Deep-Rooted Antisemitism Shapes Households' Investments

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First Version: June 2013
This Version: July 2014

Abstract

We use historical anti-Jewish sentiment to proxy for current distrust in financial markets. Households in German counties where Jews were persecuted more as far back as in the Middle Ages are less likely to invest in stocks today. A one-standard-deviation increase in Jewish persecution leads to a 7.5% to 12% drop in the average stock market participation. Results are similar if the votes for the Nazi party proxy for historical anti-Jewish sentiment. 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 likelihood of early Jewish persecution. Distrust in finance reduces the demand for stocks, and has transmitted across generations independently from antisemitism: the effect of historical anti-Jewish sentiment on current antisemitism declines with education, whereas its effect on stockholdings does not.

JEL: D91, G11, J15, N33.

Keywords: Economics of Discrimination, Social Stereotypes, Household Finance, Cultural Economics, History & Finance.

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1 Introduction

For centuries, Jews have been associated with financial services. The association dates back at least to 1049,[1] when Pope Leo IX banned Christians from lending money at interest.[2] In 1882, eight hundred years after the ban, Jews accounted for 3% of all German workers but 23% of the financial sector workforce. At the same time, Europeans have persecuted Jews since the Middle Ages. They have treated Jews as alien and enemies, especially during economic crises and pandemics like the Black Death of 1349. Hatred against Jews was tragically reflected in the *Final Solution*, arguably the most criminal Nazi policy.

In this paper, we consider the heterogeneity of historical violence against Jews across German counties (*Kreise*) to proxy for current distrust in financial markets. Distrust in finance is a channel for limited stock market participation proposed by Gennaioli, Shleifer, and Vishny (2014a) and Gennaioli, Shleifer, and Vishny (2014b). Figure 1 depicts our main finding: current inhabitants of counties where Jewish persecution was higher invest on average less in stocks. A one-standard-deviation increase in the county-level Nazi persecution is associated with a 1.2-percentage-point lower likelihood that individual households invest in stocks today, which is 7.5% of the average participation in our sample. Households in counties that experienced at least one pogrom during the Black Death of 1349 are 2 percentage points less likely to hold stocks today, which is 12% of the average participation.

Testing for a causal effect of Jewish persecution on current stockholdings is a major challenge because persecution and investment decisions realized centuries apart. Our identification strategy exploits the forced migrations of the first Jewish communities across

[1] The human capital accumulated by Jews since the second century may have facilitated their sorting into trade and finance well before 1049 (Botticini and Eckstein (2005) and Botticini and Eckstein (2012)).

[2] The legal basis for the ban was 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 by Charles the Great, 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.

the German lands to predict the likelihood of Jewish persecution in the past.[3]

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. The Crusades and 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. The diffusion has taken several centuries (see Figure 3), which may be due to transportation costs and to the slow spatial diffusion of migration networks (e.g., see Spitzer (2014)). The timing of forced migrations suggests a three-stage instrumental variable strategy. In the first stage, the distance of a German county from the Rhine Valley is used to predict the probability that a Jewish community existed before the Black Death of 1349. In the second stage, the estimated probability of existence of a Jewish community is used to predict the extent of Jewish persecution in each county. In counties with no Jewish communities, the probability of observing acts of violence against Jews must be zero, simply because no targets for such violence existed. By contrast the mere presence of a Jewish community before 1349 makes the probability of Jewish persecution positive.[4] In the third stage, we use the estimated likelihood of Jewish persecution to predict current stockholdings in each county. These three-stage least squares estimates confirm the negative effect of Jewish persecution on current stockholdings. The reduced-form and IV results survive a series of robustness and placebo tests.

A desirable property of our distance instrument is that the Rhine Valley was the cradle of Germany's industrialization and is still highly economically developed. If the distance captured economic forces that trigger stock market participation, we should observe a negative correlation between distance and participation, which goes in the opposite direction of the effect we document. Moreover, consistent with Buggle (2013),

[3]A direct test for the effect of distrust in finance on stock market participation could exploit a shock to distrust. Stock market crashes or banking crises may represent such shocks. But these shocks also affect other determinants of the decision to invest in stocks (e.g., Guiso, Sapienza, and Zingales (2013)).

[4]We will not *necessarily* observe a pogrom in counties where a Jewish community existed. Our results are similar if we only look at counties with documented Jewish communities before 1349 (see Table 2). The latter results do not exploit cross-county variation in the existence of communities, which may raise concerns of endogenous location choices by Jewish communities.

the distance from the Rhine Valley has a strong negative association with generalized trust, which positively affects stock market participation (Guiso, Sapienza, and Zingales (2009)). To support the exclusion restriction, we show the distance from the Rhine Valley does not predict current outcomes that affect stock market participation. The distance has no autonomous association with stock market participation when added to the measures of Jewish persecution in reduced form specifications. As a placebo test, we estimate the association between the distance from the Rhine Valley and the stockholdings of current French households, who live to the west of the Rhine: Ashkenazi Jews migrated eastwards, and not to the west where crusaders were already persecuting pre-existing Jewish communities. If the distance captured dimensions related to the spatial distribution of development and unrelated to Jewish migrations, it should be associated with French stockholdings as well as German stockholdings. The distance is not associated with the stockholdings of French households.[5]

Our identification strategy captures the variation in past Jewish violence across counties that hosted Jewish communities or not. Hence it cannot differentiate between counties that hosted communities and had pogroms and counties that hosted communities but had no pogroms. These two types of counties may react differently to anti-Jewish sentiment over time (e.g., Adena et al. (2013)). To further reduce concerns that our analysis captures common determinants of historical Jewish violence and current stockholdings unrelated to the existence of communities in the past, we propose an estimation that only exploits the variation in Jewish persecution across counties at a similar distance from the Rhine Valley. This estimation confirms our results.[6]

As an alternative source of variation of historical anti-Jewish sentiment, we exploit the county-level votes for the Nazi Party. Antisemitism was a major ingredient of the Nazi ideology. But the support for the Nazis also reflected Germans' discontent with their economic condition (e.g., de Bromhead, Eichengreen, and O'Rourke (2013)). Votes for the Nazis should be a better proxy for anti-Jewish sentiment in counties that

[5]We thank Sonya Lim for suggesting this placebo test.

[6]We thank Nico Voigtlaender and Ruben Enikolopov for inspiring this test. We also find that the effect of Nazi persecution on current stockholdings is larger in counties that hosted a community and had at least one pogrom, as opposed to counties that hosted a community but had no pogrom.

were less affected by the economic crisis. 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. There is no effect in counties where unemployment was high (Figure 4).[7] The variation of the Nazi electoral support does not explain other current outcomes, and the spatial variation of votes for other parties is unrelated to current stockholdings.

After having documented the negative effect of Jewish persecution on current stock market participation, we study the demand- and supply-side channels that mediate the effect. Demand-side channels may operate through the lower demand for stocks by households. Supply-side channels may operate through the lower supply of investment opportunities to households in counties where past violence against Jews was higher.

On the demand side, we first test if deep-rooted anti-Jewish sentiment does predict current distrust in financial markets. We run our own survey on a sample of German households, and ask them how comfortable they feel with having part of their wealth invested in stocks.[8] Those living in counties with higher historical violence against Jews trust the stock market significantly less than others (Table 8).

Then we consider three alternative demand-side channels. First, deep-rooted antisemitism may capture a set of household characteristics that we label "backwardness." Backwardness includes cultural cues and beliefs that make suspicious anything that is not familiar. Second, current households may still associate the stock market with Jews, be antisemitic, hence invest less in counties where historical and current antisemitism are higher. Third, current distrust in finance could be a cultural legacy of deep-rooted anti-Jewish sentiment. Distrust in finance may have transmitted across generations independently from antisemitism. Even if households do not consciously associate Jews with finance anymore, they may still distrust finance more in counties where deep-rooted

[7]This argument does not imply that unemployed voters supported the Nazi party more on average. In fact, King et al. (2008) find those Germans most hit by the economic crisis but without a high risk of unemployment were an important constituency for the Nazi party in 1933.

[8]The survey is administered by *ClickWorker*, an online platform similar to the US-based *mTurk* (see Kuziemko et al. (2013)) on a stratified sample of the German population that signs up to the platform to perform tasks and surveys for pay. The respondents only know they are part of a survey, and they ignore the identity or scopes of the researchers. This procedure avoids that survey results are invalidated by demand effects. We thank Stefano Della Vigna and Noam Yuchtman for inspiring this test.

anti-Jewish sentiment is higher.

Any attempt to disentangle these channels using the main effect is hopeless, because all three channels predict lower stock market participation in areas where deep-rooted anti-Jewish sentiment was higher. Instead we look at the heterogeneity in a dimension along which the channels have different predictions: education. More educated individuals are less prone to backwardness, and are less antisemitic today (Glaeser (2005), Voigtlaender and Voth (2013a)). We replicate these findings with novel micro-data from the *Friedrich Ebert Stiftung* that allow to additionally show more educated individuals do not associate Jews with the stock market today. But our main effect and the current distrust in finance do not vary across education levels. This result is inconsistent with generic backwardness or current antisemitism. It is consistent with a cultural norm of distrust in finance that has transmitted indipendently from antisemitism over time. Education curricula affect pupils' ideology and shape cultural norms (Cantoni et al. (2014)). German curricula do address the issue of antisemitism, which allows for antisemitism to wash away with schooling. But curricula seldom address the benefits of stock market participation. A cultural norm that hindered stock market participation would hardly wash away with schooling.

We also study two supply-side channels: the diffusion of credit unions and local saving banks after 1843,[9] and the depletion of the human capital needed to run banks efficiently after Jewish persecutions.[10] These two channels do not appear to explain the results.

Finally, we investigate the properties of the effect of deep-rooted antisemtism on current investments. First, we ask if the effect fades away over time. Voigtlaender and Voth (2012) show that anti-Jewish sentiment at the local level has persisted for six centuries. The effect of Jewish persecution on the likelihood of holding stocks is stable from 1984 to 2011. Second, we test if the size of the effect differs across cohorts. We find that the size of the effect is similar for all cohorts except those born before 1945. The massive antisemitic propaganda of the Third Reich may have eliminated the variation of

[9]We thank Joachim Voth for encouraging this analysis, which is inspired by the results of Pascali (forthcoming).

[10]We thank Ulrike Malmendier for inspiring this test.

antisemitic beliefs across German counties. But the experience of a major economic crisis may also drive the non-effect (Malmendier and Nagel (2011), Giuliano and Spilimbergo (forthcoming)). We interact cohort dummies with the unemployment rates in counties as of 1933. Results are consistent with the experience argument: the non-effect on the cohort born before 1945 is driven by those who live in counties that experienced the highest unemployment rates during the 1930s.

Our paper makes four main contributions. First, it is one of the few that documents a negative long-run effect of discrimination on wealth through the intergenerational transmission of a cultural norm.[11] Second, we contribute with three novel data sources to investigate the channels that mediate such long-run effect: i) we run our own survey to measure the current distrust in finance across Germany; ii) we use unique data collected by the *Friedrich Ebert Stiftung*, which document the current association between Jews and financial markets next to other dimensions of backwardness for current Germans; and iii) we collect novel data on the diffusion of German credit unions in the 19^{th} century, a phenomenon that is of interest of economic historians by itself. Third, we construct a novel test for the long-run effects of the Nazi ideology on economic outcomes using the spatial variation in the motives for supporting the Nazi Party. Finally, we propose an identification argument for the emergence of past anti-Jewish violence, which contributes to explain the current spatial diffusion of antisemitism analyzed by the seminal work of Voigtlaender and Voth (2012) and Voigtlaender and Voth (2013a).[12] Our argument complements Anderson, Johnson, and Koyama (2014), who exploit the variation in temperature within cities over time to predict the emergence and disappearance of Jewish expulsions over the centuries. We instead want to explain the cross-sectional variation in the first acts of violence against Jews in a county, hence we cannot exploit any time-varying

[11] Taste, statistical, and implied discrimination in settings such as the labor market (e.g., Bertrand and Mullainathan (2004), Reuben et al. (2014)) 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 descendants of discriminators give up a wealth-increasing opportunity that would not necessarily be enhanced by the discriminated population, but for instance, by non-Jewish brokers.

[12] Note that our identitfication argument explains the variation in past violence between counties that hosted no communities and those that hosted a community, but it does not explain the emergence of violence within the group of counties that hosted a Jewish community in the past.

dimensions at the city level in our analysis.

2 Related Literature

Our paper contributes to the literature on the effects of culture and of social identity on economic outcomes. Banfield (1958) and Putnam (1993) argue that social capital accumulates over time and affects political and economic development. Tabellini (2010) shows 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 economic growth in the United States. Guiso, Sapienza, and Zingales (2004) show that social capital positively affects stock market participation, illiquid investments, and the use of checks. As for social identity, Bordalo, Gennaioli, and Shleifer (2014) model stereotypes as recalling a group's distinctive trait relative to other groups. Benjamin, Choi, and Fisher (2013) test for the effect of religious identity on economic behavior in a controlled environment, whereas Campante and Yanagizawa-Drott (2014) exploit the idiosyncratic occurance of Ramadan across countries to identify the effects of religious practices on growth and happiness. Inspired by these papers and by Akerlof and Kranton (2000), a nascent strand of finance looks at the effects of social identity stereotypes on financial decisions: Han and Hirshleifer (2013) model the effect of visibility bias on consumption, whereas D'Acunto (2014a) studies the effect of gender identity stereotypes on individual investment decisions. We contribute to these fields by proposing a proxy for households' distrust in stock markets based on the historical sterotypical association of Jews with finance, and we test for its causal effect on investment decisions.

Our work also belongs to the literature that exploits historical natural experiments and the persistence of social phenomena to explain current economic outcomes recently surveyed by Spolaore and Wacziarg (2013) and Nunn (forthcoming). Giavazzi, Petkov, and Schiantarelli (2014) study the cultural traits that persist and those that fade away over time. 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. D'Acunto (2014b) documents that historical literacy predicts

the current amount of education of the blue-collar workforce across European regions, and studies its effect on innovation and investments. This strand of literature departs from the field Economic History insofar as it *directly* explains current-day economic and financial decisions using natural experiments from the past and persistent social phenomena. To stress this difference, an appropriate label for this nascent field would be "History & Economics", and for financial outcomes "History & Finance."

The paper also relates to the limited stock market participation puzzle. Paxson (1990) and Guiso et al. (1996) show theoretically and empirically that households' background risk may explain their limited investment in stocks. 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), labor income risk (Betermier et al. (2012), Berk and Walden (2013)), and corporate scandals (Giannetti and Wang, 2014). In this paper, we show that deep-rooted anti-Jewish sentiment reduces the likelihood that households invest in stocks.

Finally, we add to the literature on the consequences of Jewish persecution on economic outcomes. Voigtlaender and Voth (2012) explain Jewish persecution during the Nazi period with the town-level variation of Jewish persecution in the Middle Ages. 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 investigate the effect of anti-Jewish sentiment on households' current investments, and the channels that drive this economic effect. Second, we propose a novel identification strategy for the early emergence of Jewish persecution.
Acemoglu, Hassan, and Robinson (2011) find that Russian areas where Jews had been deported are less developed today. Jews were overrepresented in the middle and upper classes, which acted as the engine of economic growth after the Second World War.[13] Pascali (forthcoming) finds that Italian towns where Jews were expelled have a less developed banking system today. In towns where Jews provided financial services,

[13]Although unrelated to Jews, Chaney and Hornbeck (2013) also exploit the historical expulsion of a discriminated minority to study the long-run impact of a labor supply shock on economic outcomes.

