Distrust in Finance Lingers: Jewish Persecution and Households' Investments

Francesco D'Acunto Marcel Prokopczuk Michael Weber *

This Version: September 2014

Abstract

We look at the geography of historical Jewish persecution to proxy for localized distrust in finance. Households in German counties where Jewish persecution was one standard deviation higher are 7.5% to 12% less likely to invest in stocks. The results hold when comparing only geographically close counties, and counties that hosted documented Jewish communities in the distant past. Current antisemitism, discriminatory beliefs, generalized trust, or supply-side forces do not explain the effect, which instead is consistent with a norm of distrust in finance, transmitted across generations. The forced migrations of Jewish communities across the German lands in the Middle Ages help assess if the effect of Jewish persecution on stockholdings is causal.

JEL: D91, G11, J15, N33.

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

*D'Acunto is at the Haas School of Business, UC Berkeley (francesco_dacunto@haas.berkeley.edu). Prokopczuk is at Leibniz University Hannover (prokopczuk@fmt.uni-hannover.de). Weber is at the Booth School of Business, University of Chicago (Michael.Weber@chicagobooth.edu). The views expressed in this paper do not reflect those of the DIW, the Bundesbank, the Banque de France, or the Friedrich Ebert Stiftung. We thank Joachim Voth and Nico Voigtländer for making their data on anti-Jewish violence publicly available, Thomas Kick for his help with the Bundesbank data on county-level bank branches, Tobias Lauter for his assistance with the Hoppenstedt data, the ifo-Prussian Economic History project, and Ralph Melzer, Elmar Braehler, and Oliver Decker for sharing the micro-data underlying the FES Mitte-Studies. For very helpful comments and discussions, we thank Ran Abramitzky, Nick Barberis, Zahi Ben-David, Kelley Bergsma, Michael Brennan, Johannes Buggle, Filipe Campante, Davide Cantoni, Jason Chen (discussant), James Choi, Pierluigi D'Acunto, Stefano DellaVigna, Barry Eichengreen, Joey Engelberg, Ruben Enikolopov, Paola Giuliano, Will Goetzmann, Luigi Guiso, Tarek Hassan, Danling Jiang, Samuli Knupfer, Ross Levine, Sonya Lim (discussant), Dmitry Livdan, Ulrike Malmendier, Gustavo Manso, Adair Morse, Petra Moser, Rabbi Adam Naftalin-Kelman, Terry Odean, Martha Olney, Christine Parlour, Chris Parsons, Hagit Perry, Caitlin Rosenthal, Markus Schmid, Andrei Shleifer, Stephan Siegel, Paul Smeets, Robert Vishny, Nico Voigtländer, Jason Wittenberg, Noam Yuchtman, seminar participants at the 2014 UBC Summer Finance conference, the 2014 FSU Finance conference, UC Berkeley, and especially Joachim Voth. All errors are our own.

1 Introduction

For centuries, Jews have been associated with financial services, largely because Christians and Muslims were banned from lending money at interest.[1] Indeed, when local credit unions and government-owned banks had just started to diffuse across Germany in 1882, 3% of German workers were Jewish, but 23% of the financial sector workforce was Jewish. At the same time, Europeans had persecuted Jewish communities since the Middle Ages. They attacked them in times of distress, ranging from the Black Death of 1349 to the economic crisis of the 1930s.

We consider the geographic heterogeneity of historical Jewish persecution across German counties (*Kreise*) to proxy for the distrust in financial markets of Germans.[2] The monopoly Jewish people had on money-lending had been broken by the 19^{th} century due to the diffusion of credit unions and government-owned banks. But Jews were accused through the Second World War of running financial markets. In the delirious words of Hitler, "In times of distress a wave of public anger has usually arisen against the Jew; [...] the Jew gained an increasing influence in all economic undertakings by means of his predominance in the stock exchange."[3]

Figure 1 shows our main finding: we plot the partial correlation of Jewish persecution with the share of inhabitants in German counties who own stocks. Households in counties with higher persecution on average invest less in stocks. One-standard-deviation higher Nazi persecution correlates with a 1.2-percentage-point lower likelihood that households invest in stocks, or 7.5% of the average participation. Pogroms during the Black Death correlate with a 2-percentage-point lower participation, or 12% of the average participation.

[1]For instance, Pope Leo IX banned Christians from lending money at interest as far back as 1049, and Gratianus formalized the ban in the *Corpus Iuris Canonici* in 1150: "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 human capital Jews had accumulated 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]Distrust in finance is a channel for limited stock market participation complementary to generalized trust (e.g., Gennaioli, Shleifer, and Vishny (2014a) and Gennaioli, Shleifer, and Vishny (2014b)).

[3]See *Mein Kampf (James Murphy Translation)*, Vol. I, chapter XI: Race and People.

A set of tests helps us understand which variation drives these associations. The results are robust if we only exploit the variation in Jewish persecution across geographically close counties: counties in the same states (*Bundesländer*), in the same occupation zones after the Second World War, or in the same virtual states. Virtual states are arbitrary partitions of Germany that account for the endogeneity of state borders, and delimit homogeneous areas along several dimensions, such as geography, the quality of institutions, and cultural heritage. The results are also similar if we look only at counties in which a Jewish community was documented as far back as the Middle Ages.[4] Hence, the mere presence of Jewish communities that did not trigger persecution cannot explain the results.

Incentives that do not involve ideological hatred or distrust toward the Jewish population may have driven historical persecution. For instance, Christians often took part in pogroms against Jewish communities to seize their wealth. To check whether ideological hatred relates to our findings, we look at the electoral support for the Nazi party. We compare counties in which the economic crisis of the 1930s was less severe - indicating that Nazi votes were likely driven by ideological rather than economic factors (de Bromhead, Eichengreen, and O'Rourke, 2013) - with counties in which the economic crisis was more severe. This test is based on the fact that antisemitism was a pillar of Nazi ideology. Nazi votes are negatively associated to present-day stockholdings only in counties where Nazi votes were likely to signal primarily ideological, rather than primarily economic, support. Nazi votes have no significant association to economic indicators other than stockholdings, and votes for other parties are completely unrelated to present-day stockholdings.

To dig deeper into the scope for a localized cultural norm of distrust in stock markets, we investigate the channels that transmitted the long-term association between Jewish persecution and present-day stockholdings. We run a survey on a sample of 1,000 Germans, and find that respondents in counties with higher Jewish persecution trust the

[4]We thank Nico Voigtländer and Ruben Enikolopov for inspiring these tests.

stock market significantly less than others.[5] We consider three demand-side channels. First, Jewish persecution may correlate with other deep-rooted beliefs that we label "backwardness." Backwardness includes cultural cues that make suspicious anything that is not familiar, such as xenophobia and generalized distrust. Second, those German households which are antisemitic may still associate the stock market with Jews, and thus counties where historical and present-day antisemitism are higher may display less investment in stocks. Third, deep-rooted distrust in finance, proxied by Jewish persecution, may have transmitted across generations independently from antisemitism: even if households do not associate Jews with finance anymore, they may still distrust finance more in counties where Jewish persecution was higher. All three channels predict lower stock market participation in counties where Jewish persecution was higher. However, the channels have different predictions across education levels: more educated individuals are less prone to backwardness, and are less antisemitic today (e.g., Glaeser (2005), Voigtlaender and Voth (2013a)). We replicate these results with novel data from the *Friedrich Ebert Stiftung*. Education curricula affect pupils' ideology and shape cultural norms (Cantoni et al. (2014)). German curricula do address the issue of antisemitism, decreasing the antisemitism of students. Yet the curricula do not address the benefits of stock market participation, meaning that prejudice against financial institutions is not decreased alongside antisemitism. We find that Jewish persecution predicts distrust in finance and stock market participation similarly across *all* education levels. Thus, generic backwardness or present-day antisemitism cannot drive our findings. Instead, the results are consistent with a cultural norm of distrust in finance that has transmitted across generations independently from antisemitism.

We also test for two supply-side channels that may explain our findings through a lower supply or awareness of investment opportunities in the past: the diffusion process of credit unions and local saving banks, and the depletion of human capital due to Jewish persecution.[6] These channels do not explain our findings.

[5]The survey is run through *ClickWorker*, a German online platform similar to the US-based *mTurk*. The survey is inspired by the Kellogg-Booth Financial Trust Index, described by Sapienza and Zingales (2012). We thank Stefano Della Vigna and Noam Yuchtman for inspiring this test.

[6]We thank Joachim Voth and Ulrike Malmendier for suggesting these tests inspired by Pascali (2014).

To better understand the properties of the baseline effect, we analyze the heterogeneity of the effect over time and across cohorts. Localized antisemitic sentiment has persisted for at least six centuries (Voigtlaender and Voth, 2012). The effect of Jewish persecution on investments is also stable throughout our sample (1984 to 2010). Moreover, we find that the size of the effect is similar for all cohorts, except for those born before 1945. Everyone in the pre-1945 cohort invests little in stocks, and the variation in Jewish persecution cannot predict the small variation in their stockholdings. The massive antisemitic propaganda of the Nazi period may have eliminated the variation of antisemitic beliefs across German counties.[7] But the experience of a major economic crisis may also drive the result: cohorts that experienced severe stock market downturns invest in stocks less than others (Malmendier and Nagel (2011), Giuliano and Spilimbergo (forthcoming)). We interact cohort dummies with the unemployment rates in 1933, and find results consistent with the experience argument.

