	-0.005
	0.009	*0.009*	*0.009*	*0.011*	*0.007*	*0.008*
Eastern Germany f.e.	X	X	X	X	X	X
Res. medieval	X		X		X	
Res. medieval and 1930s		X		X		X

Table 4 documents the association between the measures of distance from the Rhine Valley and current county- and individual-level observables. The measures of distance are the estimated residuals from the following OLS specification:

$$DistanceRhine_k = \alpha + K_k' \times \delta + \epsilon_k \tag{7}$$

where *DistanceRhine* is one of three measures indicated above each column, and K is a set of observables capturing the economic characteristics of counties in the past. In odd columns, K includes all observable proxies for economic growth in the Middle Ages (whether the county hosted a monthly market, a bishop siege, any free imperial city, whether it was part of the Hanseatic League, whether it was incorporated before 1349) and geographic characteristics (latitude, the land quality index of Ramankutty et al. (2002), whether a navigable river existed in the county, whether the county is in eastern Germany). In even columns, K additionally includes the log of Jewish inhabitants as of 1933 and a set of socio-demographics of counties in 1933 (unemployment rate, ratio of workers in blue-collar jobs, in self-employment, ratio of Roman Catholics). Each line reports the estimated β in the following OLS specification:

$$Depvar_{ik} = \alpha + \beta \times \hat{\epsilon_k} + C_{ik}' \times \delta + \epsilon_{ik}$$

where *Depvar* is a county- or individual-level observable enlisted in the left column for each line, $\hat{\epsilon_k}$ are the residuals from Equation 7 and C is a set current individual and county-level observables. S.e. are clustered at the county level (hence equivalent to Huber-White heteroskedasticity-robust s.e. in specifications at the county level). Significance is reported as follows: *10%, **5%, ***1%.

Table 5: **Exclusion Restriction 2: Distance and Persecution in Reduced Form**

Invest in Stocks	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Distance Trier			Distance Emmerich			Minimal distance	
Log Distance	0.014	0.005	0.002	0.017	0.009	0.009	0.017	0.009	0.008
	*0.006***	*0.006*	*0.006*	*0.007***	*0.007*	*0.007*	*0.005****	*0.06*	*0.006*
P.C. Persecution		-0.011			-0.011			-0.011	
		*0.004***			*0.004***			*0.004***	
Pogrom 1349			-0.019			-0.018			-0.019
			*0.009***			*0.009***			*0.009***
Historical contr.	X	X	X	X	X	X	X	X	X
County controls	X	X	X	X	X	X	X	X	X
Individual controls	X	X	X	X	X	X	X	X	X
Observations	13870	13426	13679	13870	13426	13679	13870	13426	13679
Adjusted R^2	0.08	0.09	0.09	0.08	0.09	0.09	0.08	0.09	0.09

Table 5 reports average marginal effects for estimating the following probit specification:

$$Pr(HoldsStocks_i|X_i, K_k) = \Phi(\alpha + \beta_1 \times DistanceRhine_k + \beta_2 \times PersecutionJews_k + X_i' \times \delta + K_k' \times \delta + \eta_t + \epsilon_{ik})$$

where $DistanceRhine$ is the measure of distance from the Rhine Valley indicated above each column, and $PersecutionJews$ is the principal component of the margins of persecution against Jews in columns (2), (5), and (8), or an indicator for counties where at least a pogrom against the local Jewish community was documented during the Black Death period. S.e. are clustered at the county level. Significance is reported as follows: *10%, **5%, ***1%.