Catholic financial institutions to compete with them were founded. We show that our findings are not driven by supply-side channels related to societal composition or to the evolution of financial institutions. Waldinger (2010) and Akbulut-Yuksel and Yuksel (forthcoming) show the Nazi expulsion of Jewish instructors at the university and lower levels affected the educational outcomes of students in the short and long run. All our analyses keep the ratio of Jewish inhabitants before 1933 constant, and our effects do not differ across counties with different shares of pre-1933 Jewish inhabitants or Jews in the financial sector. Grosfeld, Rodnyansky, and Zhuravskaya (forthcoming) show that households in towns with more Jews before the Holocaust are more antisemitic, and support the market economy less. We build on their argument to identify the early diffusion of antisemitism across German counties.

3 Measuring Historical Jewish Persecution

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

Intensive and extensive margins of persecution. The first and second measures relate to the persecution of Jews during the Nazi period. These measures 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 deported to a concentration or extermination camp after 1933 from each German county. This measure increases with the size of Jewish communities within a county, hence it is higher for counties with large cities. The *intensive* margin, namely, the relative extent of Jewish persecution, is the ratio of deported Jews over the total Jewish population in a county as of 1933, when the Third Reich began. 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.

Black Death pogroms. The third measure, *Pogrom 1349*, is a dummy that equals 1 if a county experienced at least one anti-Jewish pogrom during the Black Death around 1349.

The Black Death was arguably the worst pandemic in human history: up to one third of Europeans may have died. A deep economic and social crisis followed, and historians refer to this period as one of the darkest eras in the history of humanity. Unsubstantiated theories on the origins of the pandemic diffused all over Europe. Accusations against Jews, the most influential minority group in 14^{th}-century Europe, were common and often led to mass murders and persecution, especially in the German lands. Voigtlaender and Voth (2012) find that pogroms during the Black Death predict the extent of Jewish persecution during the Nazi period at the town level. Different from Voigtlaender and Voth (2012), we do not exploit the number of pogroms (intensive margin) experienced by a county. The intensive margin is certainly meaningful in a city-level analysis, because cities that experienced more pogroms are likely to have developed a stronger anti-Jewish sentiment in the past. But in our county-level analysis dictated by the level of resolution of the stockholding data, observing more or less pogroms in a county is largely driven by the number of cities in the county for which data on pogroms are available. The number of pogroms in a county for which we observe many cities may be high even if each city only had one pogrom in the past. Hence we would mistakenly attribute high levels of past anti-Jewish sentiment to counties that had no different levels of anti-Jewish sentiment than counties for which we happen to observe historical data for less cities.

Principal component of measures. We also use the first principal component of the three measures described above. The first principal component isolates the common variation across the three alternative margins of persecution. For this reason the principal component is our preferred measure of Jewish persecution in the past, but we report results using all measures throughout the paper.

Data Sources. We collect data from four sources. The characteristics of German households are from the *Socio-Economic Panel* (SOEP) run by the *Deutches Institut fuer Wirtschaftsforschung Berlin* (DIW). The SOEP data have already been used in economics (e.g., see Fuchs-Schuendeln and Schuendeln (2005) and Burchardi and Hassan (2013)). The SOEP has conducted interviews on a yearly basis since 1984. 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. A drawback of the SOEP data is they do not provide the complete financial portfolios of households. We use the observed investment categories to proxy for households' overall wealth, and to validate our measures of distrust in finance, but we cannot document how deep-rooted antisemitism affects every single component of households' financial portfolios. Households in areas where past anti-Jewish violence was higher are less likely to invest in stocks today. But we cannot say if those households are more likely to keep their money in deposits or to invest in other safe assets.

We use the data on Jewish persecution in the Middle Ages and during the Nazi period collected by Voigtlaender and Voth (2012), which also include medieval controls from Cantoni and Yuchtman (forthcoming). We aggregate all city-level data at the level of current counties, which is the finest geographical level we can observe current households' characteristics. We obtain county-level historical characteristics from the *Ifo Prussian Economic History Database*, described in detail by Becker et al. (2014). The data are aggregated at the level of current counties. Finally, we collect current county-level controls: socio-demographics from *DeStatis*, the index of land quality of Ramankutty et al. (2002), and the latitude and longitude of the centroid from *Eurostat*.

Table 1 reports summary statistics at the household level.[14] In the left Panel we use all available observations. In the right Panel we only look at households with non-missing information. The far-right column reports p-values for testing the null hypothesis that the means across the samples are equal. The full sample of households in the SOEP county-level dataset includes 29,680 observations. The county of residence is not available for 2,655 households. We are also missing county-level historical information for 9,207 households.[15] The remaining missing observations 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 without missing information. On average, 57% of households live in counties where at least

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

[15]All results are similar if we attribute values of historical variables from neighboring counties to those for which we miss information.

one pogrom happened during the Black Death. 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.

The ratio of stock-owning households across counties and over time is about 16% in both samples. The average age of the person who makes financial decisions is 49 years. The two samples do not differ in the geographic characteristics of the counties. They are similar with respect to the average population size and the average number of Jews in 1933. But households in the running sample are more likely to be homeowners (42% vs. 39%), and they have lower self-reported income (30,500 euros vs. 31,500 euros). The SOEP survey does not ask households for an estimate of their overall wealth. We use income and homeownership to proxy for wealth. Households in the running sample are less likely to be headed by a woman. Their heads are more likely to hold a college degree and less likely to hold a high school degree.[17] Households in the running sample live in counties with lower unemployment and less blue-collar workers in 1933, and a higher proportion of Catholics in 1925. They are less likely to live in Eastern Germany, and they live in counties with a lower ratio of college graduates in 2005.

Data Properties and Baseline Correlations. Figure 2 depicts the properties of Jewish persecution and current stock market participation at the county level. Panel (a) and (b) of Figure 2 show the spatial distribution of the ratio of Jews deported during the Nazi Period and of the average ratio of households who own stocks from 1984 to 2010. In both maps, the darker a county, the higher the value of the variable. The data are not available for blank counties. Jewish Persecution was higher in Western counties. Stock market participation is higher in the south and in the north. As expected participation is much lower in Eastern Germany, which suggests that we average out the differences between east and west in the analysis. Panel (c) and (d) of Figure 2 plot the densities of the

[16]The ratio exceeds one only in 1 county (Viersen, in North Rhein-Westphalen). Excluding Viersen from the analysis keeps all results unchanged.

[17]The average ratio of household heads who declare to hold a college degree across counties from 1984 to 2010 is extremely low. For robustness, we replicate all the results using a dummy for holding a high school degree or higher levels of education, and all results are similar.

ratio of Jews deported and current stock market participation.[18] The distributions spike around their mean values of 34% and 16% (see Table 1). Panel (e) plots the correlation between the ratio of deported Jews and the average ratio of households who own stocks from 1984 to 2010 at the county level in the raw data. The two dimensions are negatively correlated (-0.13, p-value=0.03). Panel (f) of Figure 2 shows the average participation across counties with and without pogroms during the Black Death. While participation is on average higher in counties with no pogroms, a t-test for the difference of the two means does not reject the null that the means are equal. In the Online Appendix, we plot the densities of Jewish persecution for counties within German states (*Bundeslaender*). On average the persecution was higher in states closer to the Rhine Valley than those in Southern, Northern, and Eastern Germany. In the Online Appendix, we also plot the unconditional correlations between stockholdings and all the other measures of Jewish persecution.

4 Jewish Persecution and Current Stockholdings

In the baseline analysis we estimate the association between the historical Jewish persecution and current stock market participation by German households from 1984 until 2010. The following is our most general specification:

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

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_{ik} and K_{ik} are sets of controls at the household and county levels, respectively. $Income_deciles$ is a set of dummies indicating the deciles of the income distribution the household belongs to,[19] and Φ is the standard normal cdf. η_t are a set of survey-wave

[18] We have excluded Viersen, where the ratio of deported Jews exceed 100%.

[19] All the results are virtually identical if we include polynomial terms of income instead of deciles. As discussed in the previous paragraph, we do not observe a measure of the overall wealth of households in the SOEP sample. Income and homeownership serve as proxies for the overall wealth.

group fixed effects, each capturing a group of four adjacent years.[20] Standard errors of average marginal effects are computed using the delta method. We allow for correlation of unknown form across residuals at the county level, because attributing county-level measures to each household induces a mechanical correlation of residuals across households in a same county.

Nazi Persecution of Jews. Table 2 reports the average marginal effects for our baseline specification. In columns (1)-(4), $Persecution_k$ is the first principal component of the three measures of 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.

In column (2), we add individual-level controls, a dummy that equals 1 for households in Eastern Germany, current socio-demographics at the county level, and survey-wave group fixed effects. A one-standard-deviation increase in persecution (1.016) is associated with 1.22-percentage-point lower stock market participation. The effect is statistically significant and economically large. It amounts to 7.5% of the average participation. The effect is about one fifth the size of the effect of holding a college degree on the likelihood to invest in stocks. Higher education is one of the largest determinants of stock market participation.

As expected, households with higher income and singles are more likely to invest in the stock market. Households in Eastern Germany invest less in stocks. We do not find a statistically significant association of gender of the household head on stockholdings after we control for other socio-demographics. In column (3), we estimate the marginal effect in a linear probability model. The parametric assumptions do not drive our results.

In columns (5)-(10), we look separately at the extensive and intensive margins of persecution. A one-standard-deviation increase in the logarithm of the Jews deported (2.31) is associated with a 1.16-percentage-point lower likelihood that a household invests in stocks (columns (5) and (6)). In columns (8) and (9), a one-standard-deviation increase in the ratio of deported Jews over the total Jewish population in 1933 (0.192) is associated with a 0.63-percentage-point drop in the likelihood that a household invests in stocks.

[20] Results do not change if we make the survey-wave fixed effects coarser or finer. Because we exclude repeated observations across survey waves, there is no time variation at the household level.

Statistical significance is weaker when we look at the intensive margin. The sample size is smaller, because we exclude households in counties with no Jewish population as of 1933.

Pogroms in 1349. In columns (11) and (12), we measure the Jewish persecution at the county level as the occurrence of one or more pogroms during the Black Death of 1349. In both the probit and the linear specifications, households that live in counties with at least one pogrom are on average 2 percentage points less likely to hold stocks. The size and statistical significance of the effect of individual- and county-level observables on current stockholdings are comparable to our previous specifications.

Counties with documented Jewish communities. In columns (4), (7), (10), and (13) of Table 2 we estimate Equation 1 on the subsample of counties with a documented Jewish community in 1349. The restriction reduces the sample size by 27%. It allows to verify that the results are not driven by counties with no documented communities, for which the data may be lost. This subsample analysis confirms our baseline results.

Robustness. In Table 3, we test the robustness of the main results. In columns (1) and (2), we estimate the baseline specification at the county level. This test addresses the concern that individual observations in a county may be spatially correlated in a way that clustering the standard errors at the county level does not properly account for. The magnitude and significance of coefficients are similar to the household-level analysis. In columns (3) and (4), we only exploit variation in Jewish persecution within German states (*Bundeslaender*). This specification is quite demanding, because states only include on average 15 counties. The baseline magnitudes of the effect are unchanged, although we have noisier estimates when using the dummy for pogroms in 1349. In columns (5) and (6), we only exploit variation within occupation zones after the Second World War. Voigtlaender and Voth (2013b) show that the processes of denazification promoted by different occupants had different effects on the persistence of antisemitic beliefs across Germany. These zones do not exactly overlap with the current boundaries of German states, especially for the state of Baden-Wuerttemberg, which is close to the Rhine. The baseline results are replicated. In columns (7) and (8), we exclude households in counties in the bottom third of the distribution of income per capita, whereas in columns (9) and (10) we exclude households in counties in the bottom third of the ratio of college educated

inhabitants. The estimated coefficients are similar to our baseline analysis.
In summary, all measures of historical Jewish persecution are negatively associated with the likelihood that a German household invests in the stock market today.

5 Early Jewish Migrations and Persecution

Unobservable characteristics of German counties may have jointly determined the occurrence of pogroms in the Middle Ages and current stockholdings. Controlling for historical socio-demographics and geographical characteristics of counties alleviates the concern, but it is not sufficient to establish a causal effect of Jewish persecution on current financial decisions. Ideally we would exploit plausibly exogenous variation in the extent of Jewish persecution across German counties. Because the heterogeneity of anti-Jewish sentiment has persisted since the Middle Ages, the exogenous variation should be measured before the Black Death of 1349.
Figure 3 depicts our identification strategy, which exploits the staggered forced migrations of Ashkenazi Jews out of the Rhine Valley after the 11^{th} century. In Figure 3, the darker a county is, the older the first Jewish community documented in the county. Blank counties are those with missing data. The earliest Jewish presence in the German lands was found in the cities of Trier, along the Mosel, and Cologne, along the Rhine. Archaeologists date this presence to the ninth century. There is evidence of Jewish communities in the 10^{th} century along the entire Rhine Valley.[21] The Jewish population in other areas of current Germany was sparse before the 11^{th} century (Engelman (1944)). At the onset of the Crusades, Jewish communities were persecuted by Christian knights traveling to the Holy Land. Several towns on the Rhine expelled Jews, causing a massive Jewish migration towards Eastern, Northern and Southern Germany. Evidence of sizable Jewish communities dates back to the late 13^{th} and 14^{th} centuries in Munich (south) and Berlin (east) (Toch (2012)). Migrations accelerated with the Black Death.[22] In Figure 3, the Rhine river is sketched as a light blue line in Western Germany. The red straight lines

[21] We refer to Toch (2012) as a comprehensive economic history of European Jews in the Middle Ages.
[22] Only in the 15^{th} century did Ashkenazi Jews merge with the communities of Khazar origin who had moved from the Black Sea to current Poland. See van Straten (2004) for archaeological evidence and Elhaik (2012) for genetic-based evidence.

are *isodistance curves*, that is, points that lie at about 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 determined the existence of Jewish communities at the time of medieval persecutions. 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 probability of existence to predict the extent of Jewish persecution. The rationale is as follows: in counties with no Jewish communities before the Black Death, violence against Jews cannot have emerged, because no targets for such violence existed. In counties where early Jewish communities existed, the probability of a historical pogrom against the local Jews is strictly positive due to the mere presence of Jews, even if of course we we will not necessarily observe a positive realization, that is, a progrom in all these counties ex post. In the third step, we use the predicted probability of anti-Jewish violence at the county level to predict the current likelihood of stock investments at the household and county levels.

We base the 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 migratory paths of Ashkenazi Jews were not systematically determined by unobservables that also determine current stockholdings. Below, we discuss some evidence to support the exclusion restriction needed to validate our instrument.

Distance from the Rhine Valley. We consider three measures of the distance of a county from the Rhine Valley. They are the residuals ($\hat{\epsilon_k}$) 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_k is a set of dummy variables for whether the county hosted a monthly market, a

[23]The gradual diffusion would be untenable in recent periods, especially during the Nazi period with mass media and pervasive propaganda.

bishop siege, any free imperial city, any Hanseatic League city, any city 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). We compute the minimal Euclidean distance of a county from the city of Trier, on the Mosel. The shortest distance is about 2 km, whereas the greatest distance is 1100 km. We use the minimal Euclidean distance from the town of Emmerich at the north end of the German Rhine as a second measure. The lowest of these two distances is our third measure.

Identification concerns. Two major concerns are that (i) some unobserved dimensions correlated with the distance from the Rhine Valley may have determined both the Jewish persecution and past conditions that explain current stock market participation, and (ii) an economic shock close to the Rhine Valley after the Black Death may explain our results. In both cases, our instrument would be invalid. We propose a set of results to support the exclusion restriction. In Table 4, we regress several county- and individual-level observables that correlate with stock market participation on our distance measures. Different from customary reporting, each entry refers to a different regression. We enlist the outcome variables in the left column and report the main covariate at the top of each column. In odd columns, we use the residuals for the three distance measures regressed on the medieval characteristics of counties as in Equation 2. In even columns, we also condition on observables at the county level in 1933, before the start of the Third Reich. Most distance residuals are not significantly associated with 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. Two exceptions stand out: (i) the ratio of college graduates at the county level is positively associated with the residuals on the distance from Emmerich. But 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, but we detect no statistical significance.