In the last part of the paper, we investigate the extent to which the effect of Jewish persecution on stockholdings may be causal. Reverse causality issues do not arise in our setting because of the timing of the outcomes we consider. But unobservables that determined Jewish persecution may affect stockholdings through channels other than distrust in finance. The results for geographically close counties, and for counties where Jewish communities existed in the past, and the evidence on the channels that have transmitted the effect, reduce the concerns that unobservables drive our results.
The ideal analysis would exploit a quasi-exogenous shock to the occurrence of historical Jewish persecution unrelated to present-day stockholdings. To get close to such a shock, we exploit the forced migrations of the first Jewish communities across the German lands in the Middle Ages because of the Crusades. As far back as the ninth century, Ashkenazi Jews settled along the Rhine and Mosel Valleys in an area that is still known in Hebrew as Ashkenaz. The Crusades of the 11th and 12th centuries forced these communities to leave their hometowns. Over time, Ashkenazi communities have spread widely from the Rhine Valley throughout the German lands. The diffusion has taken centuries because of

[7] This explanation is at odds with the results of Voigtlaender and Voth (2012).

transportation costs and possibly the properties of migration networks (e.g., see Spitzer (2014)).

We consider a three-stage strategy based on the fact that acts of violence against Jewish communities in the past may be observed if two conditions are fulfilled: (i) a Jewish community existed in the county, and (ii) anti-Jewish sentiment had developed in the county. In the first stage, the distance of a county from the Rhine Valley predicts the probability that a Jewish community existed before the Black Death of 1349. In the second stage, the probability of existence of a Jewish community predicts the extent of Jewish persecution. The rationale is that in counties with no Jewish communities, the ex-ante probability of observing acts of violence against Jews is zero. By contrast, the mere presence of a Jewish community before 1349 makes the probability of Jewish persecution positive. In the third stage, we regress present-day stockholdings on the predicted extent of Jewish persecution. Three-stage least squares estimates based on this strategy confirm the negative effect of Jewish persecution on stockholdings.

A concern with this strategy is that no discontinuity exists in the treatment intensity, captured by the distance from the Rhine Valley. We cannot definitively rule out unobservables correlated with the distance, with Jewish persecution, and with present-day stockholdings. This concern is alleviated by the fact that our results are robust to using only the variation in Jewish persecution across counties close in space, and hence at similar distances from the Rhine Valley. Moreover, we measure the distance from the Rhine Valley for cities at the opposite ends of the Rhine river, and verify that the results are similar when we use the spatial variation across the alternative gradients north-west/south-east and south-west/north-east. We use the distance to capture the variation in Jewish persecution across counties that hosted Jewish communities or not. The distance does not explain the difference between counties that hosted persecuted communities and counties that hosted non-persecuted communities. Nevertheless, the baseline results are robust to limiting the variation in persecution only across counties that hosted a Jewish community in the past. Thus, the mere presence of Jewish communities in a county cannot explain the results.

At the same time, the distance from the Rhine Valley has several desirable properties that rule out the most plausible sources of heterogeneity that threaten the identification of a causal effect. First, the Rhine Valley was the cradle of Germany's industrialization and is still highly economically developed. If being closer to the wealthy Rhine Valley increased stock market participation, we should observe a negative correlation between distance and participation, which is the opposite of our effect. Second, as in Buggle (2013), we find that distance from the Rhine Valley has a strong negative association with generalized trust, which positively affects stock market participation (Guiso, Sapienza, and Zingales (2009)).[8] Hence, we can disentangle the effects of generalized trust from those of trust in finance, which are likely to be positively correlated in settings different from ours.[9] Third, distance from the Rhine does not predict present-day outcomes that affect stock market participation, and has no autonomous association with stockholdings when added next to Jewish persecution in the baseline specifications. Fourth, we use French households, who live to the west of the Rhine, for a placebo test: Ashkenazi Jews migrated eastwards, and not to the west where crusaders were already persecuting the local communities. If the distance captured spatial variation that is unrelated to the forced migrations, it should affect French stockholdings as well as German stockholdings, but it does not.[10]

This paper makes four contributions. It is one of the few that document a negative long-term financial effect of persecuting a minority. Because of the historically high equity premium, avoiding stock investments reduces households' accumulation of financial wealth. Second, we introduce three novel data sources: (i) we collect historical data on the diffusion of German credit unions in the 19^{th} century, a process worthy of interest in itself; (ii) we use unique data from the *Friedrich Ebert Stiftung* documenting the association between Jews and financial markets next to other dimensions of backwardness for Germans; and (iii) we run our own survey to measure the distrust in finance in Germany. Third, we use variation in the motives to support the Nazi Party to construct a

[8] Buggle (2013) identifies this effect as a result of the Napoleonic institutions in the early 19th century.

[9] Persecution is unrelated to generalized trust as measured from the World Value Survey for geographically close counties.

[10] We thank Sonya Lim for suggesting this test.

test for the long-term effects of the Nazi ideology. Fourth, we propose an argument for the emergence of Jewish persecution, which may help explain the diffusion of antisemitism in Voigtlaender and Voth (2012) and Voigtlaender and Voth (2013a).

This paper contributes to the research on the economic effects of culture initiated by Banfield (1958) and Putnam (1993). Guiso, Sapienza, and Zingales (2004) show that social capital positively affects stock market participation, illiquid investments, and the use of checks. In Algan and Cahuc (2006), the inherited component of trust by immigrants affects economic growth in the United States. We use a historical natural experiment to explain present-day economic outcomes (Spolaore and Wacziarg (2013), Nunn (2013)). This approach departs from the standard aims of economic history as it *directly* explains present-day economic decisions using natural experiments of the past. It could be labeled "History & Economics," or for financial outcomes, "History & Finance" (D'Acunto, 2014).

The paper relates to the limited stock market participation puzzle. Explanations include background risk (Paxson (1990), Guiso, Jappelli, and Terlizzese (1996)), social interactions (Hong et al., 2004), awareness (Guiso and Jappelli, 2005), generalized trust (Guiso, Sapienza, and Zingales, 2009), financial literacy (van Rooij, Lusardi, and Alessie, 2011), macroeconomic experiences (Malmendier and Nagel, 2011), labor income risk (Betermier, Jansson, Parlour, and Walden (2012)), and corporate scandals (Giannetti and Wang, 2014). Jewish persecution is used to proxy for distrust in finance, but it need not be the *only* source of distrust in finance.

We also build on recent literature on the effects of Jewish persecution, especially on Voigtlaender and Voth (2012). We use their historical data and compute their measures at the county level. Our work differs because (i) we investigate the effects of Jewish persecution on economic outcomes and the channels that drive the effect, and (ii) we propose a strategy to identify the spatial diffusion of historical Jewish communities. Grosfeld, Rodnyansky, and Zhuravskaya (2013) find an effect of the Pale of Settlement on the support for socialist/communist parties after the fall of the Soviet Union, on generalized trust, and on the ratio of self-employed workers, but not on economic outcomes. They interpret the results as evidence of anti-market beliefs generated by ethnic

hatred against Jews. In our paper, Jewish persecution is unrelated to generalized trust or political preferences, but is related to economic outcomes. Additionally, the presence of Jews does not explain our results by itself. Recent papers document supply-side effects of Jewish persecution (e.g., Acemoglu, Hassan, and Robinson (2011), Waldinger (2010), Pascali (2014), and Akbulut-Yuksel and Yuksel (forthcoming)). Supply-side channels do not drive our findings.

2 Measuring Historical Jewish Persecution

Building on Voigtlaender and Voth (2012) and Voigtlaender and Voth (2013a), we propose four measures of Jewish persecution 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. They 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 from each German county to a concentration or extermination camp after 1933. 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. German Jews started to hide and live in captivity after the *Kristallnacht* pogrom of 1938. To locate hiding Jews, the Nazis relied on delation by locals. Households in areas where antisemitism 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 years of the Black Death, around 1349. The Black Death was arguably the worst pandemic in human history: it lasted several years, and up to one third of Europeans may have died. A deep economic and social crisis followed, leading historians to refer to this period as one of the darkest eras in western history. 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. In contrast to Voigtlaender and Voth (2012), we do not exploit the number of pogroms in a county. The number of pogroms is certainly meaningful in a city-level analysis, because cities that experienced more pogroms presumably 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, the mere number of cities in the county for which data on pogroms are available, as opposed to the concentration of actual pogroms in a city, would increase this margin of persecution. The number of pogroms in a county for which we observe many cities is high even if each city has only experienced one pogrom.

Principal component of measures. We also compute the first principal component of the three measures described above. The principal component isolates the common variation across the other three alternative margins of persecution. For this reason, the principal component is our preferred measure of Jewish persecution, 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 für Wirtschaftsforschung Berlin* (DIW). Economics research has already used the SOEP data (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 that they do not provide the complete financial portfolios of households; hence, we cannot document how Jewish persecution affects every component of households' financial portfolios, the fraction of wealth invested in stocks, or whether lower stockholdings translate into higher deposits and other safe assets.