Table 6: **Instrumenting Deep-rooted Antisemitism: Three-stage OLS**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	First stage: Existence Jewish community in 1049			Second stage: Persecution of Jews			Third stage: Holds stocks		
	Dist. Trier	Dist. Emmerich	Minimal dist.	Dist. Trier	Dist. Emmerich	Minimal dist.	Dist. Trier	Dist. Emmerich	Minimal dist.
Res. Distance Rhine	-0.136	-0.213	-0.148						
	*0.044***	*0.043****	*0.037****						
Existence Jewish comm. In 1049				1.063	1.508	1.274			
				*0.504***	*0.430****	*0.490****			
Persecution of Jews							-0.063	-0.050	-0.057
							*0.027***	*0.021***	*0.024***
Historical contr.	X	X	X	X	X	X	X	X	X
Regional controls	X	X	X	X	X	X	X	X	X
Observations	268	268	268	268	268	268	268	268	268
Adjusted R^2	0.09	0.08	0.06	0.33	0.35	0.25	0.16	0.16	0.16

This Table reports OLS coefficients for the three-stage instrumental variable procedure described in Section 4. In the first stage (columns (1)-(3)), the probability that a Jewish community existed in 1049 in each German county is predicted by the residuals of the distance of the county from the Rhine Valley regressed on all available observables for medieval economic conditions of counties. In the second stage (columns (4)-(6)), the extent of Jewish persecution during the Nazi period is predicted with the predicted probability that a Jewish community existed in a county in 1096. In the third stage (columns (7)-(9)), the ratio of households who own stocks in each county is predicted with the predicted extent of Jewish persecution. In all stages observations are German counties and coefficients are estimated with OLS. Hubert-White s.e. are reported below each coefficient. Significance is as follows: *10%, **5%, ***1%.

Table 7: **Robustness**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Aggregating at Kreis level			Excl. least educated Kreise			Excl. poorest Kreise			Excl. high unemployment 1933			Placebo Distances	
	P.C. pers.	Minimal distance	Pogrom 1349	P.C. pers.	Minimal distance	Pogrom 1349	P.C. pers.	Minimal distance	Pogrom 1349	P.C. pers.	Minimal distance	Pogrom 1349	Dist. Ulm (south)	Dist. Flensburg (north)
Persecution of Jews	-0.017	0.011	-0.026	-0.020	0.014	-0.031	-0.018	0.018	-0.028	-0.009	0.016	-0.014	-0.0067	-0.005
	*0.005***	*0.008*	*0.011***	*0.006***	*0.007***	*0.012***	*0.005***	*0.007***	*0.001***	*0.004***	*0.006***	*0.011*	*0.007*	*0.007*
Individual contr.	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Historical contr.	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Wave groups f.e.				X	X	X	X	X	X	X	X	X	X	X
Regional controls	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Observations	261	270	270	8,228	8,348	8,348	9,269	9,371	9,371	9,738	10,009	10,009	13,870	13,870
N. of clusters				164	168	168	153	156	156	208	217	217	270	270
(Pseudo-) R^2	0.23	022	0.23	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10

This Table reports average marginal effects computed after estimating the following probit spefication:

$$Pr(HoldsStocks_i|X_i, K_{ik}) = \Phi(\alpha + \beta \times PersecutionJews_{ik} + X_i' \times \delta + K_{ik}' \times \delta + \eta_t + \epsilon_i)$$

Each observation is a German household interviewed by the SOEP any time between 1984 and 2011. In all columns, the dependent variable is a dummy which equals one if the household holds stocks. *Persecution of Jews* is the measure of Jewish persecution described at the top of each column. In columns (1) to (3) we exclude households in counties in the lowest third of the distribution of college education. In columns (4) to (6) we exclude households in counties in the lowest third of the distribution of GDP per capita. In columns (7) to (9) we exclude households in counties in the highest third of the distribution of unemployment as of the rise of the Nazi party in 1933. In columns (10) and (11) we use placebo measures of the persecution of Jews to verify they do not affect stock market participation. S.e. are clustered at the county level (Kreis). Significance is as follows: *10%, **5%, ***1%.