In Panel A of Table 5, we estimate the reduced-form effect of the distances on the ratio of households that own stocks when the distances enter as regressors instead 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 OLS regressions. In columns (1), (4), and (7), all three distances are positively associated with the likelihood that households hold stocks. Once the measures of Jewish persecution enter the reduced form specifications, the estimated autonomous associations of the distances with current stockholdings drop in magnitude. The estimated standard errors for each coefficient stay virtually identical. In Panel B of Table 5, we propose a placebo test for an association of the distance from the Rhine Valley and current stockholdings through channels different than Jewish migrations. We look at the effect of the distance on the likelihood that French households own stocks. Data for French households are from the *Enquete Patrimoine* run by the Banque of France in 2004. If the distance from the Rhine captures anything peculiar to the spatial diffusion of development, we should observe an effect of the distance on the stockholdings by French households who live to the West of the Rhine. Across all our measures, we find no association between the distance from the Rhine and the current stockholdings of French households.

In the Appendix, we provide additional evidence to support the exclusion restriction. We derive the conditions for identifying our three-stage OLS system presented below and detect no correlation between the instrument and the residuals from the structural equations.

3SLS results. Table 6 reports results for estimating the stages described above in a three-stage OLS framework (see Becker and Woessmann (2009)) at the county level:[24]

$$Community1096_k = \alpha + \beta \times LogDistanceRhine_k + K_k' \times \delta + \epsilon_k \tag{3}$$

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

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

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)-(3) of Table 6 report

[24]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.

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 (0.69) reduces by 9 percentage points the likelihood that an early Jewish community existed in a county. This figure is 27% of the average likelihood that a county hosted a Jewish community in 1096 (33%), the date before the Black Death for which we have data on most counties. Estimated magnitudes when using the distance from Emmerich and the minimal distance are -13 and -11 percentage points.[25] Hence the farther a German county is from the Rhine, the less likely a Jewish community existed 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. The magnitudes are similar when we predict the likelihood of a Jewish community with the other measures of distance. Columns (7)-(9) of Table 6 document the third stage. Consistent with the reduced-form results, an increase in the instrumented persecution of Jews significantly 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 3).

Within-distance-group estimation. Our IV strategy relies on the distance from the Rhine to predict the exogenous component of the variation in the existence of Jewish communities in the past. We have discussed evidence to support the exclusion restriction, but we cannot ultimately test whether it holds. In particular, our strategy does not differentiate between counties that hosted a Jewish community and had pogroms in the past and counties that hosted a Jewish commmunity but had no acts of violence against them in the past. We propose an additional estimation that only exploits the variation of Jewish persecution across counties with a similar distance from the Rhine Valley. In the terminology of Figure 3, we exploit the variation in Jewish persecution across counties lying on the same isodistance curve. This strategy does not exploit the quasi-exogenous variation in the existence of Jewish communities in the past. We identify the effect of

[25]The standard deviations of the log distances from Emmerich and the minimal distance are 0.62, and 0.77.

Jewish persecution only through its variation across counties that are close in space, hence plausibly similar along several geographical, historical, and socio-demographic characteristics.[26] In Table 7, we estimate variations of the following equation:

$$Pr(HoldsStocks_{ik}|X_{ik}, K_{ik}) = \Phi(\alpha+\beta\times Persecution_k+X'_{ik}\times\gamma+K'_{ik}\times\delta+\eta_t+\eta_s+\epsilon_{ik}) \quad (6)$$

where all the variables are defined in Equation 1 except for η_s. We sort the counties based on their distance from the Rhine Valley, and split them in 5, 8, or 10 equal-size bins. η_s is a set of dummies that identify a group of counties with similar distance from the Rhine Valley. Panels (a),(b), and (c) of Table 7 depict the size of the distance groups, marked by alternate colors, in longitude-latitude space. Each point is a county. There is a trade-off between the number of groups and sample size. The higher the number of groups, the more similar the distance from the Rhine of counties within the groups. But the higher the number of groups, the smaller the samples whose variation drives the results, as indicated in Table 7. In all specifications, the results are qualitatively similar to those in Table 2. The estimated size of the coefficients is generally lower than in Table 2, which is expected under our interpretation that the distance provides variation in the existence of early communities, hence of Jewish persecution, which the within-distance-group estimator neuters by construction. The drop in size does not affect the conclusions except for the ratio of deported Jews during the Nazi period, whose association with current stockholdings becomes statistically indistinguishable from zero. In untabulated results, we also look at the effect of the intensive and extensive margins of Nazi persecution only for counties that hosted a Jewish community *and* had pogroms in 1349, which should be the most homogenous in terms of the determinants of deep-rooted anti-Jewish sentiment. This subsample includes 7,906 households, which is about one half of the full sample of households. For probit specifications analogous to columns (7) and (10) of Table 2, we estimate a coefficient of -0.005 (s.e. 0.002, p-value <5%) for the extensive margin of Nazi persecution, and of -0.048 (s.e. 0.019, p-value <2%) for the intensive margin of Nazi persecution.

[26]The specification still includes an Eastern Germany fixed effect.

6 Anti-Jewish Ideology, Nazi Votes, and Stockholdings

So far, we have used indirect measures of historical anti-Jewish sentiment based on past violence against 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. Voting choices were 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 included other elements on top of 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 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. It is important to stress that the argument does not imply the Nazis had higher support, on average, in counties with higher unemployment,[27] but it exploits different motives to support the Nazis *within* the group of Nazi voters in 1933.

We therefore expect a negative association between Nazi vote shares in 1933 and current

[27] In fact, King et al. (2008) find voters hit by the economic crisis but without a high risk of unemployment supported the Nazi party in 1933.

stockholdings in counties where unemployment was low in 1933, and a less negative effect in other counties. We find exactly this pattern. In Figure 4, 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 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.

Placeboes. The media affect hatred-based voting behavior. DellaVigna et al. (forthcoming) show that the nationalistic content on the Serbian radio increased the anti-Serbian hatred of Croatians who could access the radio signals, hence their likelihood to vote for Croatian nationalistic parties. For the Nazi period, Adena et al. (2013) show 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. Once the Nazis obtained the control of the radio, it had a large positive effect on their vote share (March 1933). In Figure 5 Panel (a), we find a virtually identical pattern as in Figure 4 for the September 1930 elections. For these elections we have detailed data at the county level. The elections 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.

We also propose a placebo analysis to corroborate our interpretation of the evidence. Panels (b) and (c) of Figure 5, show that the pattern for Nazi votes is not replicated when we look at votes for the social democrats and the communists in 1933. Whereas the economic motives to vote for these parties might be similar as for the Nazis, these parties should have not attracted ideological antisemitists more than the Nazis. Panels (d), (e), and (f) of Figure 5 show that conditional on the unemployment in 1933, the share of votes for the Nazi party in a county is not significantly associated with current outcomes. It is uncorrelated with the likelihood that households invest in life insurance products or real estate. It is also not significantly associated with current households' income. In unreported graphs, we find no significant associations if using education, gender, or age of the household head as alternative placebo outcomes.

7 Channels Mediating the Effect of Jewish Persecution on Stockholdings

We now move on to discuss the channels through which the historical Jewish persecution may affect the likelihood that current inhabitants invest in stocks. We consider demand- and supply-side channels. Demand-side channels mediate the negative effect of deep-rooted anti-Jewish sentiment on current stock market participation through the current demand for stocks by households. Supply-side channels mediate the negative effect through the lower supply of financial investment opportunities to households in counties where violence against Jews was higher.

Validation of the proxies for distrust in finance. We first need to validate our proxies and test if deep-rooted anti-Jewish sentiment indeed predicts current distrust in financial markets. We do not observe distrust in finance for the households in our sample. For direct validation, we run our own survey on a sample of 1,000 German households asking them how comfortable they feel with having part of their wealth invested in stocks.[28] Survey respondents scroll a slider that runs from 0 to 100 at increments of one unit. The number

[28]The design of a survey to elicit the amount of trust in finance is inspired by the Kellogg-Booth Financial Trust Index, which is the first survey-based evidence on trust in finance as described by Sapienza and Zingales (2012).

they pick is their measure of trust in financial markets.[29] The survey is administered by the company *ClickWorker* on a stratified sample of the German population that sign up to the platform to perform tasks and surveys for pay. The respondents only know they are part of a survey, and they ignore the identity or scopes of the researchers. This procedure is crucial to avoid that survey results are invalidated by demand effects. We also ask for some demographics including the zip code, which we map into counties. In Figure 12 of the Online Appendix, we report the average levels of trust across gender, age, and education level of respondents. In the analysis below, we restrict the sample to individuals in counties for which we have data on historical anti-Jewish sentiment, and counties for which we have at least 5 respondents.[30] The average trust in the full sample of respondents (40.91), and in the restricted sample (40.99) are almost identical. The two-sided p-value for a t-test of the difference of the averages across samples is 0.88.

In Panel A of Table 8 we estimate a tobit specification at the individual-respondent level, because our measure of trust in finance is censored at 0 and 100. In column (1) the unconditional correlation of trust in financial markets and the principal component of the measures of deep-rooted antisemitism is negative, although we detect no statistical significance. In columns (2)-(4), we add individual respondent's characteristics, current and historical county characteristics, and we limit the variation within *Laender* (German states). In all three specifications, a one-standard-deviation increase in deep-rooted antisemitism reduces trust in finance by about 3 percentage points, which is 7.3% of the average trust. The magnitude of the effect is in line with the effect of the principal component of deep-rooted antisemitism on current stockholdings, which amounts to 7.5% of the average likelihood to hold stocks in the SOEP sample. The similarity of the magnitudes of the effects is important to corroborate that distrust in finance can drive the effects not only qualitatively, but also quantitatively. In Panel B of Table 8 we depict the negative unconditional correlation of deep-rooted antisemitism and current trust in financial markets when averaging our measures at the county level.

[29]The screenshot of the survey is in Figure 12 of the Online Appendix.

[30]Results are similar if we set the threshold above or below five. Estimated standard errors increase with lower thresholds, consistent with the fact that we add noisier information. Our main specifications use a threshold because we do not find the data representantive of the level of trust in a county for counties where we have too few respondents.

The direct validation of our proxy for distrust in finance is qualitatively and quantitatively consistent with our interpretations. At the same time, the sample on which we run the survey is not the same sample for which we observe stockholdings. This is why in Table 9 we run a placebo analysis to validate our proxies indirectly on the sample whose stock market participation we observe. In Panel B of Table 9, we test if Jewish persecution affects the decision to invest in real estate. Like stocks, real estate is a more illiquid investment than deposits. Contrary to stocks, households do not necessarily need to interact with financial sector operators when buying a house.[31] 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. In Panel C of Table 9, 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. Under our interpretation, we should see no effect of antisemitism on the propensity of households to invest in life insurance, and indeed we do not.[32] Anti-Jewish sentiment may correlate with broader distrust toward the market economy (Grosfeld et al. (forthcoming)). Households in areas we identify as antisemitic may, for ideological reasons, dislike the accumulation of wealth. In Panel D of Table 9, 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 current income of households. Moreover, socially conservative counties may be more antisemitic. Social conservatism may make households suspicious of non-traditional forms of investing. We estimate the effect of Jewish persecution on the likelihood that a female member of the household is in charge of financial decisions. More socially conservative households would arguably tend to claim men are in charge. We do not find any effect of Jewish persecution on the likelihood that a woman is declared in charge of financial decisions

[31]Several households apply for mortgages when buying a house. But the realtor often mediates the mortgage negotiations between the household and the bank. In addition, around 70% of German mortgages are provided by local credit unions and government-owned banks.

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

within the household (Panel E). Finally, deep-rooted antisemitism may be correlated with intergenerationally-transmitted lower cognitive abilities. Lower cognition may make households less financially literate or aware of stock investments. In Panel F of Table 9, we test for an effect of historical anti-Jewish violence on the likelihood that the household head has at least a high school degree, because we do not observe a direct measure of cognitive abilities. We fail to detect any effect of anti-Jewish violence on the education level of household heads.

Demand-side Channels and Education. We consider three demand-side channels. First, deep-rooted antisemitism may capture households' "backwardness", which includes cultural cues and beliefs that promote distrust toward the unfamiliar. Second, current households may explicitly associate the stock market with Jews and be antisemitic, and thus invest less in stocks. Third, distrust in finance could be a norm produced by deep-rooted anti-Jewish sentiment. It may have transmitted across generations independently from antisemitism. Even if most households do not associate the financial sector with Jews anymore, they distrust stock market investments more in counties where deep-rooted anti-Jewish sentiment was higher. We cannot disentangle these three channels based on the main effect: all three channels predict lower stock market participation in counties with higher deep-rooted anti-Jewish sentiment. We exploit the heterogeneity of households by education, a dimension for which the channels have alternative predictions. More educated households are less prone to backwardness, and are less antisemitic today (Glaeser (2005), Voigtlaender and Voth (2013a)). In Panel B and Panel C of Figure 6, we replicate the drop in backwardness and antisemitism for current German households using novel data from the *Friedrich Ebert Stiftung*, whose characteristics and properties are described in Decker, Kiess, and Braehler (2012). We use these unique data because they allow for the first time to observe if current households think Jews have too much influence in financial markets (left graph of Panel B of Figure 6). The association of Jews with the stock market decreases with education similarly to other measures of antisemitism and backwardness. But the distrust in finance does not decline monotonically with education, as we show in the left graph of Panel A of Figure 6.[33]

[33]To make patterns easily comparable, we have defined the measure of distrust as (100-Trust)/20, where Trust is the survey-based measure of trust in finance from the previous paragraph.

The effect we detected could be explained by backwardness or current antisemitism only if the size of the effect decreased with the education level of households. But the main effect and the current distrust in finance are strinkingly similar across education levels. In Table 10, we test for the interaction of our baseline effect with the level of education of household heads. Panel A of Table 10 shows that the baseline effect of Jewish persecution on current stockholdings is not attenuated for college-educated household heads. The same holds true if we test for differential effects of college and high school education.[34] In Panel B of Table 10, we run the interaction analysis for the ratio of college graduates in a county. We interact our baseline measures of persecution with dummies capturing counties in the top and the bottom third by the ratio of college graduates. The effect of antisemitism on stockholdings does not vary systematically with the average education level. Whereas this path is not consistent with generic backwardness or current antisemitism, it is consistent with a cultural norm of distrust in finance that has transmitted indipendently from antisemitism over time, a norm that does not vary across education levels as shown in Panel A of Figure 6. Education curricula affect pupils' ideology and shape cultural norms (Cantoni et al. (2014)). German curricula do address the issue of antisemitism, hence allow for antisemitism to wash away with schooling. Instead, education curricula seldom address the benefits of stock market participation. A cultural norm that hinders stock market participation would hardly wash away with schooling, as we oberve in the data.

Supply-side Channels. We also study two supply-side channels. Starting in 1843, credit unions (*Volks-* and *Raiffeisenbanken*) have diffused across Germany. They were and still are specialized in financing local businesses and collecting households' savings. If credit unions had diffused more and earlier in more antisemitic areas, current households in those areas would be less likely to invest in stocks. We collect novel data on the foundation dates of credit unions across German counties from the proprietary registry of the *Hoppenstedt Firmendatenbank*.[35] Panel A of Figure 7 describes the diffusion path

[34]This result also differentiate the channel of distrust in finance from the generalized trust of Guiso et al. (2004), whose effect is driven by the least educated.

[35]The registry reconstructs the chains of mergers and acquisitions over the decades for currently-existing German banks. They collect the foundation date, the type, and other characteristics of any entity involved in these chains as far back as any information is retraceable.

of credit unions at four points in time. Before 1860, few credit unions existed in the current southern state of Baden-Wuerttemberg and in the eastern states of Brandenburg and Sachsen-Anhalt. In the following fifteen years, credit unions spurred in the south and center of the country, and a few banks were founded in the west. As of 1900, credit unions had diffused in the whole west and the whole south, as well as the north and the center of current Germany. Finally, the last picture shows that credit unions can be found in almost every German county today. The foundation of banks in the east and south, and their subsequent gradual diffusion towards west is inconsistent with the patterns of early anti-Jewish violence. In Panel B of Figure 7, we plot the year when the first credit union is documented in a county against the ratio of deported Jews over the 1933 Jewish population at the county level. The two dimensions are not negatively correlated.