We use the data collected by Voigtlaender and Voth (2012) on Jewish persecution in the Middle Ages and during the Nazi period. We aggregate all city-level data at the level

of present-day counties, which is the finest geographical level at which we can observe households' characteristics. We obtain county-level historical characteristics from the *Ifo Prussian Economic History Database*, described in detail by Becker et al. (2014). Finally, we collect current county-level controls: socio-demographics from *DeStatis*, the index of land quality of Ramankutty et al. (2002), and the coordinates of the centroid from *Eurostat*.

Table 1 reports summary statistics at the household level.[11] 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 non-repeated households in the SOEP county-level data set 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.[12] The other missing observations are due to blanks in 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 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.[13] To compute this measure, we exclude 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. However, 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

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

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

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

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 only a high school degree.[14] Households in the running sample live in counties with lower unemployment and fewer blue-collar workers in 1933, and a higher proportion of Catholics in 1925, which allows to control for the religious composition of the local population. 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 present-day 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 is, 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. Panels (c) and (d) of Figure 2 plot the densities of the ratio of Jews deported and present-day stock market participation.[15] 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. Although participation is on average higher in counties with no pogroms, a t-test for the difference between the two means does not reject the null that the means are equal. In Figure A.1 of the Online Appendix, we plot the densities of Jewish persecution for counties within German states (*Bundesländer*). On average, the persecution was higher in states closer to the Rhine Valley than those in Southern, Northern, and Eastern Germany. In Figure A.2

[14]The average ratio of household heads who claim 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.

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

of the Online Appendix, we plot the correlations between stockholdings and all the other measures of Jewish persecution.

3 Jewish Persecution and Current Stockholdings

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

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

where $HoldsStocks_{ik}$ is a dummy that equals 1 if household i in county k holds any stocks, $Persecution_k$ is one of our four measures of Jewish persecution. X_{ik} includes the following individual-level controls: gender, single/marital status; income (2nd degree polynomial); age (2nd degree polynomial); education level; homeownership; and investment in life insurance products. K_k includes the following county-level current and historical controls: latitude; income per capita; share of college-educated population; index of quality of cultivable land; log of population in 1933; log of Jewish population in 1933; unemployment ratio in 1933; share of workers in manufacturing, in retail, and self-employed in 1933; and ratio of Catholic population in 1925. $Income\_deciles$ are dummies indicating the decile of the income distribution to which the household belongs, and Φ is the standard normal cdf.[16] η_t are a set of survey-wave group fixed effects, each capturing a group of four adjacent years.[17] 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.

Table 2 reports the average marginal effects for our baseline specification. In columns (1)-(3), $Persecution_k$ is the first principal component of the three measures of Jewish

[16] 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.

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

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 in our sample. The effect is about one fifth the size of the effect of holding a college degree on the likelihood of investing in stocks. Higher education is one of the largest determinants of stock market participation. 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.

Nazi Persecution. In columns (4)-(7) of Table 2, 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 (4)-(5)). In columns (6)-(7), a one-standard-deviation higher 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. Statistical significance is lower for the intensive margin, which excludes counties without Jewish population in 1933.

Black Death Pogroms. In columns (8)-(9) of Table 2, 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 two percentage points less likely to hold stocks. The size and statistical significance of the effect of individual- and county-level observables on present-day stockholdings are comparable across specifications.

Homogeneous Counties. German counties are likely to differ along several dimensions,

such as geography, history, and the quality of current and historical institutions. Replicating our baseline results when comparing only geographically close counties, which have arguably similar geographic characteristics, and are exposed to similar institutions and cultural heritages, is important. In columns (1)-(4) of Table 3, we report the coefficient for the principal component of the measures of Jewish persecution when augmenting Equation 1 with geographic fixed effects, which allows exploiting only the variation in Jewish persecution across counties close in space. In column (1), we only exploit the variation in Jewish persecution across counties that belong to the same state (*Bundesland*). This specification is quite demanding, because each state includes on average only 15 counties. The estimated magnitudes of the effect are similar to our baseline result. Counties in current German states are exposed to the same local institutions and policies, but are not necessarily exposed to the same historical institutions, because the borders of states do not always coincide with the borders of historical administrative regions. Moreover, the size of states is unbalanced: some states comprise few counties, and hence have little variation in Jewish persecution. To address these issues, in columns (2)-(3) of Table 3, we divide Germany into 9 and 16 squares of similar size by longitude and latitude, and we only exploit the variation in Jewish persecution across counties that belong to the same squares, which are arbitrary partitions of the country.[18] This method also overcomes the endogeneity of state borders. The baseline results are replicated in both specifications. In column (4) of Table 3, we only exploit the variation in Jewish persecution within occupation zones after the Second World War, because Voigtlaender and Voth (2013b) show that the processes of denazification promoted by occupants had different effects on the persistence of antisemitic beliefs across occupation zones. These zones do not perfectly overlap with the state boundaries. This specification also replicates the baseline results.

Counties with documented Jewish communities. Unobservables that favored the settlement of Jewish communities in the distant past could be driving our results thus far, and such determinants may affect stockholdings. To tackle this issue, in column (5) of Table 3, we restrict the sample to households in counties with a documented Jewish

[18]Figure A.4 shows how counties are grouped based on these arbitrary partitions of Germany.

community before the Black Death. The restriction reduces the sample size by 27%, but unobservables that explain the settlement of Jewish communities in the past cannot be driving the variation in persecution across these counties. This specification replicates the baseline results.

Robustness. In columns (6)-(10) of Table 3, we assess the robustness of the baseline results by conducting a set of subsample analyses. In column (6), 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 not properly accounted for by the clustering of the standard errors at the county level. The assumption that correlated realizations are independent could be driving our results. The magnitude of the coefficient on the principal component of persecution is - if anything - larger than in the individual-level specifications, and the precision of the estimates is similar. In column (7), we exclude households in counties in the bottom third of the distribution of present-day income per capita, and in column (8), we exclude households in counties in the bottom third of the distribution based on the share of college-educated inhabitants. The estimated coefficients are slightly higher than in our baseline analysis, and the precision of the estimates is similar. In columns (9)-(10), we run robustness checks excluding households in counties that were wealthy in the distant past, and specifically, counties that hosted any Hanseatic League cities, or any free imperial cities in the Middle Ages. The estimated coefficients are not different from the baseline results.

4 Anti-Jewish Ideology, Nazi Votes, and Stockholdings

Incentives unrelated to ideology may have driven Jewish persecution. Individuals may have hoped to obtain rewards based on the number of Jews they helped the Nazis locate. In the Middle Ages, individuals and political leaders may have hoped to seize Jewish property if they took part or promoted the attacks against Jews.

Unlike persecution, 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. 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. Many voters 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 European voting patterns for extreme-right parties in the 1920s and 1930s. They 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 antisemitic 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 antisemitic sentiment in counties where unemployment was high, that is, where voters were likely driven by economic motives when voting for the Nazis. This argument does not imply the Nazis had higher support, on average, in counties with higher unemployment,[19] but it exploits the different motives for supporting the Nazis *within* the group of Nazi voters in 1933.

We expect a negative association between Nazi vote shares in 1933 and present-day stockholdings in counties where unemployment was low in 1933, and a less negative effect in other counties. We find exactly this pattern. In Figure 3, the left vertical axis reports the average marginal effect of the Nazi county-level vote share in 1933 elections on 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

[19]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.

where unemployment in 1933 was low, up to the 45th percentile of the distribution. The magnitude of the effect and its statistical significance decrease up to households in the 80th percentile of the unemployment distribution in 1933. The estimated effect becomes economically small and statistically insignificant once we add households in counties above the 85th percentile of the distribution of unemployment in 1933.

Placeboes. The media affect hatred-based voting behavior. Adena et al. (2013) show that based on who controlled the aired content, the radio had opposite effects on the vote shares of the Nazis. When the content was anti-Nazi (1929-1932), the radio reduced the vote share of the Nazis. Once the Nazis obtained control of the radio, it had a large positive effect on their vote share (March 1933). In Figure 4 Panel (a), we find a virtually identical pattern as in Figure 3 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 4 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 antisemitists more than the Nazis. In Panels (d), (e), and (f) of Figure 4, we show that the Nazi votes are not significantly associated with present-day outcomes other than stockholdings conditional on unemployment in 1933. They are uncorrelated with the likelihood that households invest in life insurance products, that the household head is a woman, or with the households' income. In Figure A.5 of the Online Appendix, we find no significant associations if using the age, the education level, or the homeownership status of the household head as alternative outcomes.

5 Channels Mediating the Effect of Jewish Persecution on Stockholdings

In this section, we discuss the channels through which Jewish persecution may affect the likelihood that present-day inhabitants invest in stocks. We consider demand- and

supply-side channels. Demand-side channels may mediate the negative effect of Jewish persecution on stock market participation through the present-day demand for stocks. Supply-side channels may mediate the negative effect through a historically lower supply of financial investment opportunities in counties with higher anti-Jewish sentiment.

Validation of the proxies for distrust in finance. We first need to validate our proxies and test if deep-rooted antisemitic sentiment indeed predicts distrust in financial markets. We do not observe distrust in finance for the households in the SOEP 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. Survey respondents scroll a slider that runs from 0 to 100 at increments of one unit. The number they pick is their measure of trust in financial markets.[20] 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 demand effects invalidating the procedure. We also ask for some demographics including the zip code, which we map into counties. In Figure A.6 of the Online Appendix, we report the average levels of trust across respondents' 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 five respondents.[21] 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 Table 4, we estimate tobit specifications at the individual-respondent level, because trust in finance is measured as an integer between 0 and 100. In column (1), the unconditional correlation of trust in financial markets and the principal component of the measures of Jewish persecution is negative, although we detect no statistical

[20]The screenshot of the survey is in Figure A.6 of the Online Appendix. The text of the main question reads "Finden Sie die Idee einen Teil Ihres Vermoegens in Aktien zu investieren behaglich?" (Are you comfortable with the idea that part of your wealth is invested in stocks?).