Table 8: **Placebo Analysis: (Non-)Effect of Antisemitism on other Outcomes**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	P.C. intensive, extensive margins of persecution		Pogrom 1349		Log distance from Emmerich		IV	
Panel A. Bank branches p.c.								
Persecution	0.002	0.001	0.002	-0.001	-0.000	0.001	-0.000	-0.004
	*0.001***	*0.001*	*0.001***	*0.001*	*0.001*	*0.001*	*0.001*	*0.003*
Observations	268	267	277	276	277	276	268	267
Panel B. Home ownership								
Persecution	-0.002	-0.006	-0.011	-0.011	-0.014	0.004	0.036	-0.011
	0.010	*0.008*	*0.016*	*0.015*	*0.013*	*0.011*	*0.040*	*0.033*
Observations	17467	14169	17808	14440	17808	14440	17467	14169
Panel C. Life Insurance								
Persecution	0.006	0.009	-0.007	-0.008	0.008	0.005	-0.021	-0.014
	0.007	*0.007*	*0.014*	*0.014*	*0.010*	*0.011*	*0.034*	*0.033*
Observations	17360	14169	17701	14440	17701	14440	17360	14169
Panel D. Log Income								
Persecution	-0.035	-0.034	0.014	0.046	0.061	0.041	0.198	-0.137
	0.037	*0.033*	*0.063*	*0.063*	*0.048*	*0.050*	*0.154*	*0.143*
Observations	17310	14123	17651	14394	17651	14394	17310	14123
Panel E. Female HH head								
Persecution	-0.007	-0.004	$-6*10^{-5}$	-0.001	0.026	0.014	-0.066	-0.046
	0.005	*0.005*	*0.010*	*0.010*	*0.008****	*0.010*	*0.036**	*0.031*
Observations	14258	13599	14529	13870	14529	13870	14258	13599
Panel F. High School or higher								
Persecution	0.001	0.004	0.004	0.009	0.002	-0.001	-0.012	-0.016
	0.005	*0.006*	*0.010*	*0.012*	*0.007*	*0.009*	*0.022*	*0.091*
Observations	17477	12726	17818	12966	17818	12966	17477	12726
Individual, historical contr.		X		X		X		X
Wave groups f.e.		X		X		X		X

This Table reports average marginal effects computed after estimating the following probit spefication:

$$Pr(Outcome_{ik}|X_i, K_{ik}) = \Phi(\alpha + \beta \times PersecutionJews_k + X_i' \times \gamma + K_{ik}' \times \delta + \eta_t + \epsilon_{ik})$$

Each observation is a German household interviewed by the SOEP any time between 1984 and 2011. In all columns, the dependent variable is a dummy which equals one if the household holds stocks. *Persecution* is the measure of Jewish persecution described at the top of each column. In each Panel , we report the estimated $\hat{\beta}$ where $Outcome_i$ is the indicated individual-level dependent variable. S.e. are clustered at the county level (Kreis). Significance is as follows: *10%, **5%, ***1%.