An alternative supply-side channel refers to the skill structure of counties after the persecution of Jews. Because a large share of finance workers were Jewish, persecution may have hindered the development of local financial services at the county level. In this case, the availability of financial services today should be lower in counties where the Jewish persecution was higher. But in Panel A of Table 9 there is no effect of persecution on current private bank branches per capita at the county level. If the effect was driven by the depletion of human capital needed to run financial institutions, the effect should be larger in counties where the ratio of financial sector workers who were Jews was higher. However, in Panel A of Figure 8 we find no associaton of the ratio of Jews in finance as of 1882 on current stockholdings. In Panel B, we proxy the ratio of Jews in finance as of 1933 with the ratio of Jews in the total population of a county in 1933, and similarly we find no association between the main effect and the ratio of Jews in a county.

8 Properties of the Effect of Jewish Persecution on Stockholdings

Finally, we investigate the properties of the effect of deep-rooted anti-Jewish sentiment on current stock market participation.

Effect over time. First, we ask whether the magnitude of the effect varies over time.

The SOEP data allow 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 of the "dot-com bubble" and the subsequent burst, and the financial crisis of 2008.

In Table 11, we replicate the estimation of Equation 1 adding a set of interactions between each measure of Jewish persecution and wave-group dummies capturing periods of four years.[36] All other controls are the same as in Table 2. 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 faded away over time, we would find that the coefficients on the interaction terms are significantly negative. Their magnitude should decrease monotonically over time. If the effect was stable over time, the coefficients attached to the interaction terms would be indistinguishable from zero. In all the columns of Panel A of Table 11, the effect of Jewish persecution for the period 2008-2011, as captured by the baseline coefficient, is similar to the baseline effect in Table 2 in terms of magnitude and statistical significance. None of the coefficients attached to the interaction terms differ significantly from zero.

Effect across cohorts. 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 of the Nazis.

In Panel B of Table 11, we test how the impact of deep-rooted antisemitism on financial decision-making varies across cohorts. We divide the households into three cohorts based on the year the household head was born: the *Nazi-period cohort* includes households whose head was born before 1945;[37] the *Postwar cohort* includes those born between 1945 and 1965; the *Post 1965 cohort* includes those born after 1965. Averaging out time effects is crucial, because the Nazi-period cohort observations are concentrated in the first half of the survey waves, whereas the Post 1965 observations in the second half. The average stock market participation is substantially different across the two periods. The effects we estimate for the Postwar cohort are similar to the baseline estimates in

[36]Results are the same if we change the number of years covered by each dummy.

[37]If we define the cohorts using different time cuts, results are similar.

Table 2 in terms of magnitude and statistical significance. The effect disappears for households in the Nazi-period cohort. Two explanations seem plausible. The massive antisemitic propaganda of the Nazis may have eliminated the cross-sectional variation of anti-Jewish sentiment across counties.[38] Also, the Nazi-period cohort witnessed economic crises and social disruptions. Malmendier and Nagel (2011) show cohorts that experienced the Great Depression are less likely to invest in stocks. The effect of antisemitism on financial decision making could be of second order compared to personal experiences of large business-cycle fluctuations. To disentangle the two hypotheses, we exploit the cross-sectional variation of the depth of the economic crisis in the 1930s at the county level. We look at the stock market participation of Nazi-period cohort households sorted by the county-level unemployment rate in 1933. We split 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' cohort and with the dummies for the severity of unemployment. The reversal of the effect of antisemitism for the Nazi-period cohort is driven by households in counties where the level of unemployment was high. The estimated coefficient on the interaction between the Nazi-period cohort and persecution is significantly positive (0.021, s.e. 0.008), but the coefficient on the triple interaction of Nazi-period 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-period 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. The coefficients on the triple interactions are noisier for other measures of persecution.

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

[38]This explanation would be inconsistent with the 600-year-long persistence in the heterogeneity of anti-Jewish sentiment documented by Voigtlaender and Voth (2012).

9 Conclusions

We exploit the variation in historical Jewish persecution 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. We confirm the results by instrumenting for Jewish persecution with the distance of a county from the early settlements of Ashkenazi communities. We find similar results when proxying for anti-Jewish sentiment with the electoral support for the Nazis. The magnitude of the effect is stable from 1984 until 2011 and across cohorts, except for those who experienced the Nazi period.

Potential supply-side channels do not mediate the effect of Jewish persecution on current stockholdings. The results are not driven by current antisemitism, by a conscious association of Jews with finance, or by general backwardness of households. Distrust in finance has transmitted across generations independently from antisemitism: the effect of Jewish persecution on current stockholdings does not vary across education levels.

Because the anti-Jewish sentiment is a cultural trait transmitted across generations, these results contribute to the interdisciplinary debate on hatred beliefs and their consequences on societies. The results 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 the economic wealth of the broader population. The results may suggest an economic paradigm to justify policies against hatred beliefs. Starting with Aristotle, the public debate has condemned hatred beliefs based on the violation of 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. To the contrary, promoting the economic wealth of society is universally accepted. Cultures that do not promote human rights may be persuaded by evidence that hatred beliefs reduce not only the economic wealth of the discriminated, but of the discriminators as well, hence ultimately of the whole society.

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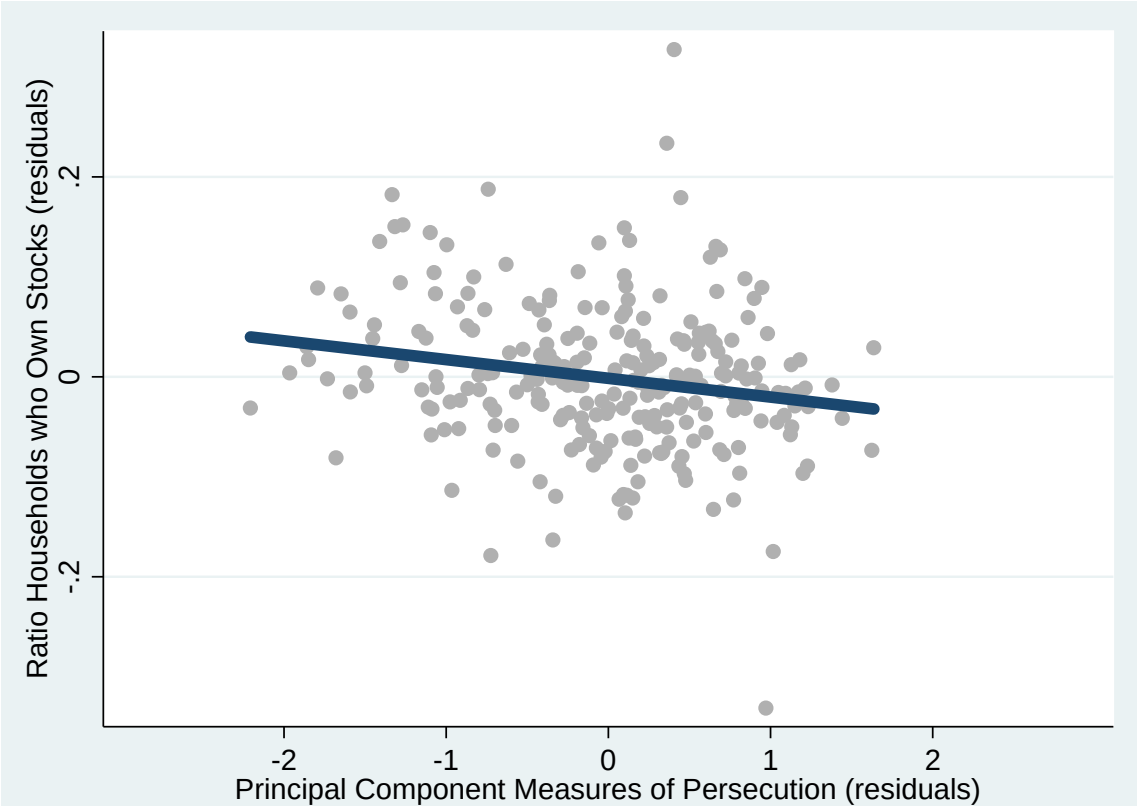
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Figure 1: **Historical Persecution of Jews and Current Stock Market Participation across German Counties**



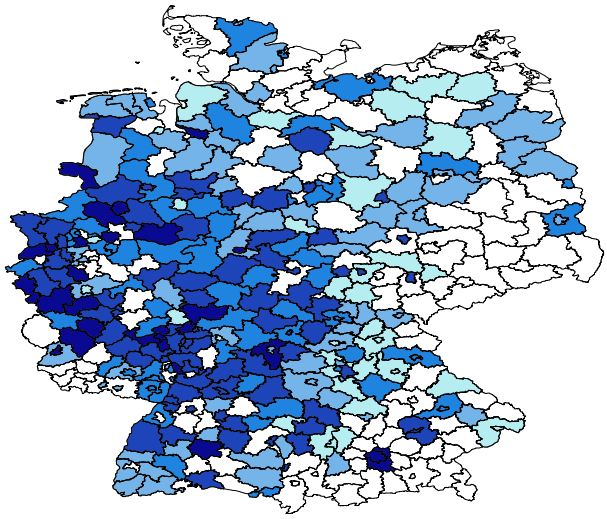
Each point is a German county. The vertical axis plots the residuals from estimating the following equation:

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

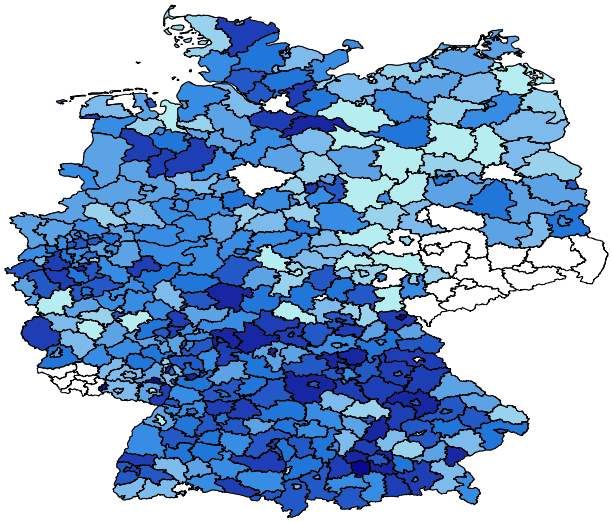
where K_k is the set of county-level observables of Equation 1 in the paper, against the residuals of a regression of the principal component of the measures of Jewish persecution on the same set of covariates, K_k.

Figure 2: **Data Properties: Persecution and Stock Market Participation across German Counties**