[21]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 representative of the level of trust in a county for counties where we have too few respondents.

significance. In columns (2)-(4), we add individual respondents' characteristics and current and historical county characteristics, and we limit the variation within state. In all three specifications, a one-standard-deviation increase in Jewish persecution reduces trust in finance by about three 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 Jewish persecution on stockholdings, which amounts to 7.5% of the average likelihood of holding stocks in the SOEP sample. The similarity of the magnitudes of the effects is important to establish that distrust in finance can drive the effects not only qualitatively, but also quantitatively. In Figure 5, we plot the negative unconditional correlation of Jewish persecution and trust in financial markets when averaging our measure at the county level.

Demand-side Channels and Education. We consider three demand-side channels. First, Jewish persecution may capture households' backwardness, which includes cultural cues and beliefs that promote distrust toward the unfamiliar. Second, current households which are antisemitic may still associate the stock market with Jews, and thus invest less in stocks. Third, distrust in finance may be a by-product of historical 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 antisemitic 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 Panels B and C of Figure 6, we replicate the drop in backwardness and antisemitism for 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 us for the first time to observe whether 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.[22]

Backwardness or present-day antisemitism could explain the effect only if the size of the effect decreased with the education level of households. But the main effect and the distrust in finance are strikingly similar across education levels. In Table 5, we test for the interaction of our baseline effect with the level of education of household heads. Panel A of Table 5 shows that the baseline effect of Jewish persecution on 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.[23] In Panel B of Table 5, 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 persecution on stockholdings does not vary systematically with the average education level. This path is not consistent with generic backwardness or present-day antisemitism, but it is consistent with a cultural norm of distrust in finance that has transmitted independently from antisemitism across generations. Education curricula affect pupils' ideology and shape cultural norms (Cantoni et al. (2014)). German curricula address the issue of antisemitism, decreasing the antisemitism of students. But education curricula seldom address the benefits of stock market participation, meaning that prejudice against financial institutions is not decreased alongside antisemitism.

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 diffused early into more antisemitic areas, current households in those areas might be less aware of stock investment. We collect novel data on the foundation dates of credit unions across German counties from the proprietary registry of the *Hoppenstedt*

[22]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.

[23]This result also helps disentangle distrust in finance from generalized trust, the effect of which is driven by the least educated (Guiso et al., 2004).

Firmendatenbank.[24] Panel A of Figure 7 shows the diffusion path of credit unions. Before 1860, few credit unions existed in the current southern state of Baden-Württemberg and in the eastern states of Brandenburg and Sachsen-Anhalt. Credit unions diffused in the south and center of the country, and a few banks were founded in the west. As of 1900, credit unions had diffused throughout the whole west and the whole south, as well as the north and the center of Germany. The bottom-right picture of Panel A shows that credit unions can be found in every German county today. 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.

Another supply-side channel concerns the skill structure of counties. Persecution may have reduced local financial services because a large share of finance workers were Jewish. If the depletion of human capital needed to run financial institutions drove our baseline effect, the effect should be larger in counties with a higher ratio of Jewish workers in finance. In Panel A of Figure 8, we find no association of the ratio of Jews in finance as of 1882 on present-day stockholdings. In Panel B, we proxy the ratio of Jews in finance as of 1933 with the ratio of Jews in the total population in 1933, and we find no association between the main effect and the ratio of Jews in a county.

6 Properties of the Effect of Jewish Persecution on Stockholdings

Effect over time. First, we investigate whether the magnitude of the effect varies over time. The SOEP data allow us to observe stock market participation from 1984 until 2010, that is, for more than 25 years. In these years, stock market participation varied substantially. The German stock market has also witnessed periods of booms and busts: the crash of 1987, the bull markets of the "dot-com bubble," and the financial crisis of 2008.

[24] 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.

In Table 6, we estimate Equation 1 adding a set of interactions between each measure of Jewish persecution and wave-group dummies capturing periods of four years.[25] All other controls are the same as in Table 2. The omitted category is the most recent group of waves, from 2008 to 2010. If the effect of antisemitic 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 were stable over time, the coefficients attached to the interaction terms would be indistinguishable from zero. In all the columns of Panel A of Table 6, the effect of Jewish persecution for the period 2008 to 2010, 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. Individuals born before the end of the Second World War have also experienced the massive antisemitic Nazi propaganda. In Panel B of Table 6, we test how the impact of Jewish persecution 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;[26] 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 are concentrated in the second half. The average stock market participation differs substantially across the two periods. The effects we estimate for the Postwar cohort are similar to the baseline estimates in Table 2 in terms of magnitude and statistical significance. The interaction effect disappears for households in the Nazi-period cohort, because everyone in this cohort invests little in stocks, and the variation in Jewish persecution has no predictive power. Two explanations seem plausible. The massive antisemitic propaganda of the Nazis may have eliminated the cross-sectional variation of anti-Jewish sentiment across counties,

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

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

making it impossible for the variation in Jewish persecution to explain stockholdings.[27] Alternatively, the economic crises and social disruptions witnessed by the Nazi-period cohort may be responsible for the generalized lower investment in stocks. In a parallel historical event, Malmendier and Nagel (2011) show that U.S. cohorts that experienced the Great Depression are less likely to invest in stocks. The effect of persecution 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. Counties where the level of unemployment was high drive the reversal of the effect of persecution for the Nazi-period cohort. 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 the 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 Jewish persecution 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.

7 Forced Migrations, Jewish Persecution, and Stockholdings

Unobservable characteristics of German counties may have jointly determined Jewish persecution and stockholdings. Comparing geographically close counties helps alleviate

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

this concern, but this test may not be enough to establish a causal effect of Jewish persecution on present-day financial decisions. Ideally, we would exploit a shock to the extent of Jewish persecution unrelated to stockholdings. Because localized anti-Jewish sentiment has persisted since the Middle Ages, the shock should have happened before the Black Death of 1349.

To get close to such an ideal shock, we look at the forced migrations of Ashkenazi Jews out of the Rhine Valley after the 11^{th} century. In Panel (a) of Figure 9, 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. Research has found evidence of Jewish communities in the 10^{th} century along the entire Rhine Valley.[28] 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.[29] In Panels (b) and (c) of Figure 9, we show the location of the cities of Trier, on the Mosel, and Emmerich, on the northern end of the German Rhine. The age of the first documented Jewish community in a county increases as one moves towards each of these cities.

We argue that the distance from the Rhine Valley determined the existence of Jewish communities at the time of medieval persecutions. In a first step, we use the distance of a county from the Rhine Valley to predict the probability that a Jewish community existed in the county before the Black Death. In a 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

[28] We refer to Toch (2012) as a comprehensive economic history of European Jews in the Middle Ages.

[29] 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.

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 because of the mere presence of Jews. Of course, we will not necessarily observe a positive realization, that is, a pogrom in all of these counties ex post. In a third step, we use the predicted probability of persecution at the county level to predict the likelihood of stock investments at the household and county levels.

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 bishop siege, free imperial cities, Hanseatic League cities, a city incorporated before 1349, and geographic characteristics (latitude, the land quality index of Ramankutty et al. (2002), and 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 cities of Trier, on the Mosel, and Emmerich, on the northern end of the German Rhine (Figure 9). The lowest of these two distances is our third measure. The shortest distance is about 2 km, whereas the greatest distance is 1100 km. The alternative measures aim to capture two gradients of the distance from the Rhine Valley, that is, the south-west/north-east gradient and the north-west/south-east gradient. As Panels (b) and (c) of Figure 9 show, counties lying at the same distance from each of the two cities, that is, on the same *isodistance curves*, are different; hence, our measures indeed capture two alternative gradients of the distance from the Rhine Valley. Across both gradients, the likelihood that a Jewish community existed in the Middle Ages increases towards the Rhine Valley. To make our specifications directly comparable to the baseline results, we include the latitude of counties when computing the residual distances, but all the results are virtually identical if we exclude the latitude from the vector K_k in Equation 2.

Identification concerns. The shock helps us identify a causal effect of Jewish

persecution on present-day stockholdings only if the residual distance from the Rhine Valley does not affect stockholdings through channels different from the persecution of Jewish communities. We propose a set of results to assess the plausibility of this exclusion restriction. In Table 7, 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 8, 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 antisemitic 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 stockholdings drop in magnitude, whereas the estimated standard errors for each coefficient stay virtually identical. Hence, the insignificant effects are not due to the imprecision of our estimates.

In Panel B of Table 8, we propose a placebo test for an association of the distance from the Rhine Valley and stockholdings through channels other 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 economically or statistically significant association between the distance from the Rhine and the stockholdings of French households. In the Online Appendix, 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 9 reports results for estimating the stages described above in a three-stage OLS framework (see Becker and Woessmann (2009)) at the county level,[30]

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

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

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

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 9 report the coefficient on $LogDistanceRhine_k$ when estimating the system using the three alternative measures of distance from the Rhine Valley. In column (1), a one-standard-deviation increase in the residual distance from Trier (0.69) reduces by nine 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.[31] Hence, the farther a German county is from the Rhine, the less likely a Jewish community existed there in 1096. In columns (4)-(6) of Table 9, 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

[30]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 results for variables aggregated at the county level in all stages to be more conservative.