Table 9: **Properties of the Effect: Time, Cohorts, and Education**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Effect over time	P.C. intensive, extensive margins of persecution		Log deported Jews (extensive margin)		Ratio deported Jews (intensive margin)		Pogrom 1349	
	Probit	OLS	Probit	OLS	Probit	OLS	Probit	OLS
Persecution	-0.016	-0.015	-0.005	-0.006	-0.047	-0.044	-0.025	-0.024
	*0.006***	*0.006***	*0.003**	*0.003***	*0.025**	*0.022***	*0.013**	*0.013**
Persecution*1984-1987	0.017	0.014	0.005	0.004	0.102	0.101	0.003	0.001
	0.013	*0.011*	*0.007*	*0.006*	*0.057**	*0.053**	*0.025*	*0.021*
Persecution*1988-1991	0.001	-0.001	-0.003	-0.004	0.028	0.024	0.002	0.001
	0.015	*0.017*	*0.008*	*0.008*	*0.082*	*0.089*	*0.034*	*0.038*
Persecution*1992-1995	0.004	0.003	-0.002	-0.002	0.040	0.042	0.006	0.004
	0.013	*0.014*	*0.006*	*0.007*	*0.059*	*0.064*	*0.029*	*0.033*
Persecution*1996-1999	-0.004	-0.008	0.001	0.001	-0.034	-0.059	0.010	0.019
	0.014	*0.017*	*0.008*	*0.009*	*0.088*	*0.098*	*0.029*	*0.033*
Persecution*2000-2003	0.006	0.004	-0.003	-0.004	0.018	0.001	0.023	0.029
	0.007	*0.009*	*0.003*	*0.004*	*0.042*	*0.049*	*0.019*	*0.022*
Persecution*2004-2007	0.008	0.007	0.001	0.001	0.009	0.010	0.013	0.009
	0.008	*0.007*	*0.004*	*0.003*	*0.047*	*0.040*	*0.018*	*0.017*
Panel B. Effect across cohorts								
Persecution	-0.017	-0.018	-0.006	-0.007	-0.047	-0.043	-0.026	-0.027
	*0.006***	*0.006***	*0.002***	*0.002***	*0.022**	*0.022**	*0.010**	*0.011**
Persecution*Nazi Cohort	0.011	0.012	0.007	0.007	0.085	0.084	0.031	0.033
	*0.007**	*0.007**	*0.002***	*0.002***	*0.026***	*0.027***	*0.012**	*0.013**
Persecution*Post 1965 Cohort	0.005	0.006	-0.004	-0.004	-0.068	-0.053	-0.010	-0.008
	0.008	*0.007*	*0.002**	*0.002**	*0.027**	*0.022**	*0.013*	*0.012*
Panel C. Effect by ratio college graduates								
Persecution	-0.020	-0.020	-0.008	-0.008	-0.093	-0.087	-0.029	-0.030
	*0.006***	*0.006***	*0.003***	*0.003***	*0.035***	*0.035**	*0.013**	*0.014**
Persecution*High college	0.007	0.005	0.007	0.006	0.129	0.107	-0.015	-0.014
	0.009	*0.009*	*0.004**	*0.004*	*0.056**	*0.056**	*0.018*	*0.020*
Persecution*Low College	0.013	0.012	0.004	0.003	0.069	0.063	0.024	0.024
	*0.007**	*0.007**	*0.004*	*0.004*	*0.036**	*0.036**	*0.017*	*0.017*
Individual, historical controls	X	X	X	X	X	X	X	X
Wave groups f.e.	X	X	X	X	X	X	X	X
Current regional controls	X	X	X	X	X	X	X	X
Observations	13,559	13,559	13,870	13,870	13,559	13,559	13,870	13,870
N. of clusters	261	261	270	270	261	261	270	270
(Pseudo-) R^2	0.10	0.08	0.10	0.08	0.10	0.08	0.10	0.08

This Table reports average marginal effects computed after estimating the following probit spefication:

$$Pr(HoldsStocks_{ik}|X_i, K_{ik}) = \Phi(\alpha + \beta \times Persecution_k + X_i' \times \gamma + K_{ik}' \times \delta + \eta_t + \epsilon_{ik})$$

Each observation is a German household interviewed by the SOEP any time between 1984 and 2011. In all columns, the dependent variable is a dummy which equals one if the household holds stocks. The main covariate of interest, *Persecution*, is the measure of persecution of Jews in the past described at the top of each column. In Panel A, *Persecution* is interacted with a set of dummy variables each including four waves of SOEP surveys. In Panel B, it is interacted with dummy variables for three cohorts of households: those who experienced the Nazi period, those born after the Second World War, and those born after 1965. In Panel C, it is interacted with dummy variables for three terciles of the distribution of counties based on the ratio of college graduates as of 2005. S.e. are clustered at the county level (Kreis). Significance is as follows: *10%, **5%, ***1%.