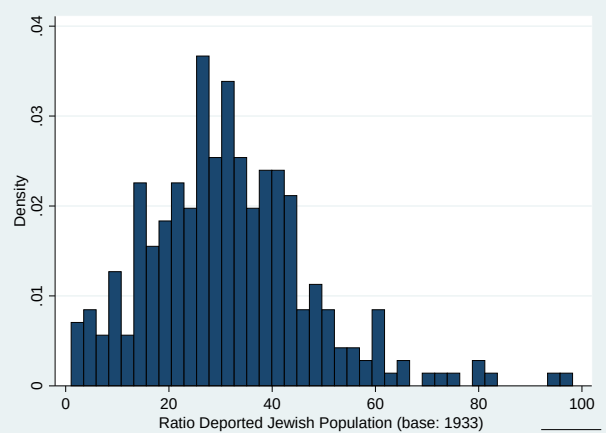
(a) Jewish Persecution Nazi Period



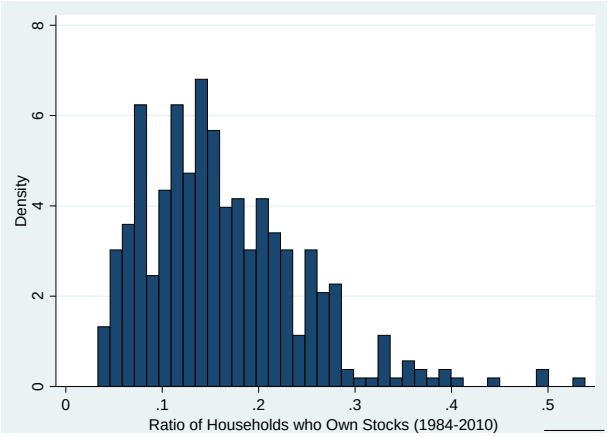
(b) Stock Market Participation



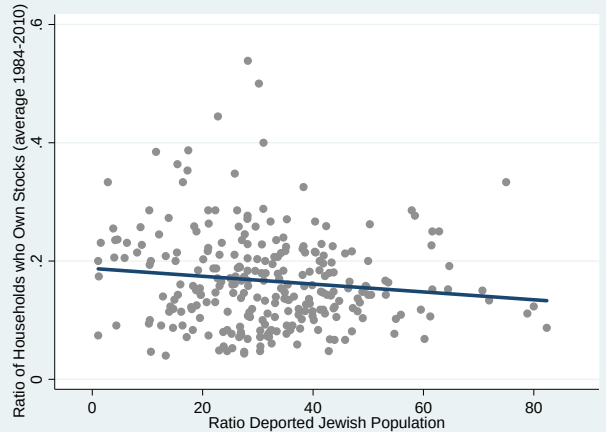
(c) Density Jewish Persecution



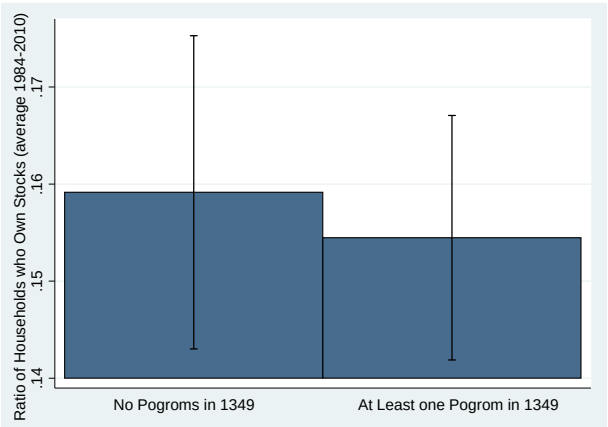
(d) Density Stock Market Participation



(e) Persecution and Participation

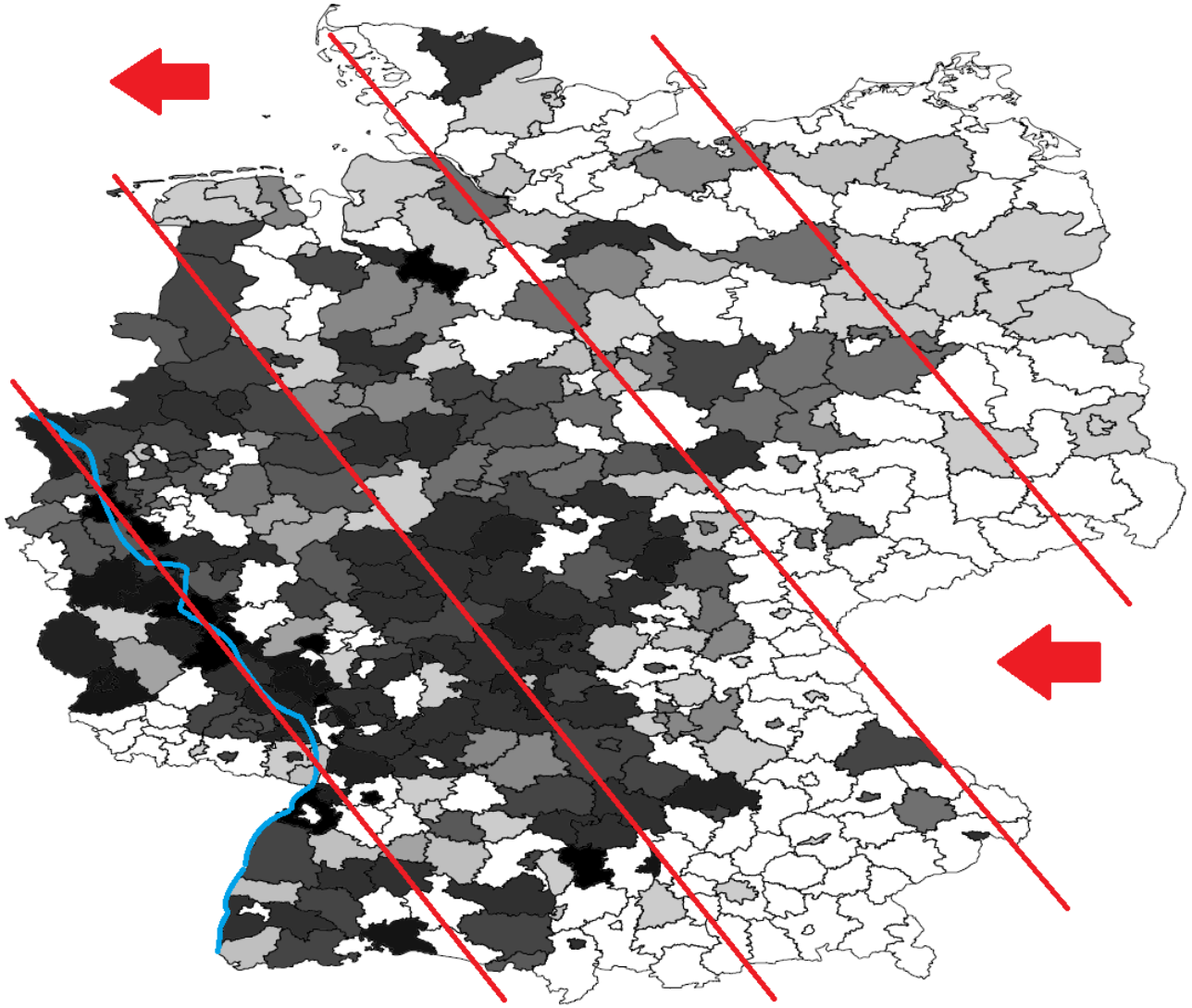


(f) Pogroms and Participation



In Panels (a) and (b), the darker a county is, the higher the value of the depicted variable. Blank counties are those for which the data are not available. Panel (a) plots the ratio of Jews deported during the Nazi period over the total Jewish population in 1933. Panel (b) plots the average yearly ratio of households who have invested in stocks from 1984 to 2010. Panels (c) and (d) plot the sample distributions of the same measures as above. Panel (e) depicts the unconditional correlation between stock market participation and the ratio of Jews deported across German counties. Panel (f) shows the mean stock market participation in counties that experienced and did not experience a pogrom in 1349.

Figure 3: **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 Rheinland-Pfalz and Nordrhein-Westfalen. 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 4: **Nazi Votes, Economic Crisis, and Stock Market Participation**

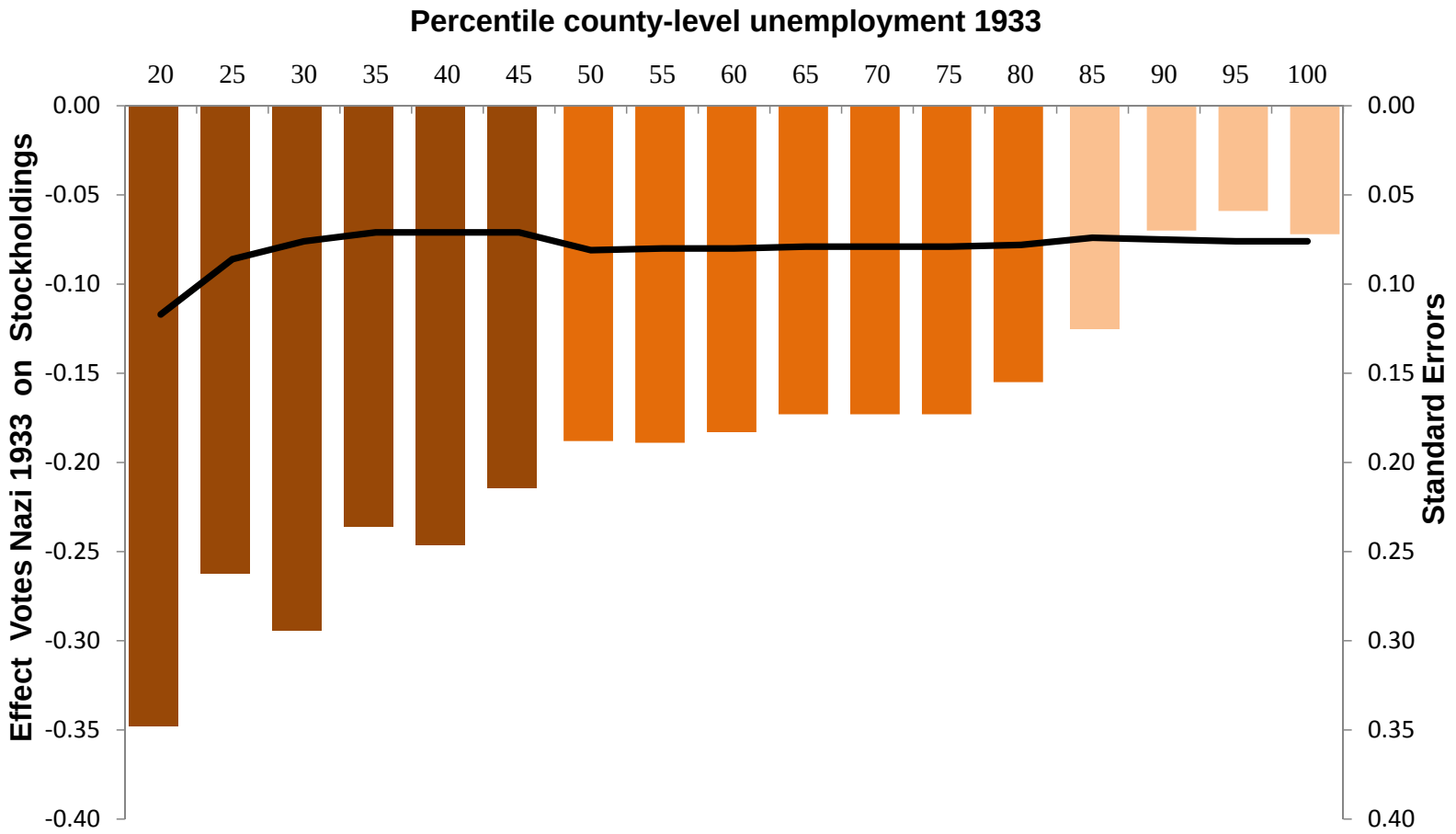


Figure 4 plots the average marginal effects of the vote share for the Nazi Party in 1933 when estimating the following probit specification across subsamples of households sorted by the unemployment rate in their county as of 1933:

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

Each observation is a German household interviewed for the first time by the SOEP 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 for households in counties where the unemployment rate in 1933 was below the 20th percentile; the histogram labeled "30" reports the average marginal effect for estimating the probit model for households in counties where 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 5: **Economic Crisis and Stock Market Participation: Alternative Elections and Placebo Outcomes**



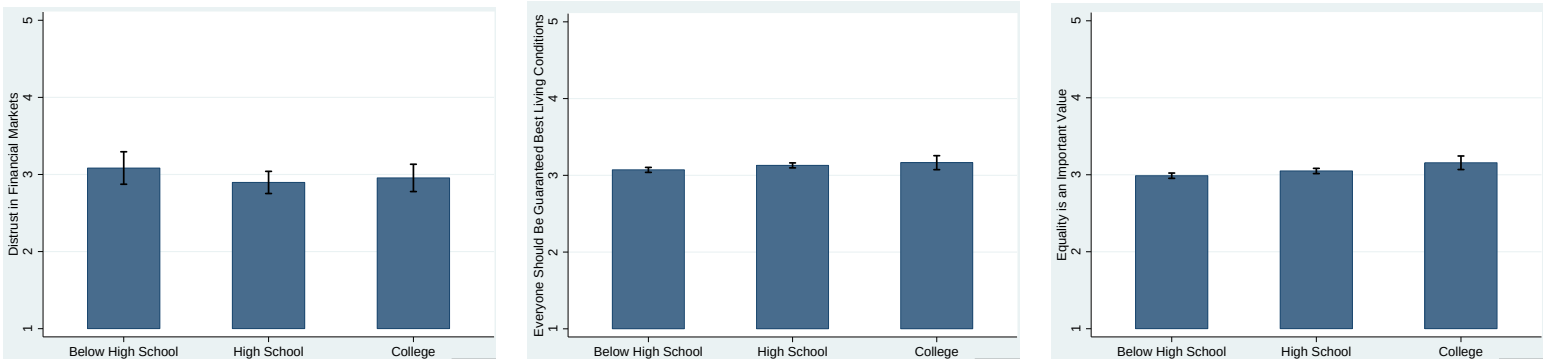
Figure 5 plots the average marginal effects of the vote share for the Nazi Party in 1933 on several outcomes computed from the following OLS specifications across subsamples of households sorted by the unemployemnt rate in their county of residence as of 1933:

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

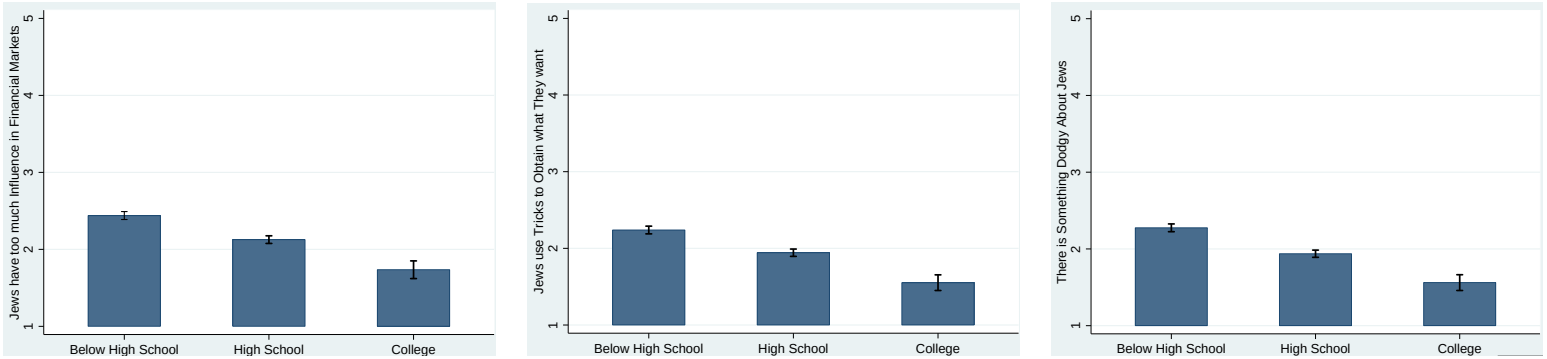
Each observation is a German household interviewed by the SOEP any time between 1984 and 2011. $DepVar$ is indicated on top of each graph. In each graph, the left vertical axis reports the OLS coefficient on $VoteShareNazi1933$, and it is associated with the histograms. The horizontal axis indicates 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 where 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 where 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 6: **Distrust in Finance, Antisemitism, and Backwardness by Education Levels**

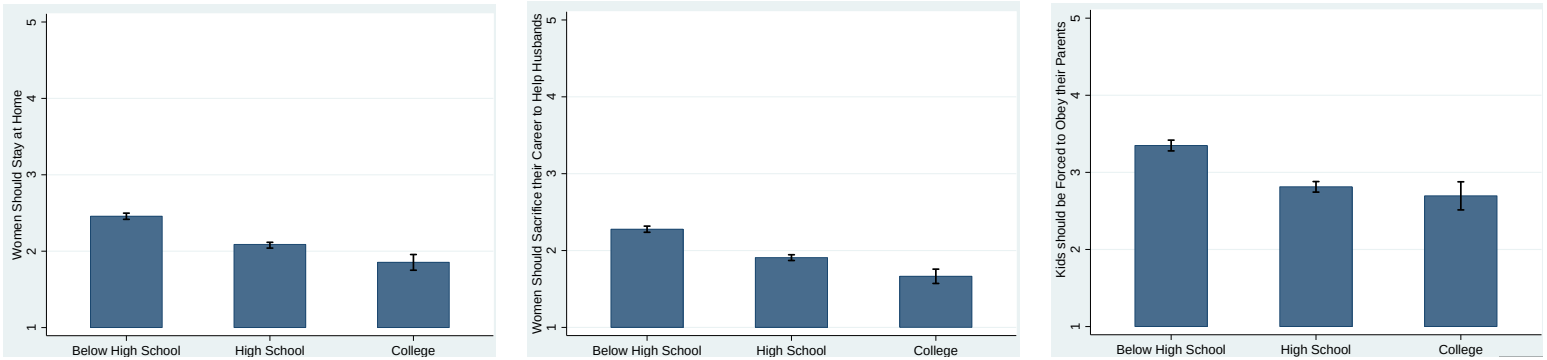
(a) Distrust in Finance and Neutral Values