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

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 9 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.

8 Conclusions

We use the geographic variation in Jewish persecution across German counties to proxy for localized distrust in finance. A one-standard-deviation increase in persecution 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 two percentage points less likely to invest in stocks. The results are robust to using only the variation in persecution across geographically close counties. The magnitude of the effect is stable from 1984 until 2010 and across cohorts. Plausible supply-side channels do not mediate the effect of Jewish persecution on stockholdings. The results are not driven by present-day antisemitism, or by the backwardness of households. The results are consistent with the notion that distrust in finance has transmitted across generations independently from antisemitism, because the effect of Jewish persecution on stockholdings does not vary across education levels.

The results contribute to the interdisciplinary debate on hatred beliefs and their long-term consequences on societies. They suggest that policies designed to eliminate race- and religious-based persecution should not only be justified by the promotion of human rights, but also by the wealth of the broader population. The results may suggest an economic paradigm to justify policies against hatred beliefs. While several non-Western cultures do not share the ethical notion of the primacy of individual interests over the collective ones, promoting the wealth of societies is universally accepted. Cultures that do not promote human rights may be persuaded by evidence that the persecution of minorities reduces not only the long-term wealth of the persecuted, but of the persecutors as well.

References

D. Acemoglu, T. Hassan, and J. Robinson. Social Structure and Development: A Legacy of the Holocaust in Russia. *Quarterly Journal of Economics*, 126(2), 2011.

M. Adena, R. Enikolopov, M. Petrova, V. Santarosa, and E. Zhuravskaya. Radio and the Rise of the Nazis in Prewar Germany. *PSE Working Paper 2013-32*, 2013.

M. Akbulut-Yuksel and M. Yuksel. The Long-Term Direct and External Effects of Jewish Expulsions in Nazi Germany. *American Economic Journal: Economic Policy*, forthcoming.

Y. Algan and P. Cahuc. Inherited Trust and Growth. *Journal of Political Economy*, 114(2):280–316, 2006.

E. Banfield. The Moral Basis of a Backward Society. The Free Press, 1958.

S. Becker and L. Woessmann. Was Weber Wrong? A Human Capital Theory of Protestant Economic History. *Quarterly Journal of Economics*, 124(2):531–596, 2009.

S. Becker, F. Cinnirella, E. Hornung, and L. Woessmann. iPEHD - The Prussian Ifo Economic History Database. *Historical Methods*, 47(2):57–66, 2014.

S. Betermier, T. Jansson, C. Parlour, and J. Walden. Hedging Labor Income Risk. *Journal of Financial Economics*, 105(3):622–639, 2012.

M. Botticini and Z. Eckstein. Jewish Occupational Selection: Education, Restriction or Minorities? *Journal of Economic History*, 65(4):922–948, 2005.

M. Botticini and Z. Eckstein. The Chosen Few. *Princeton University Press*, 2012.

J.C. Buggle. Law and Social Capital: Evidence from the Code Napoleon in Germany. *Working Paper*, 2013.

K. Burchardi and T. Hassan. The Economic Impact of Social Ties: Evidence from German Reunification. *Quarterly Journal of Economics*, 128(3):1219–1271, 2013.

D. Cantoni, Y. Chen, D. Yang, N. Yuchtman, and J. Zhang. Curriculum and Ideology. *Working paper*, 2014.

F. D'Acunto. History & Finance: a Nascent Field. *Working Paper*, 2014.

A. de Bromhead, B. Eichengreen, and K. O'Rourke. Political Extremism in the 1920s and 1930s: Do the German Lessons Generalize? *Journal of Economic History*, 73(2): 371–406, 2013.

O. Decker, J. Kiess, and E. Braehler. Die Mitte im Umbruch: Rechtextreme Einstellungen in Deutschland 2012. *Bonn: Verlag J.H.W. Dietz Nachf. GmbH*, 2012.

E. Elhaik. The Missing Link of Jewish European Ancestry: Contrasting the Rhineland and the Khazarian Hypotheses. *Genome Biology and Evolution*, 5(1):61–74, 2012.

U. Z. Engelman. The Rise of the Jew in the Western World. *New York: Behrman's Jewish Book House*, 1944.

N. Fuchs-Schuendeln and M. Schuendeln. Precautionary Savings and Self-Selection: Evidence from the German Reunification Experiment. *Quarterly Journal of Economics*, pages 1085–1120, 2005.

N. Gennaioli, A. Shleifer, and R. Vishny. Finance and the Preservation of Wealth. *Quarterly Journal of Economics, forthcoming*, 2014a.

N. Gennaioli, A. Shleifer, and R. Vishny. Money Doctors. *Journal of Finance, forthcoming*, 2014b.

M. Giannetti and T. Wang. Corporate Scandals and Household Stock Market Participation. *Working Paper*, 2014.

P. Giuliano and A. Spilimbergo. Growing Up in a Recession. *Review of Economic Studies*, forthcoming.

E. Glaeser. The Political Economy of Hatred. *Quarterly Journal of Economics*, 120(1): 45–86, 2005.

I. Grosfeld, A. Rodnyansky, and E. Zhuravskaya. Persistent Antimarket Culture: A Legacy of the Pale of Settlement after the Holocaust. *American Economic Journal: Economic Policy*, 5(3):189–226, 2013.

L. Guiso and T. Jappelli. Awareness and Stock Market Participation. *Review of Finance*, 9(4):537–567, 2005.

L. Guiso, T. Jappelli, and D. Terlizzese. Income Risk, Borrowing Constraints and Portfolio Choice. *American Economic Review*, 86:158–172, 1996.

L. Guiso, P. Sapienza, and L. Zingales. The Role of Social Capital in Financial Development. *American Economic Review*, 94(3):526–556, 2004.

L. Guiso, P. Sapienza, and L. Zingales. Trusting the Stock Market. *Journal of Finance*, 63(6):2557–2600, 2009.

H. Hong, J.D. Kubic, and J. Stein. Social Interaction and Stock Market Participation. *Journal of Finance*, 59(1):137–163, 2004.

G. King, O. Osen, M. Tanner, and A. Wagner. Ordinary Economic Voting Behavior in the Extraordinary Election of Adolf Hitler. *Journal of Economic History*, 68(4):951–996, 2008.

U. Malmendier and S. Nagel. Depression Babies: Do Macroeconomic Experiences Affect Risk-Taking? *Quarterly Journal of Economics*, 126(1):373–416, 2011.

N Nunn. Historical Development. *Handbook of Economic Growth*, 2013.

L. Pascali. Banks and Development: Communities in the Italian Renaissance and Current Economic Performance. *Review of Economics and Statistics*, 2014.

C. Paxson. Borrowing Constraints and Portfolio Choice. *Quarterly Journal of Economics*, 105(2):535–543, 1990.

R. Putnam. Making Democracy Work. Princeton University Press, 1993.

N. Ramankutty et al. The Global Distribution of Cultivable Lands. *Global Ecology and Biogeography*, 11:377–392, 2002.

P. Sapienza and L. Zingales. A Trust Crisis. *International Review of Finance*, 12(2): 123–131, 2012.

Y. Spitzer. Pogroms, Networks, and Migration. *Working Paper*, 2014.

E. Spolaore and R. Wacziarg. How Deep Are the Roots of Economic Development? *Journal of Economic Literature*, 51(2):325–369, 2013.

M. Toch. The Economic History of European Jews: Late Antiquity and Early Middle Ages. *Leiden: Brill Academic Publishers*, 2012.

M. van Rooij, A. Lusardi, and R. Alessie. Financial Literacy and Stock Market Participation. *Journal of Financial Economics*, 101(2):449–472, 2011.

J. van Straten. Jewish Migrations from Germany to Poland: the Rheinland Hypothesis Revisited. *The Mankind Quarterly*, pages 367–384, 2004.