11 Appendix

We aim to estimate the following system of three structural equations:

$$Existence1096_k = \alpha_1 + \beta_1 Distance_purged_k + K_k'\delta_1 + \epsilon_{1k}$$
$$PersecutionJews_k = \alpha_2 + \beta_2 Existence1096_k + K_k'\delta_2 + \epsilon_{2k}$$
$$RatioHoldStocks_k = \alpha_3 + \beta_3 PersecutionJews_k + K_k'\delta_3 + \epsilon_{3k}$$

where $Existence1096$, $PersecutionJews$ and $RatioHoldStocks$ are endogenous variables, and $Distance_purged$ are the estimated residuals from a regression of distance from the Rhine on economic and socio-demographic observables for counties in the past.
We obtain conditions for identification of the system assuming that $Distance_purged$ is an exogenous variable. We solve the system for the implied reduced-form equation:

$$RatioHoldStocks_k = \alpha + \beta Distance_purged + K_k'\delta + \epsilon_k$$

where

$\alpha = \alpha_3 + \beta_3\alpha_2 + \beta_3\beta_2\alpha_1$

$\beta = \beta_3\beta_2\beta_1$

$\delta = \beta_3\beta_2\delta_1 + \beta_3\delta_2 + \delta_3$

$\epsilon_k = \beta_3\beta_2\epsilon_1 + \beta_3\epsilon_2 + \epsilon_3$

The implied reduced-form equation suggests the following identifying condition:

$$Cov(Distance_purged, \epsilon) = 0$$
$$\Leftrightarrow \beta_3\beta_2 Cov(Distance_purged, \epsilon_1) + \beta_3 Cov(Distance_purged, \epsilon_2) + Cov(Distance_purged, \epsilon_3) = 0$$

Since $Cov(Distance_purged, \epsilon_1) = 0$ by construction, this is satisfied by any of the two sufficient conditions below:

A) $Cov(Distance_purged, \epsilon_2) = Cov(Distance_purged, \epsilon_3) = 0$

B) $Cov(Distance_purged, \epsilon_3) = -\beta_3 Cov(Distance_purged, \epsilon_2)$.

Table 10: **Residuals from Structural Equations and Purged Distance**

	(1)	(2)	(3)	(4)	(5)	(6)
	Second stage residuals: Persecution of Jews on Existence 1096			Third stage residuals: % Hold stocks on Persecution of Jews		
	Dist. Trier	Dist. Emmerich	Minimal dist.	Dist. Trier	Dist. Emmerich	Minimal dist.
P.C. Persecution Jews	-0.053	-0.179	-0.092	-0.001	0.006	0.001
	0.115	*0.103**	*0.099*	*0.009*	*0.007*	*0.007*
Extensive Margin	-0.007	-0.067	-0.046	-0.002	0.009	0.001
	0.115	*0.106*	*0.093*	*0.009*	*0.007*	*0.007*
Intensive Margin	-1.946	-3.620	-2.463	-0.002	0.006	0.001
	2.204	*2.759*	*2.140*	*0.009*	*0.007*	*0.007*
Pogrom 1349	0.011	-0.060	-0.005	-0.001	0.010	0.002
	0.047	*0.045*	*0.041*	*0.009*	*0.007*	*0.007*
Historical contr.	X	X	X	X	X	X
Regional controls	X	X	X	X	X	X

Table 10 documents the association between the measures of distance from the Rhine Valley and the residuals from the second- and third-stage structural equations of the 3SLS system. The measures of distance are the estimated residuals from the following OLS specification:

$$DistanceRhine_k = \alpha + K_k' \times \delta + \epsilon_k$$

where $DistanceRhine$ is one of three measures indicated above each column, and K is a set of observables capturing the economic characteristics of counties in the past. Columns (1)-(3) regress the residuals from the following structural equation on the distance measures:

$$PersecutionJews_k = \alpha_2 + \beta_2 \times Exist1096_k + K_k' \times \delta_2 + \epsilon_{2k}$$

where $PersecutionJews$ is the measure of persecution enlisted in the left column for each line, and $Exist1096$ is an indicator for whether a Jewish community existed in a county as of 1096. Columns (4)-(6) regress the residuals from the following structural equation on the distance measures:

$$RatioHoldStocks_k = \alpha_3 + \beta_3 \times PersecutionJews_k + K_k' \times \delta_3 + \epsilon_{3k}$$

Hubert-White s.e. are reported below each coefficient. Significance is reported as follows: *10%, **5%, ***1%.