(b) Current Antisemitism



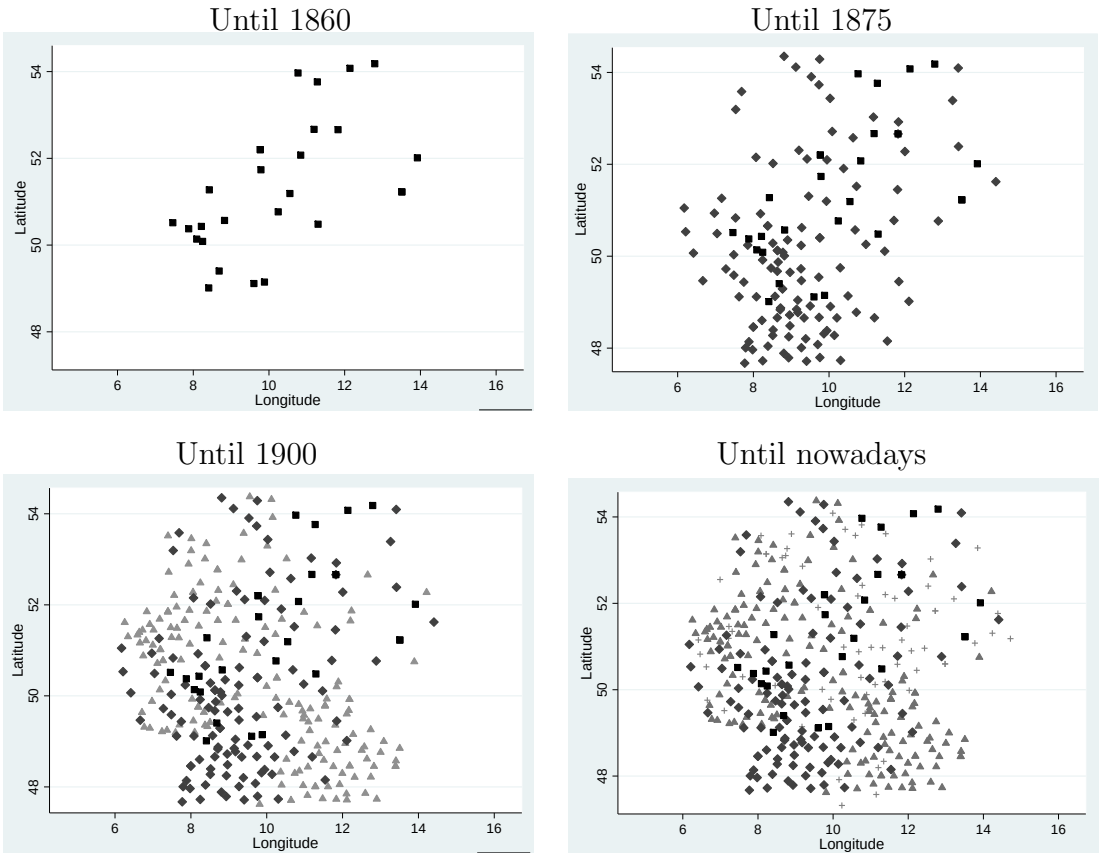
(c) Current Backwardness



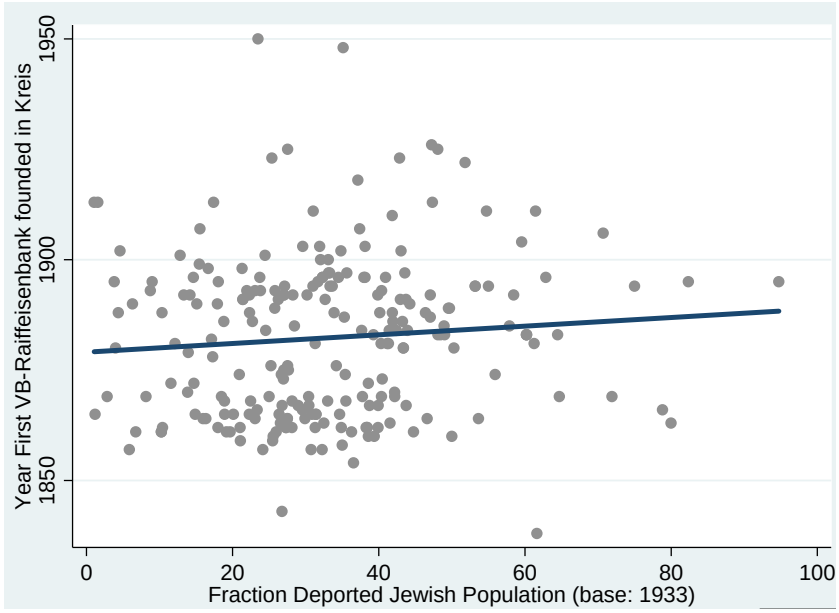
Each graph in Figure 6 reports sample mean of the outcomes on the vertical axes across three groups of respondents sorted by their education level: *Below High School* includes respondents who have a Realschule degree or lower levels of education; *High School* includes respondents who have an Abitur or correspondent levels of education in the German system. *College* includes respondents with a Vordiplom or higher levels of education.

Figure 7: **Supply-side Channel 1: Deep-Rooted Antisemitism and the Diffusion of Credit Unions**

(a) Diffusion Path of Credit Unions across Space and Over Time



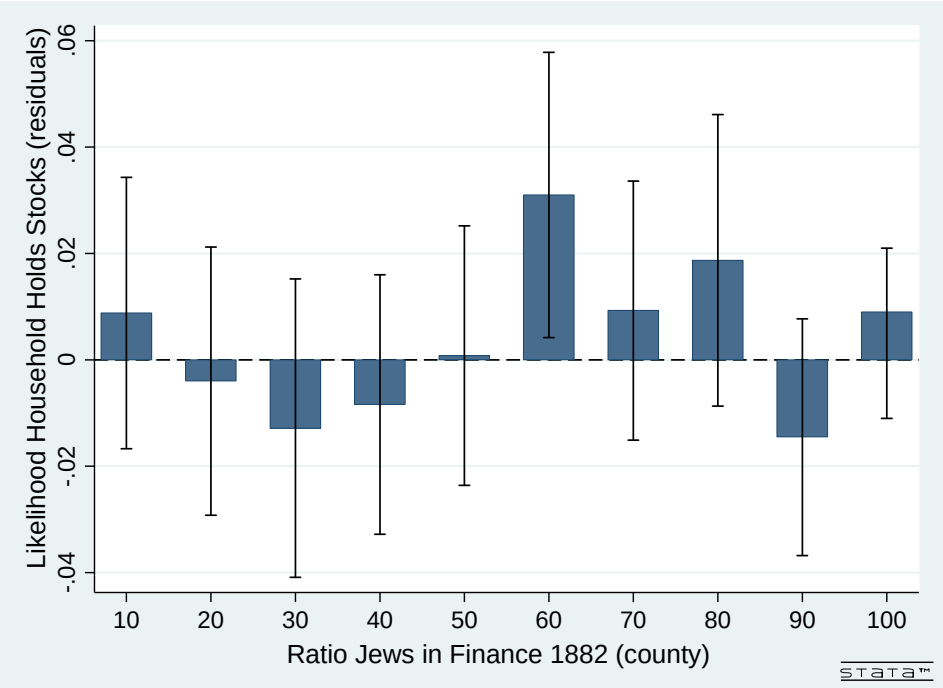
(b) Deep-Rooted Antisemitism and the Foundation of Credit Unions



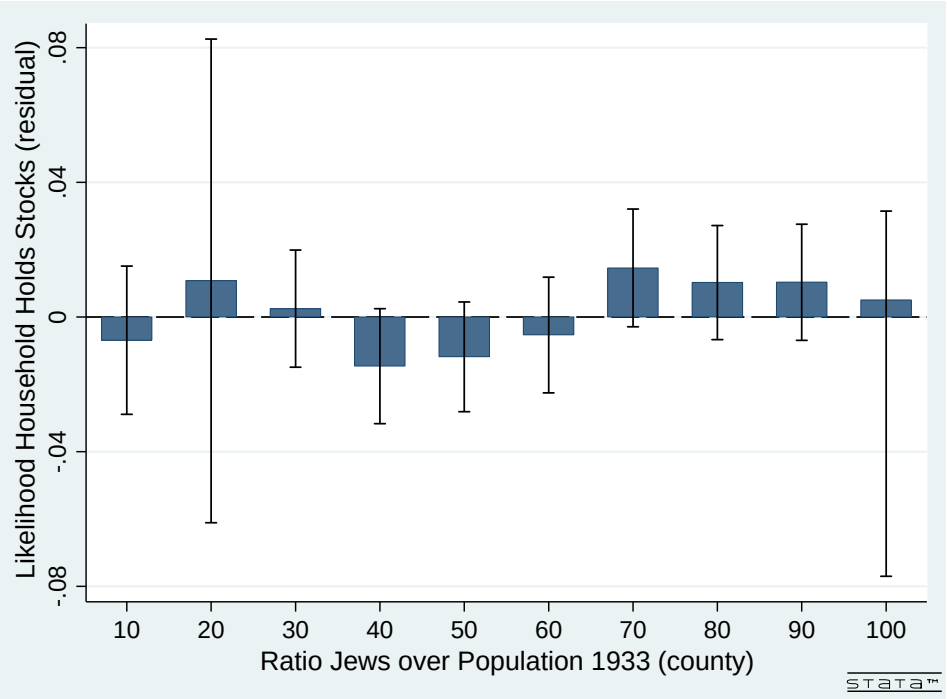
Panel (a) of Figure 7 plots the Germans counties where credit unions were first introduced at different points in time. Panel (b) of Figure 7 plots the correlation between the year when the first credit union was founded in a county and the ratio of Jews deported during the Nazi period over the total Jewish population in 1933. Each point is a German county.

Figure 8: **Supply-side Channel 2: 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



Panel (a) and Panel (b) of Figure 8 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 share of financial employees of Jewish religion in a county as of 1882 and of the ratio of Jews over the total German population in a county as of 1933. Intervals represent 95% confidence intervals for the estimated averages.

Table 1: **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
College	27075	0.016	0.127	0	1	13867	0.022	0.146	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
Ratio College graduates 2005	24766	24.40	4.929	17.60	34.60	13870	23.55	4.463	17.60	34.60	0.010

Table 1 reports summary statistics for 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 for the first 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 whose county of residence is known. The right Panel reports statistics for the subsample of households for which we do not miss any household or county-level characteristics. The column on the far right reports p-values for tests of the difference in the means across the two samples.

Table 2: **Jewish Persecution and Stock Market Participation**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
		Principal Component Measures of Persecution			Log Deported Jews (*extensive margin)*			Ratio Deported Jews (*intensive margin)*			Pogrom 1349		
	Probit	Probit	OLS	Probit	Probit	OLS	Probit	Probit	OLS	Probit	Probit	OLS	Probit
Persecution of Jews	-0.011	-0.012	-0.013	-0.010	-0.005	-0.006	-0.004	-0.032	-0.033	-0.039	-0.021	-0.021	-0.011
(column heading)	*0.005***	*0.004****	*0.004****	*0.004****	*0.002***	*0.002****	*0.002***	*0.017**	*0.017**	*0.016***	*0.009***	*0.009***	*0.011*
Log Jews 1933	0.045	-0.054	-0.017	-0.019	0.022	-0.014	0.015	-0.020	-0.032	-0.023	-0.025	-0.031	-0.019
	0.047	*0.052*	*0.052*	*0.009***	*0.038*	*0.038*	*0.010*	*0.056*	*0.052*	*0.009***	*0.037*	*0.037*	*0.010**
% Catholics 1925	0.029	0.006	0.004	0.002	0.003	-0.002	0.001	0.004	0.002	0.019	0.001	-0.001	0.001
	*0.013***	*0.011*	*0.012*	*0.014*	*0.011*	*0.011*	*0.001*	*0.011*	*0.011*	*0.014*	*0.011*	*0.011*	*0.001*
Age		-0.002	-0.002	-0.003	-0.003	-0.003	-0.003	-0.002	-0.002	-0.003	-0.003	-0.003	-0.003
		*0.001**	*0.001**	*0.001***	*0.001***	*0.001***	*0.001***	*0.001**	*0.001***	*0.001***	*0.001***	*0.001***	*0.001***
Age^2 /100		0.004	0.004	0.005	0.004	0.005	0.005	0.004	0.004	0.005	0.005	0.004	0.005
		*0.001****	*0.001****	*0.001****	*0.001****	*0.001****	*0.001****	*0.001****	*0.001****	*0.001****	*0.001****	*0.001****	*0.001****
Female		-0.036	-0.010	0.009	-0.014	-0.020	0.009	0.003	-0.010	0.009	-0.015	-0.020	0.009
		0.060	*0.061*	*0.007*	*0.060*	*0.060*	*0.007*	*0.006*	*0.061*	*0.007*	*0.060*	*0.060*	*0.007*
Single		0.058	0.052	0.063	0.057	0.052	0.063	0.058	0.052	0.063	0.058	0.052	0.064
		*0.010****	*0.009****	*0.010****	*0.010****	*0.009****	*0.010****	*0.010****	*0.010****	*0.010****	*0.010****	*0.009****	*0.010****
College		0.058	0.091	0.059	0.058	0.091	0.059	0.058	0.091	0.059	0.059	0.093	0.060
		*0.021****	*0.032****	*0.024***	*0.021****	*0.031****	*0.024***	*0.021****	*0.032****	*0.024***	*0.020****	*0.031****	*0.024***
Eastern Germany		-0.028	-0.024	0.008	-0.035	-0.031	0.008	-0.029	-0.026	0.010	-0.026	-0.021	0.008
		0.021	*0.019*	*0.029*	*0.021*	*0.019*	*0.029*	*0.022*	*0.020*	*0.029*	*0.020*	*0.018*	*0.028*
Income p.c. 2005		0.033	0.037	0.055	0.035	0.039	0.055	0.037	0.040	0.059	0.028	0.031	0.050
		*0.016***	*0.017***	*0.025***	*0.016***	*0.017***	*0.025***	*0.017***	*0.017***	*0.025***	*0.017**	*0.017**	*0.025***
% College graduates 2005		0.128	0.110	-0.001	0.162	0.145	-0.001	0.129	0.113	-0.001	0.090	0.065	-0.001
		0.142	*0.142*	*0.002*	*0.141*	*0.140*	*0.002*	*0.145*	*0.146*	*0.002*	*0.128*	*0.126*	*0.002*
Income deciles	X	X	X	X	X	X	X	X	X	X	X	X	X
Other historical controls	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	X
Regional controls		X	X	X	X	X	X	X	X	X	X	X	X
Only if comm. in 1349				X			X			X			X
Observations	17,360	13,596	13,596	9881	13,867	13,867	9881	13,596	13,596	9981	13,867	13,867	9881
N. of clusters	287	261	261	190	270	270	190	261	261	190	270	270	190
(Pseudo-) R^2	0.01	0.11	0.09	0.09	0.11	0.09	0.09	0.11	0.09	0.09	0.11	0.09	0.09

Table 2 reports the average marginal effects computed after estimating the following probit specification:

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

Each observation is a German household interviewed by the SOEP for the first 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 Jewish persecution reported at the top of each column. S.e. are clustered at the county level. Statistical significance is reported as follows: *10%, **5%, ***1%.

Table 3: **Robustness**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	County level		Within-state var.		Within-occupation zone IIWW var.		Excl. poorest counties		Excl. least educated counties	
	P.C. pers.	Pogrom 1349	P.C. pers.	Pogrom 1349	P.C. pers.	Pogrom 1349	P.C. pers.	Pogrom 1349	P.C. pers.	Pogrom 1349
Persecution of Jews	-0.017	-0.026	-0.009	-0.012	-0.011	-0.017	-0.018	-0.028	-0.020	-0.031
	*0.005***	*0.011***	*0.004***	*0.009*	*0.004***	*0.009**	*0.005****	*0.001***	*0.006****	*0.012***
Individual contr.	X	X	X	X	X	X	X	X	X	X
Historical contr.	X	X	X	X	X	X	X	X	X	X
Regional controls	X	X	X	X	X	X	X	X	X	X
Wave group f.e.			X	X	X	X	X	X	X	X
Observations	261	270	13,599	13,870	13,599	13,870	9,269	9,371	8,228	8,348
N. of clusters			261	270	261	270	153	156	164	168
(Pseudo-) R^2	0.23	0.23	0.10	0.10	0.09	0.09	0.10	0.10	0.10	0.10

Table 3 reports the average marginal effects computed after estimating the following probit spefication:

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

Persecution is the measure of Jewish persecution described at the top of each column. In columns (1) and (2), each observation is a German county, and the outcome variable is the share of households who own stocks in the county. In columns (3)-(10), each observation is a German household interviewed by the SOEP any time between 1984 and 2011, and the dependent variable is a dummy which equals one if the household holds stocks. In columns (3) and (4), we only exploit the variation in Jewish persecution within German states (*Bundeslaender*). In columns (5) and (6), we only exploit the variation in Jewish persecution within the four occupation zones after the Second World War, which were exposed to different denazification policies. In columns (7) and (8), we exclude households in counties in the lowest third of the distribution of GDP per capita. In columns (9) and (10), we exclude households in counties in the lowest third of the distribution of college education. S.e. are clustered at the county level (Kreis). Statistical significance is reported 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 residuals ($\hat{\epsilon_k}$) 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_k is a set of observables capturing the economic characteristics of counties in the past. In odd columns, K_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, any Hanseatic League city, any city 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_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). Statistical significance is reported as follows: *10%, **5%, ***1%.