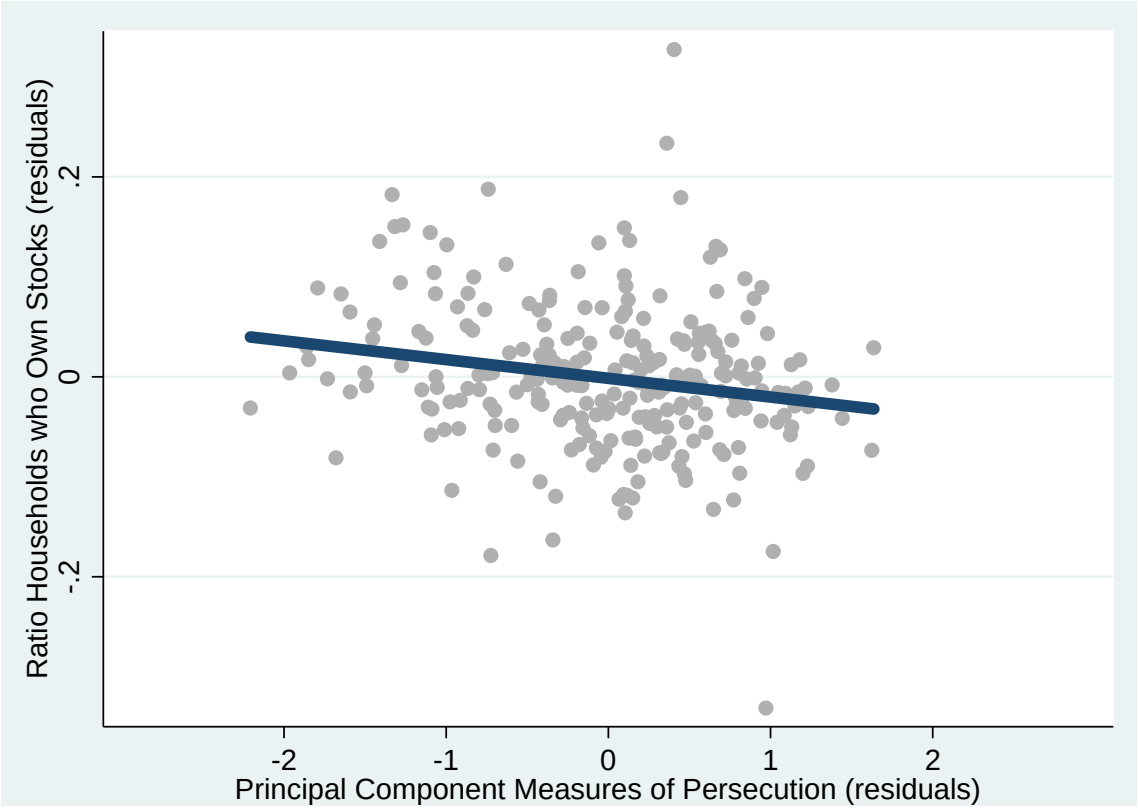
N. Voigtlaender and H.J. Voth. Persecution Perpetuated: The Medieval Origins of Anti-Semitic Violence in Nazi Germany. *Quarterly Journal of Economics*, 127(3):1339–1392, 2012.

N. Voigtlaender and H.J. Voth. Married to Intolerance: Attitudes towards Intermarriage in Germany, 1900-2006. *American Economic Review Papers and Proceedings*, 103(3): 79–85, 2013a.

N. Voigtlaender and H.J. Voth. Exorcizing Hitler: Denazification and Anti-Semitism in Germany, 1933-2006. *CEPR Discussion Paper 8935*, 2013b.

F. Waldinger. Quality Matters: The Expulsion of Professors and the Consequences for PhD Students Outcomes in Nazi Germany. *Journal of Political Economy*, 118(4):787–831, 2010.

Figure 1: **Jewish Persecution and Present-Day Stock Market Participation across German Counties**



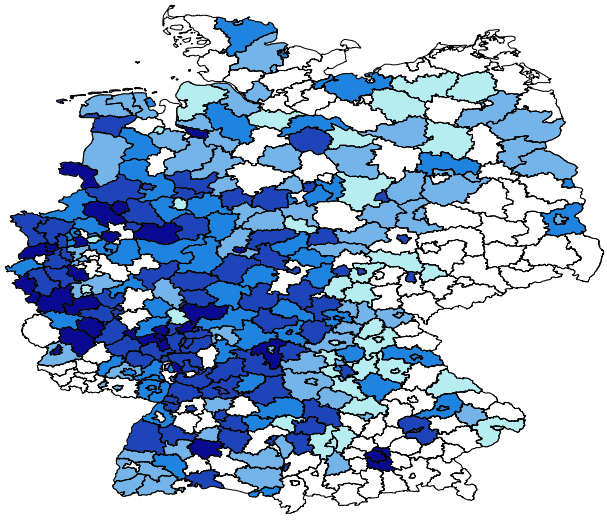
Each point is a German county. The vertical axis plots the residuals from estimating the following equation,

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

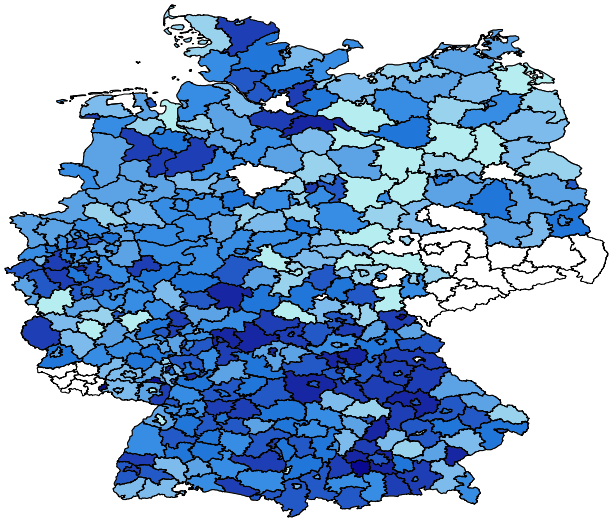
where K_k is the set of county-level observables of Equation 1 in the paper, against the residuals of a regression of the principal component of the measures of Jewish persecution on the same set of covariates, K_k.

Figure 2: **Data Properties: Jewish Persecution and Stock Market Participation**

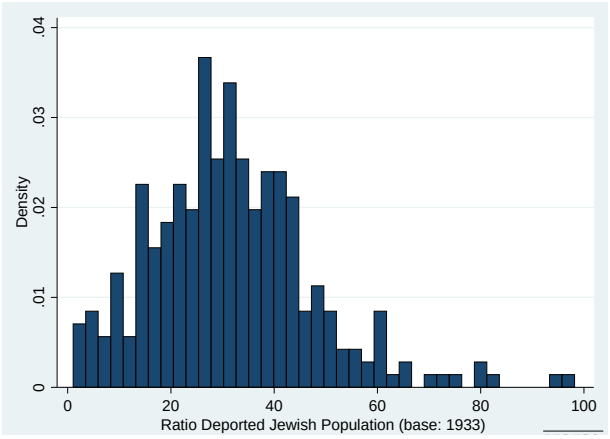
(a) Jewish Persecution Nazi Period



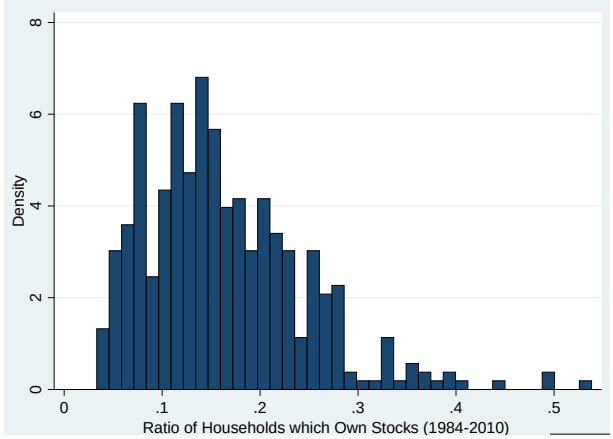
(b) Present-day Stock Market Participation



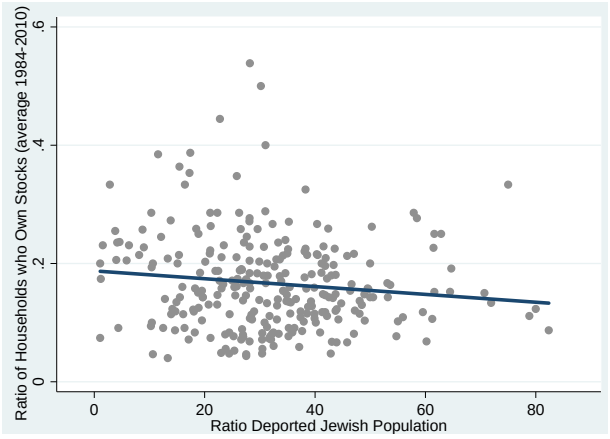
(c) Jewish Persecution Density



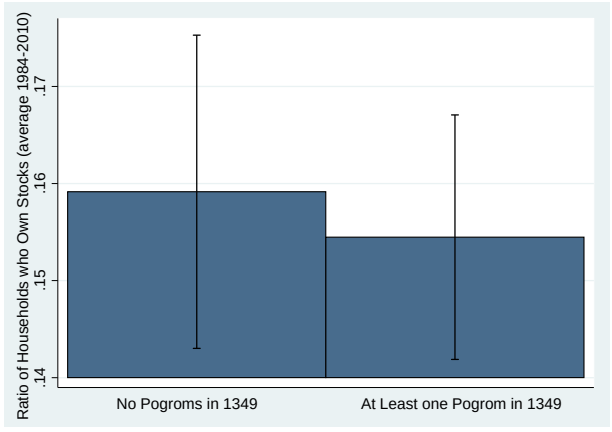
(d) Stock Market Participation Density



(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: **Nazi Votes, Economic Crisis, and Stock Market Participation**