Table 5: **Exclusion Restriction 2: Reduced Form and Placebo Distance**

Panel A. Reduced Form	(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

Panel B. Placebo Distance	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		French Households: West of the Rhine Valley, no Jewish migrations							
		Distance Trier			Distance Emmerich			Minimal distance	
Log Distance	-0.003	-0.005	-0.004	0.005	-0.003	-0.003	-0.003	-0.005	-0.004
	0.013	*0.011*	*0.011*	*0.028*	*0.024*	*0.024*	*0.013*	*0.011*	*0.011*
Regional controls	X	X	X	X	X	X	X	X	X
Individual controls		X	X		X	X		X	X
Income quintiles		X	X		X	X		X	X
Education quintiles		X	X		X	X		X	X
Size of town			X			X			X
Observations	9692	9383	9383	9692	9383	9383	9692	9383	9383
Adjusted R^2	0.01	0.13	0.13	0.01	0.13	0.13	0.01	0.13	0.13

Panel A of Table 5 reports the average marginal effects computed after estimating the following probit specification:

$$Pr(HoldsStocks_i|X_i, K_k) = \Phi(\alpha + \beta_1 \times DistanceRhine_k + \beta_2 \times PersecutionJews_k + X_i' \times \delta + K_k' \times \delta + \eta_t + \epsilon_{ik}) \quad (8)$$

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 with at least a pogrom against the local Jewish community in 1349 in columns (3), (6), and (9). Panel B of Table 5 reports the coefficient on the distance measures for estimating Equation 8 for French households interviewed by the *Enquete Patrimoine* run by the Bank of France in 2004. S.e. are clustered at the county level. Statistical significance is reported as follows: *10%, **5%, ***1%.

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

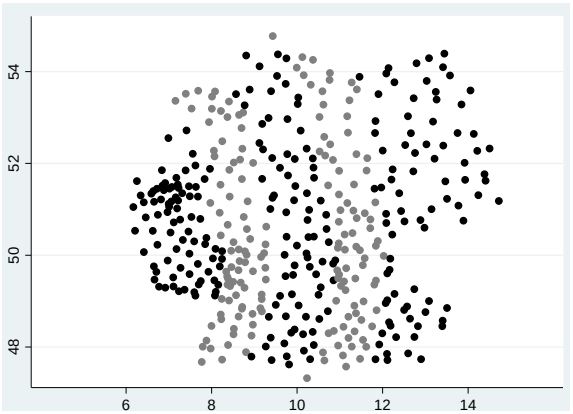
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	First stage: Existence Jewish community in 1049			Second stage: Persecution of Jews			Third stage: Holds stocks		
	Dist. Trier	Dist. Emmerich	Minimal dist.	Dist. Trier	Dist. Emmerich	Minimal dist.	Dist. Trier	Dist. Emmerich	Minimal dist.
Res. Distance Rhine	-0.136	-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

Table 6 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 principal component of the measures of Jewish persecution 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. Statistical significance is reported as follows: *10%, **5%, ***1%.

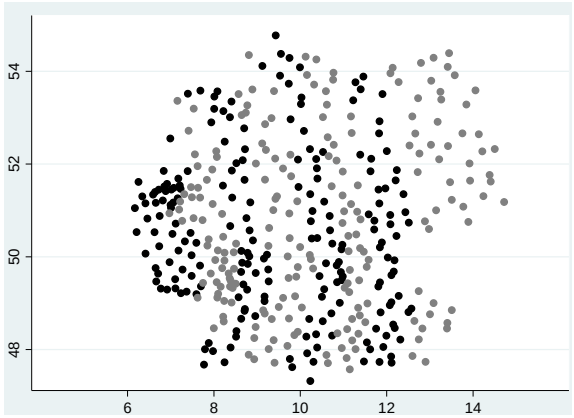
Table 7: **Keeping the Distance from the Rhine Valley Constant: Within-distance-group analysis**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Principal Component Measures of Persecution			Log deported Jews *(extensive margin)*			Ratio deported Jews *(intensive margin)*			Pogroms 1349		
Persecution of Jews	-0.011	-0.010	-0.010	-0.006	-0.006	-0.005	-0.027	-0.025	-0.021	-0.018	-0.015	-0.018
(see columns heading)	*0.004***	*0.004***	*0.004***	*0.002****	*0.002****	*0.002****	*0.017*	*0.017*	*0.016*	*0.009***	*0.009**	*0.009***
Distance group f.e.	X	X	X	X	X	X	X	X	X	X	X	X
Number distance groups	5	8	10	5	8	10	5	8	10	5	8	10
Avg. st.dev.distance within groups (Km)	135	127	120	135	127	120	137	127	120	137	127	120
Average n. counties across groups	52	41	31	54	42	33	52	41	31	54	42	33
Individual, historical controls	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	X
Observations	13,599	13,599	13,599	13,870	13,870	13,870	13,599	13,599	13,599	13,870	13,870	13,870
N. of clusters	261	261	261	270	270	270	261	261	261	270	270	270
(Pseudo-) R^2	0.09	0.09	0.10	0.10	0.09	0.09	0.09	0.09	0.10	0.09	0.09	0.10

(a) Five Distance Groups



(b) Eight Distance Groups



(c) Ten Distance Groups

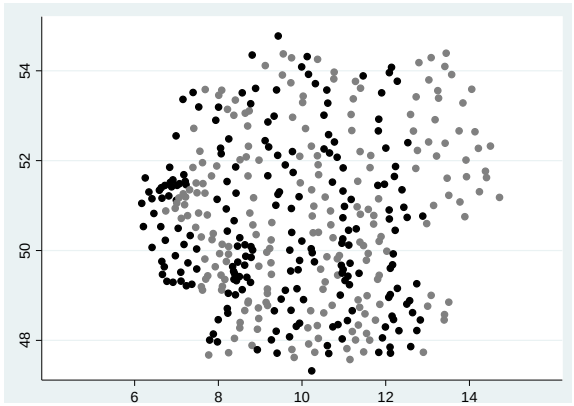


Table 7 reports the average marginal effects computed after estimating the following probit specification:

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

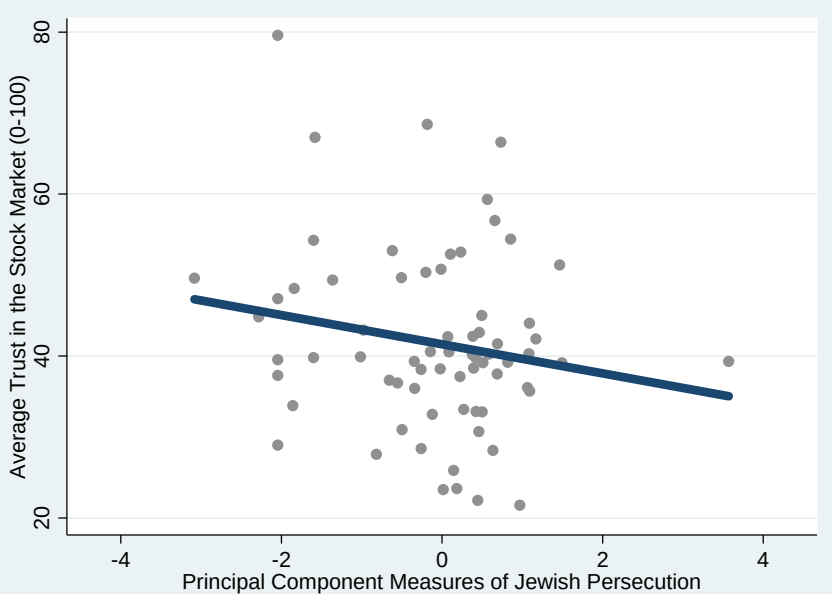
Each observation is a German household interviewed by the SOEP for the first time between 1984 and 2011. In all columns, the dependent variable is a dummy which equals one if the household holds stocks. *Persecution* is the measure of Jewish persecution described at the top of each column. η_s are a set of 5, 8, or 10 groups of counties at similar distance from the Rhine Valley. S.e. are clustered at the county level. Statistical significance is reported as follows: *10%, **5%, ***1%.

Table 8: **Direct Validation: Deep-Rooted Antisemitism and Trust in Finance**

Panel A. Deep-Rooted Antisemitism and Trust in Finance: Individuals

	(1)	(2)	(3)	(4)
P.C. Persecution Jews	-1.812	-2.151	-3.205	-3.000
	(1.031)	(1.083)**	(1.412)**	(1.551)*
Male		9.182	8.767	8.885
		(2.436)***	(2.437)***	(2.573)***
Log Jews 1925			1.175	1.218
			(0.950)	(1.044)
% unemployed 1933			0.030	0.031
			(0.189)	(0.202)
Age group f.e.		X	X	X
Education group f.e.		X	X	X
Income p.c. quintiles		X	X	X
Land f.e.				X
Observations	700	618	611	611
N. of clusters	69	64	63	63
(Pseudo-) R^2	0.01	0.01	0.01	0.01

Panel B. Deep-Rooted Antisemitism and Trust in Finance: Counties



Panel A of Table 8 reports the coefficients for estimating four tobit specification whose outcome variable is the survey-based measure of trust in the financial markets at the individual level, which ranges from 0 to 100. Standard errors are clustered at the county level. S.e. are clustered at the county level (Kreis). Statistical significance is reported as follows: *10%, **5%, ***1%. Panel B of Table 8 plots the average trust in financial markets at the county level against the principal component of the measures of Jewish persecution at the county level.

Table 9: **Placebo Validation: (Non-)Effect of Antisemitism on other Outcomes**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	P.C. Measures 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

Table 9 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 for the first time between 1984 and 2011. In all columns, the dependent variable is a dummy equal to 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 individual-level dependent variable described on the left. S.e. are clustered at the county level (Kreis). Statistical significance is reported as follows: *10%, **5%, ***1%.

Table 10: **Non-sensitivity of the effect to education levels**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	P.C. Measures of Persecution		Log deported Jews (extensive margin)		Ratio deported Jews (intensive margin)		Pogrom 1349	
Panel A. Effect by individual level of education								
Persecution	-0.011	-0.010	-0.005	-0.005	-0.030	-0.015	-0.013	-0.024
	*0.005***	*0.007*	*0.002***	*0.003*	*0.018**	*0.037*	*0.009*	*0.014**
Persecution*College	-0.029	-0.029	-0.009	-0.009	-0.017	-0.016	-0.045	-0.041
	0.026	*0.0027*	*0.011*	*0.012*	*0.014*	*0.014*	*0.059*	*0.061*
Persecution*High School		-0.002		-0.001		-0.002		0.006
		0.006		*0.003*		*0.019*		*0.013*
College	0.105	0.106	0.157	0.159	0.163	0.162	0.138	0.137
	*0.031****	*0.0032****	*0.059****	*0.063***	*0.053****	*0.055****	*0.046****	*0.047****
High School		0.013		0.015		0.019		0.009
		0.008		*0.018*		*0.015*		*0.012*
Panel B. Effect by ratio college graduates in county								
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

Table 10 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 for the first time between 1984 and 2011. In all columns, the dependent variable is a dummy equal to 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 dummies that equal 1 if the household head holds a college degree or a high school degree. In Panel B, *Persecution* is interacted with dummy variables for two groups of households: those in the top third and those in the bottom third of the distribution based on the county-level share of college graduates. S.e. are clustered at the county level (Kreis). Statistical significance is reported as follows: *10%, **5%, ***1%.

Table 11: **Properties of the Effect: Over Time and Across Cohorts**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Effect over time	P.C. Measures 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*
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

Table 11 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 for the first time between 1984 and 2011. In all columns, the dependent variable is a dummy equal to one if the household holds stocks. The main covariate of interest, $Persecution$, is the measure of persecution of Jews described on 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. S.e. are clustered at the county level (Kreis). Statistical significance is reported as follows: *10%, **5%, ***1%.

10 Appendix

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

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

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:

$$\begin{aligned} &Cov(Distance_purged, \epsilon) = 0 \\ \Leftrightarrow &\beta_3\beta_2 Cov(Distance_purged, \epsilon_1) + \beta_3 Cov(Distance_purged, \epsilon_2) + Cov(Distance_purged, \epsilon_3) = 0 \end{aligned}$$

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 12: **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 12 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%.

Online Appendix

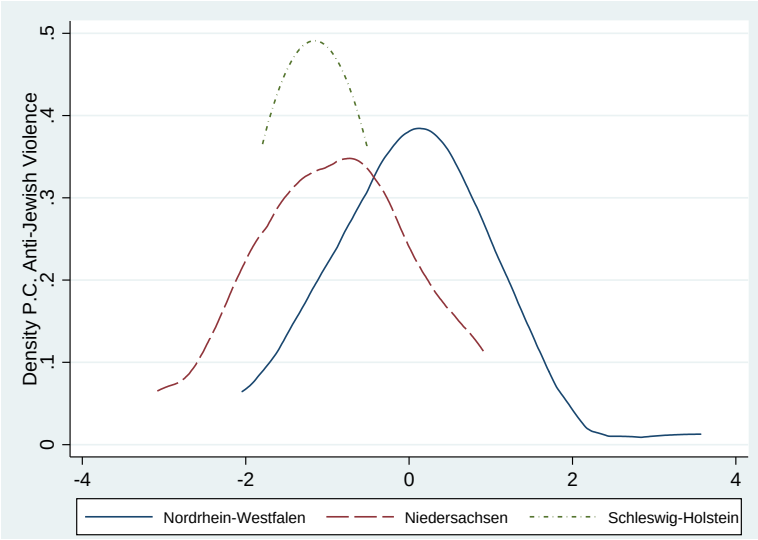
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DEEP-ROOTED ANTISEMITISM AFFECTS HOUSEHOLDS' INVESTMENTS

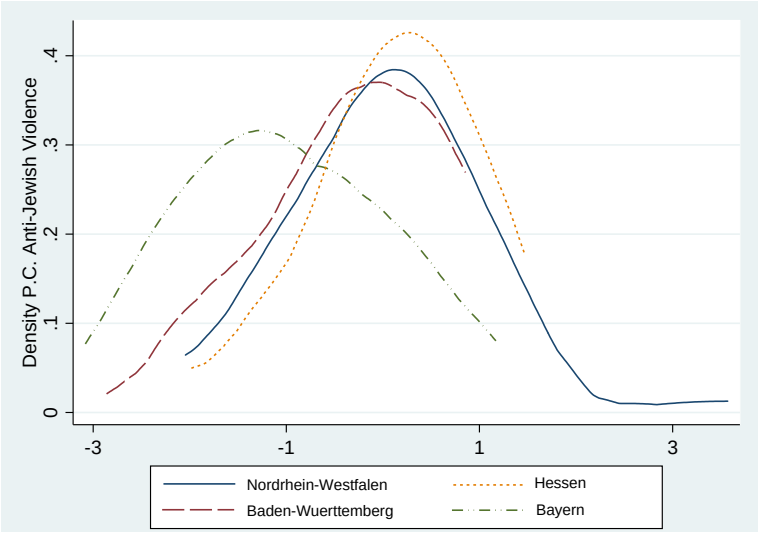
Francesco D'Acunto Marcel Prokopczuk Michael Weber