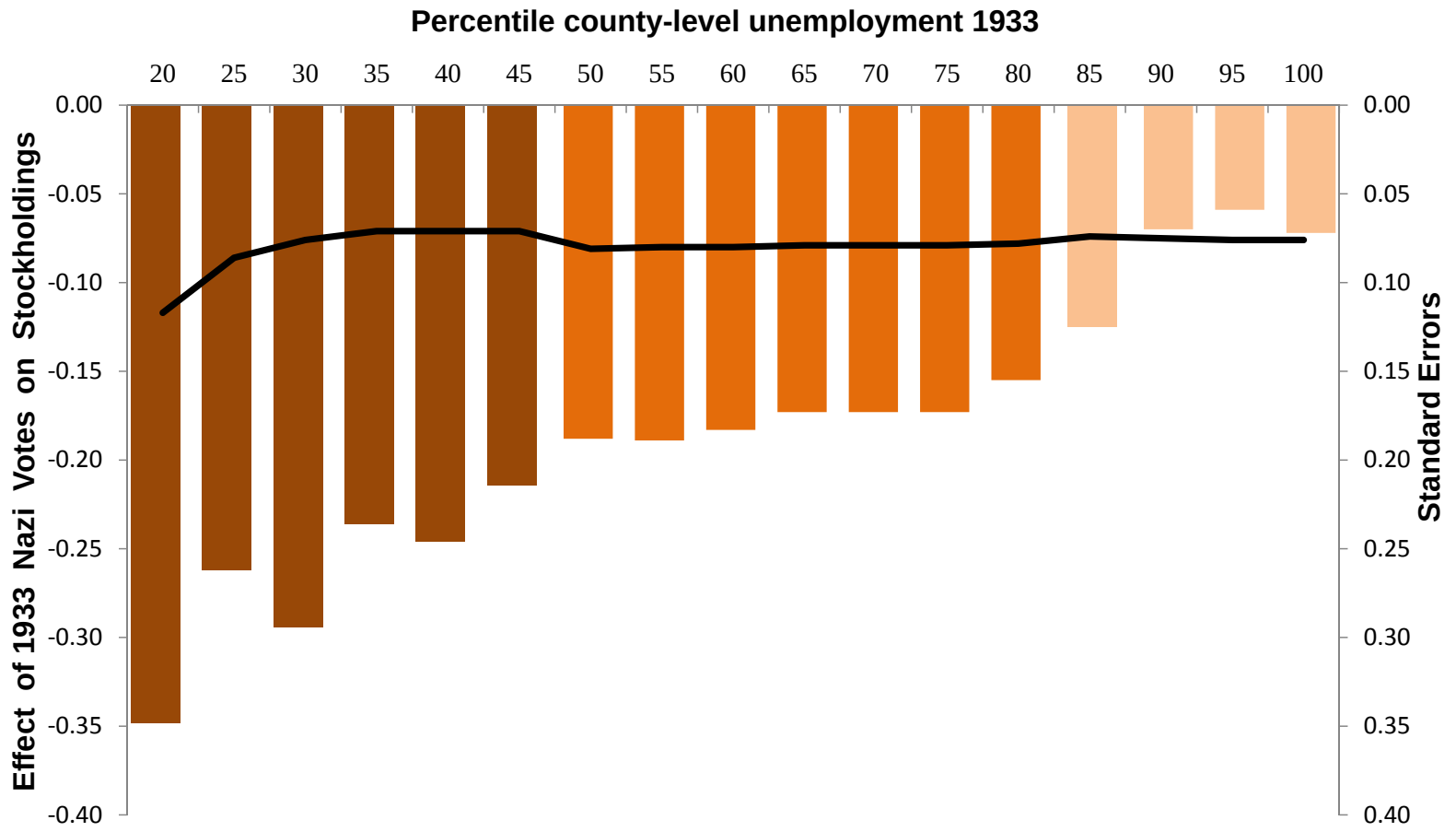


Figure 3 plots the average marginal effects of the vote share for the Nazi Party in 1933 when estimating the following probit specification across subsamples of households sorted by the unemployment rate in their county as of 1933:

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

Each observation is a German household interviewed for the first time by the SOEP between 1984 and 2010. 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 are significant at the 5% level; white histograms are not significant at any conventional level.

Figure 4: **Economic Crisis and Stock Market Participation: Other Parties and Placebo Outcomes**

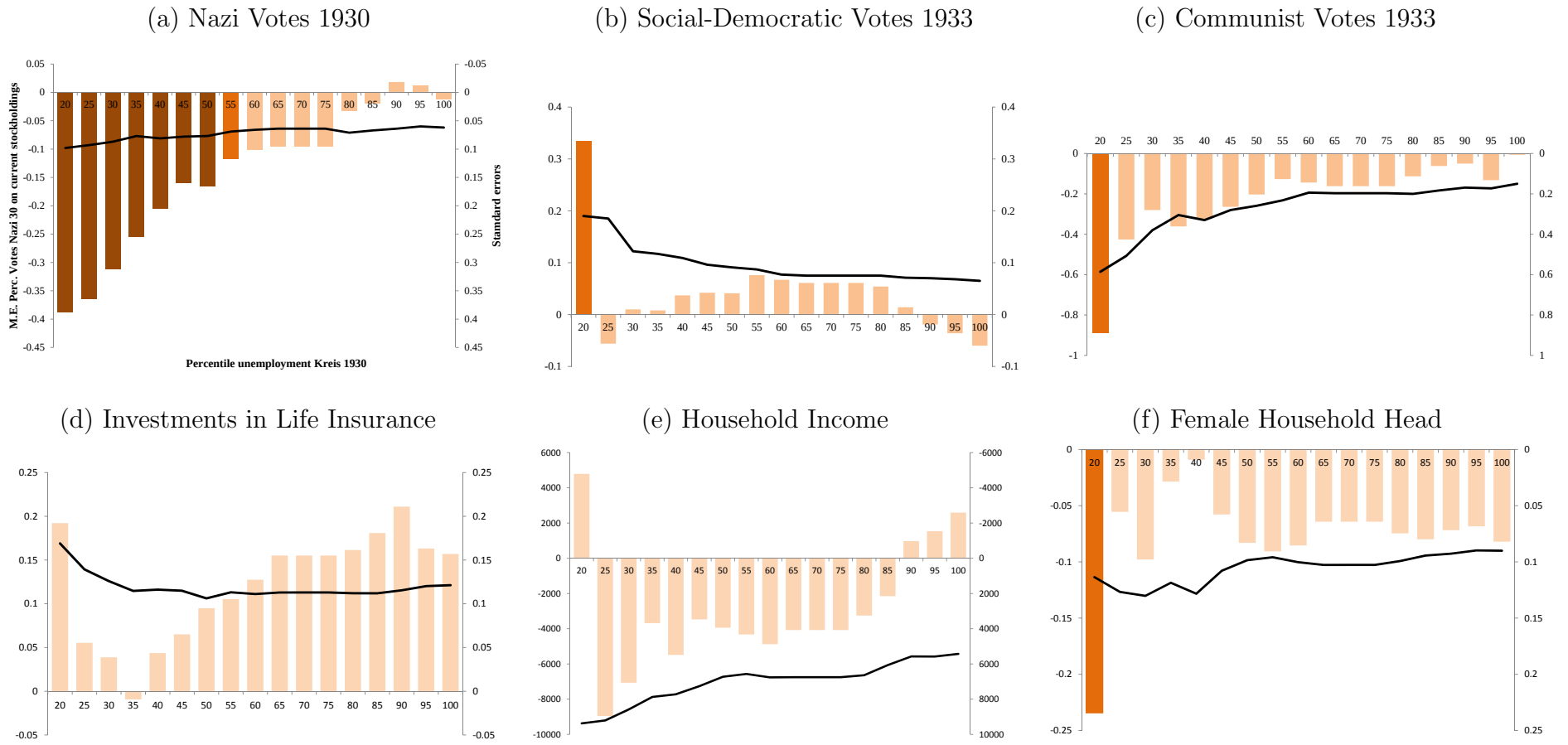
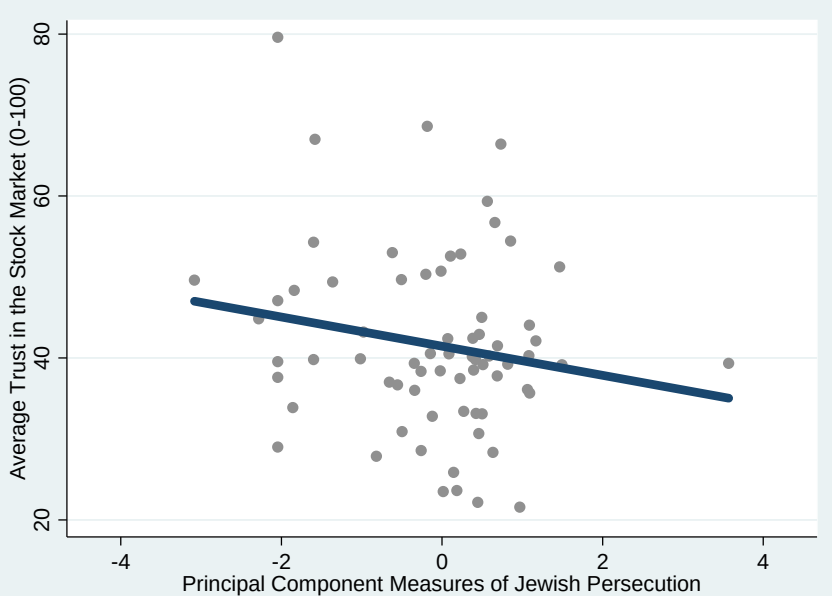


Figure 4 plots the average marginal effects of the vote share for the Nazi Party in 1933 on several outcomes computed from the following OLS specifications across subsamples of households sorted by the unemployment rate in their county of residence as of 1933:

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

Each observation is a German household interviewed by the SOEP any time between 1984 and 2010. $DepVar$ 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 which 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 are significant at the 5% level; white histograms are not significant at any conventional level.