Figure 9: **Densities of county-level Persecution across German States**

(a) Out of Rhine Valley towards North



(b) Out of Rhine Valley towards South



(c) Furthest Distance from Rhine Valley in both Directions

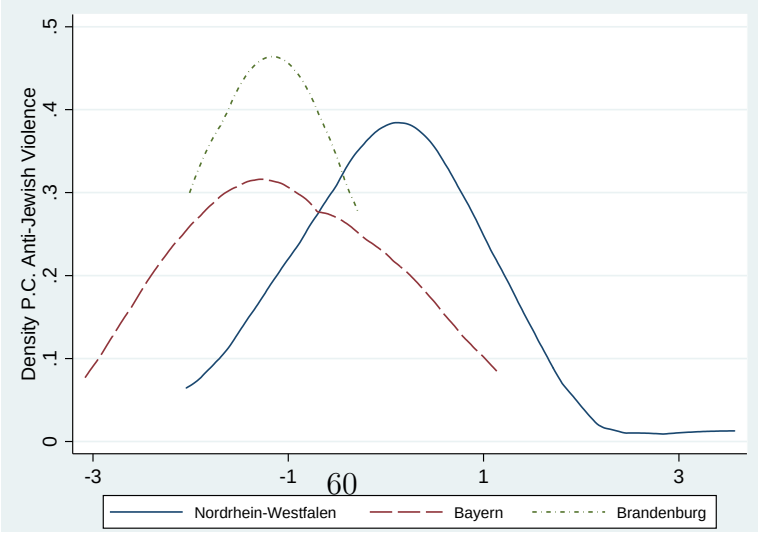


Figure 10: **Raw Data - Correlations**

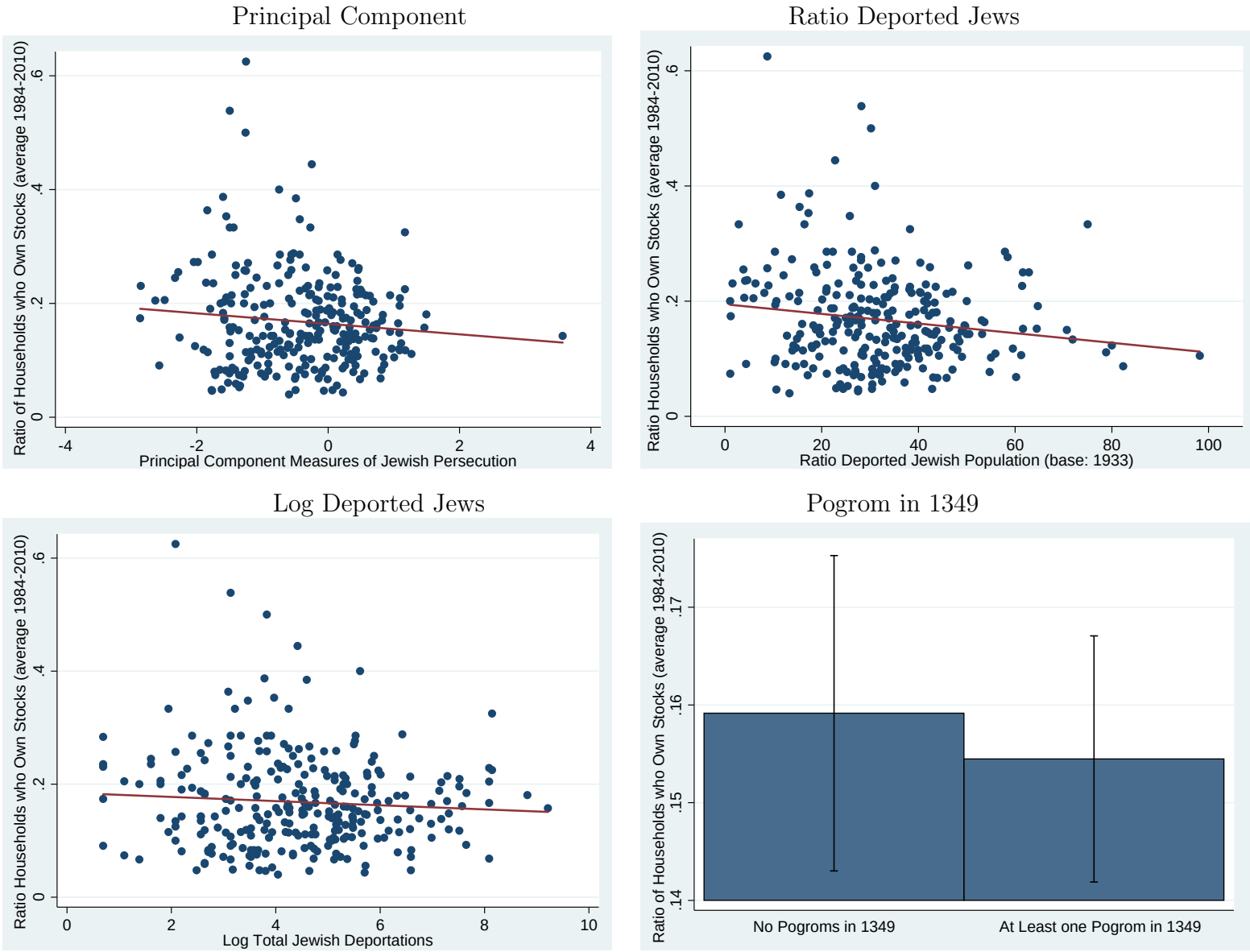


Figure 11: **Raw Data - Stockholdings, Principal Component by Pogrom in 1349**

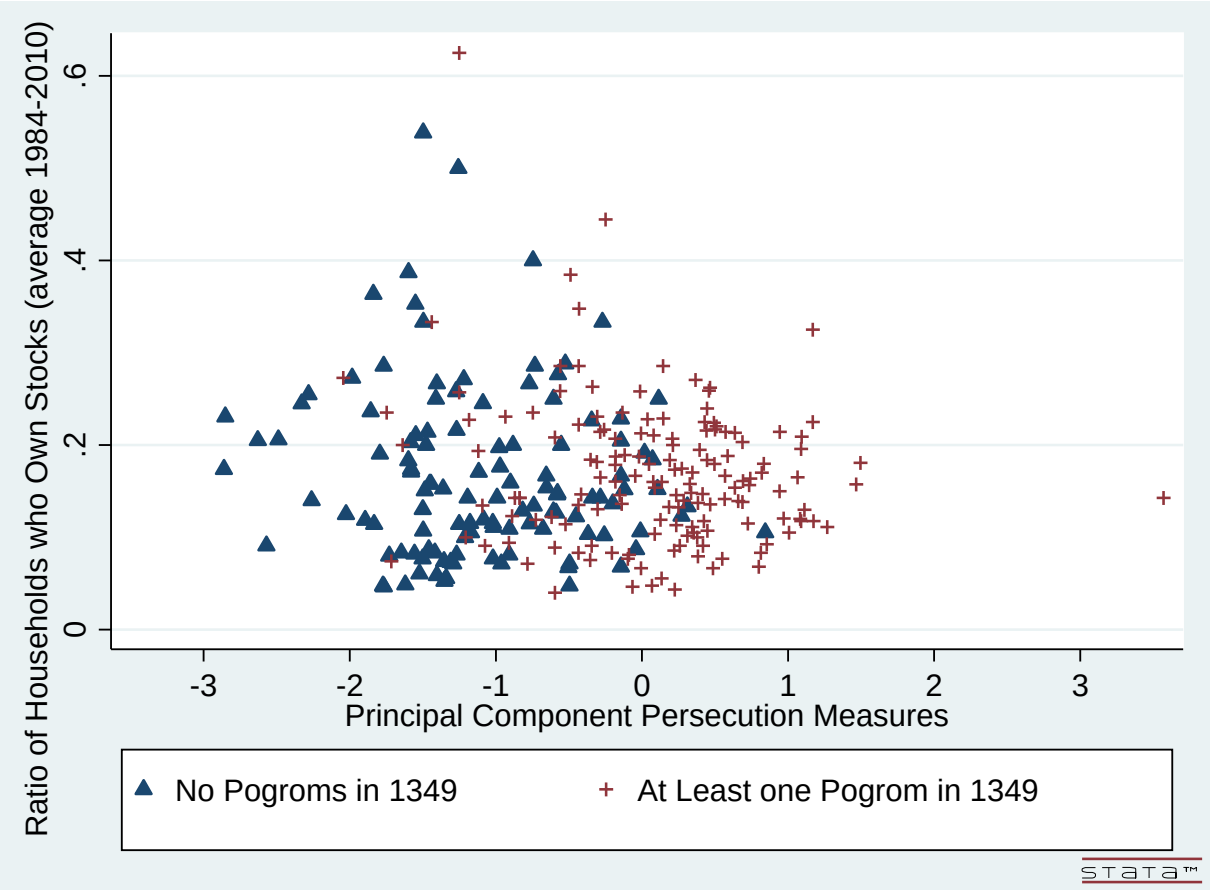
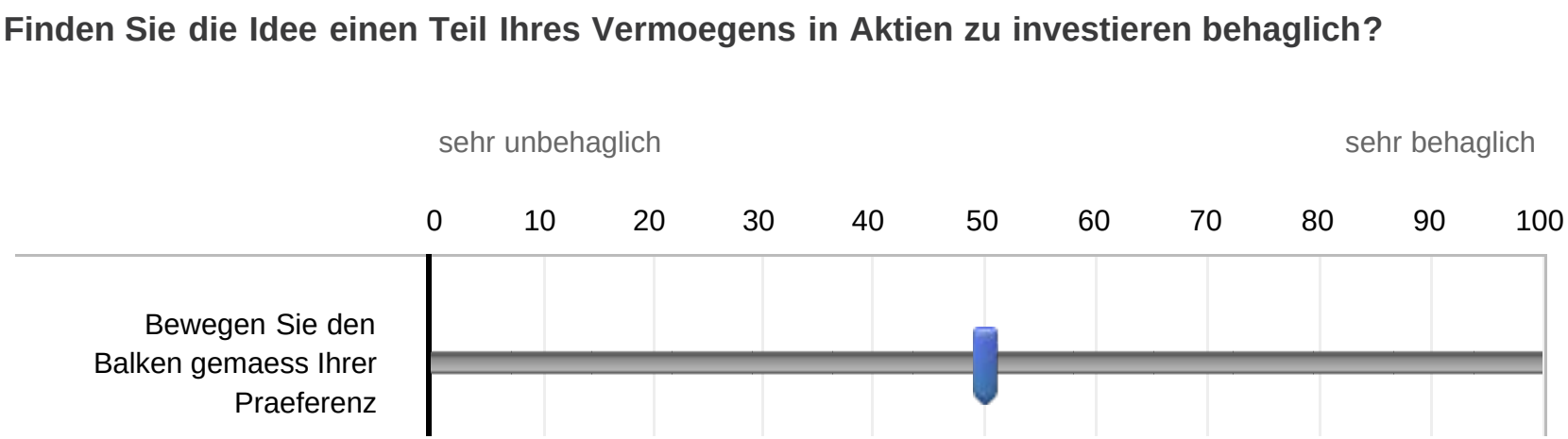
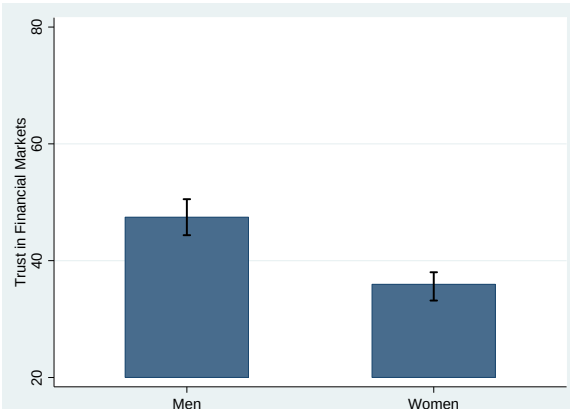


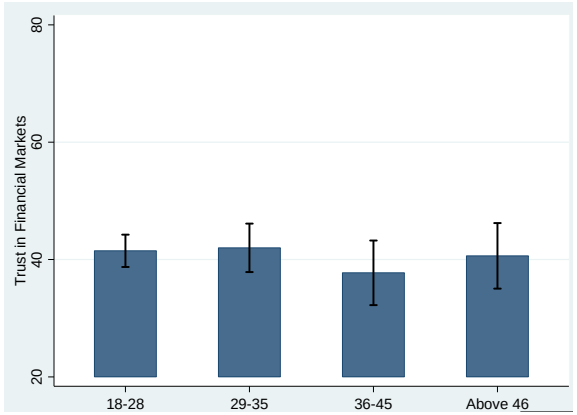
Figure 12: **Trust in Finance: Survey and Demographics**



(a) Trust in Finance by Gender



(b) Trust in Finance by Age



(c) Trust in Finance by Education

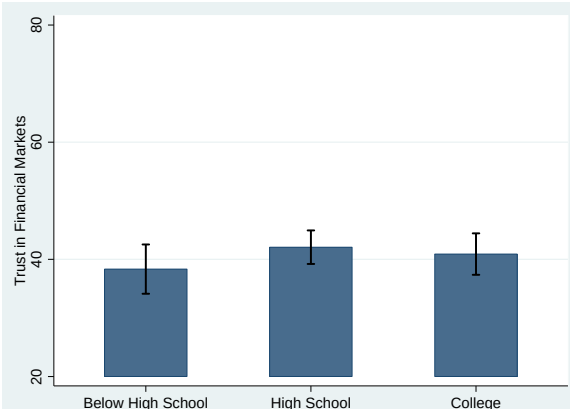
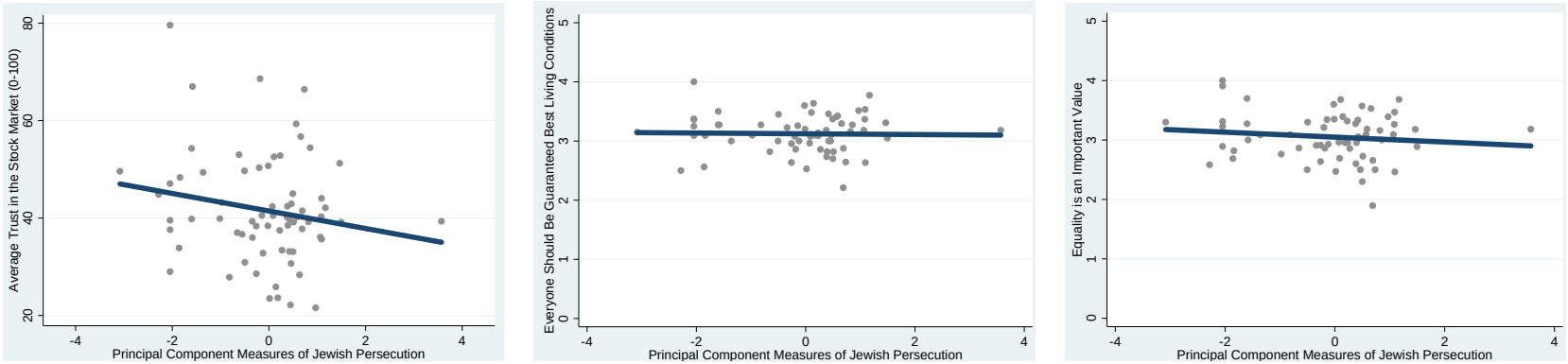
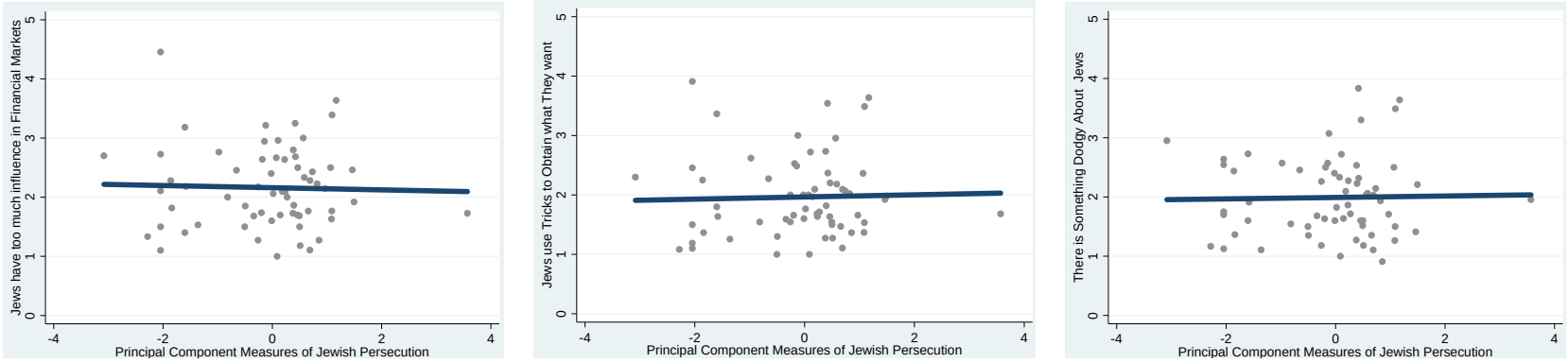


Figure 13: **Distrust in Finance, Antisemitism, Backwardness, and Jewish Persecution**

(a) Distrust in Finance and Neutral Values



(b) Current Antisemitism



(c) Current Backwardness