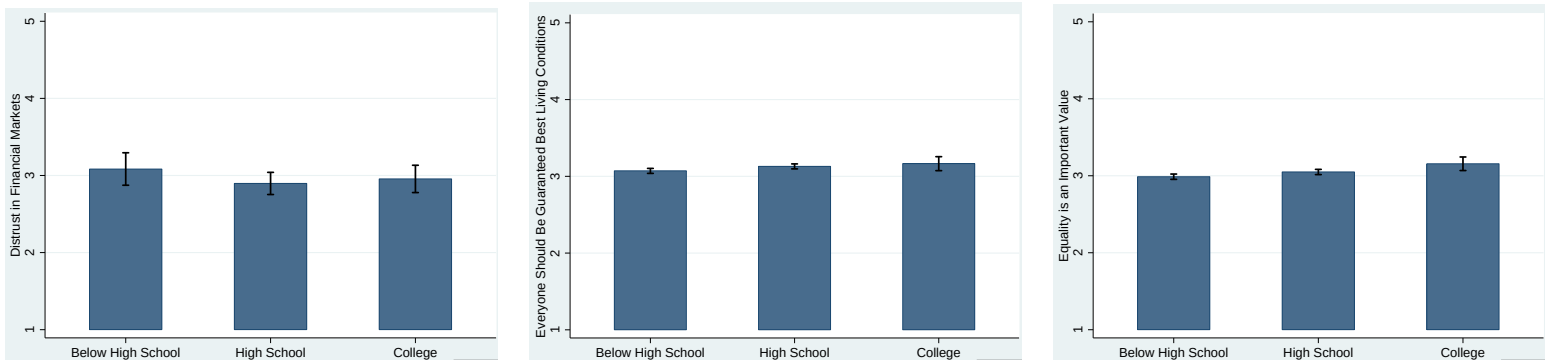
Figure 5: **Jewish Persecution and Trust in Finance: County-level Averages**



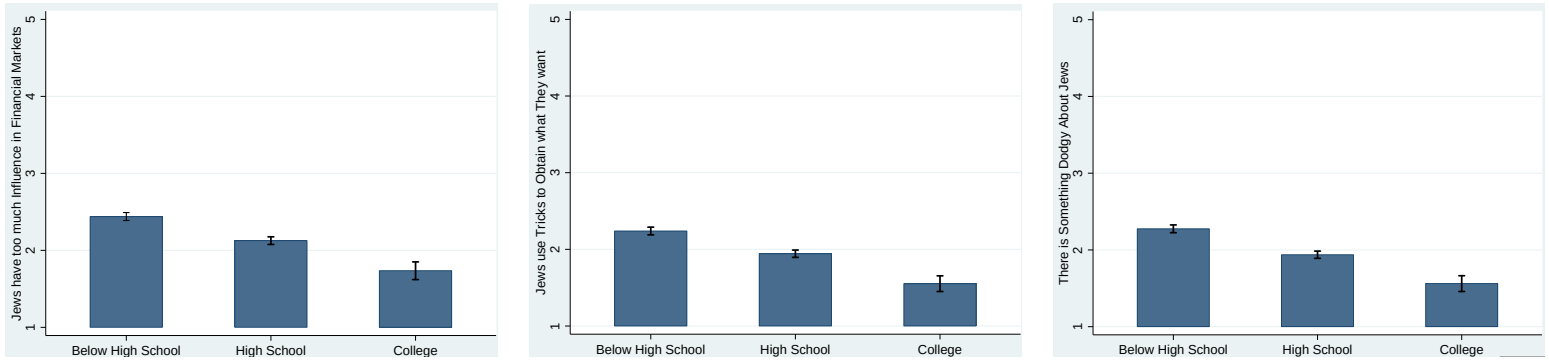
In Figure 5, the vertical axis reports the county-level average trust in financial markets self-reported on an integer scale between 0 and 100 by a pool of German respondents surveyed by the authors through the company *Clickworker*. The horizontal axis reports the principal component of the measures of Jewish persecution at the county level.

Figure 6: **Distrust in Finance, Antisemitism, and Backwardness by Education Levels**

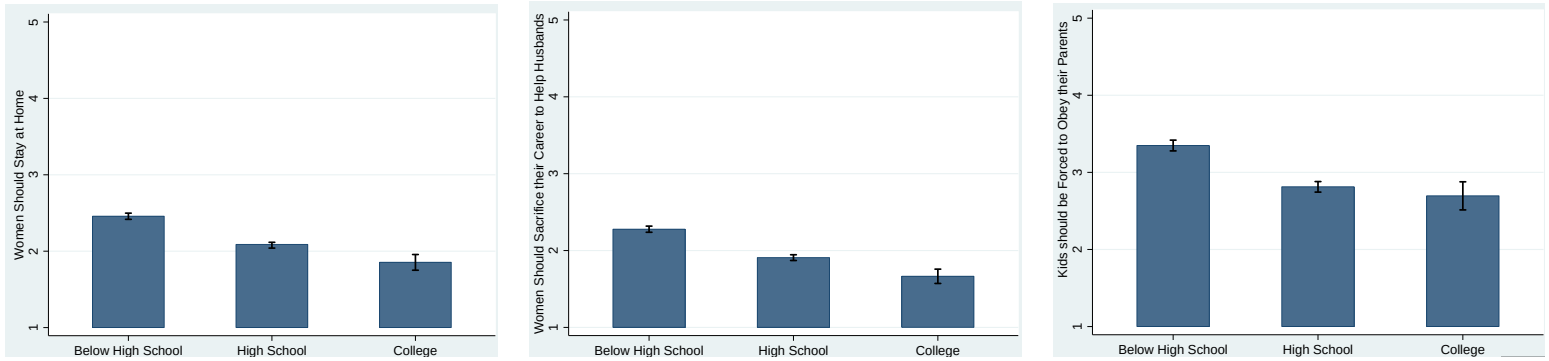
(a) Distrust in Finance and Neutral Values



(b) Present-day Antisemitism



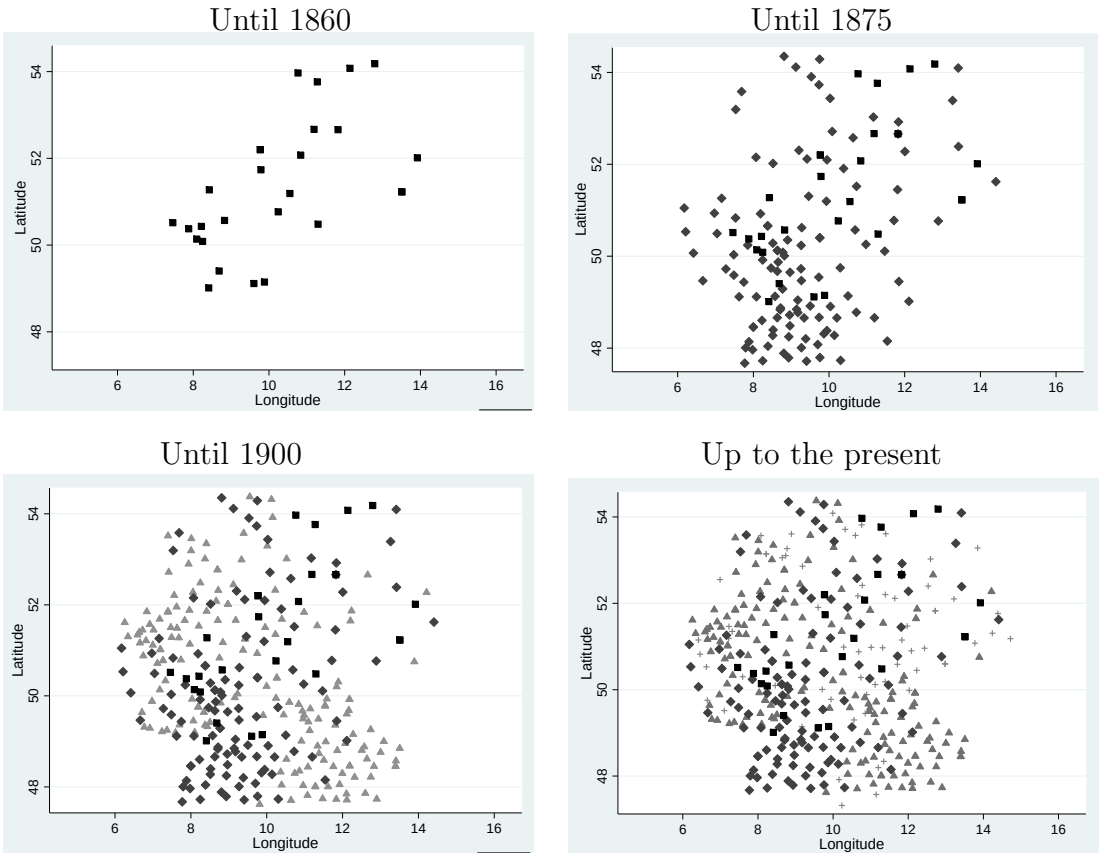
(c) Present-day Backwardness



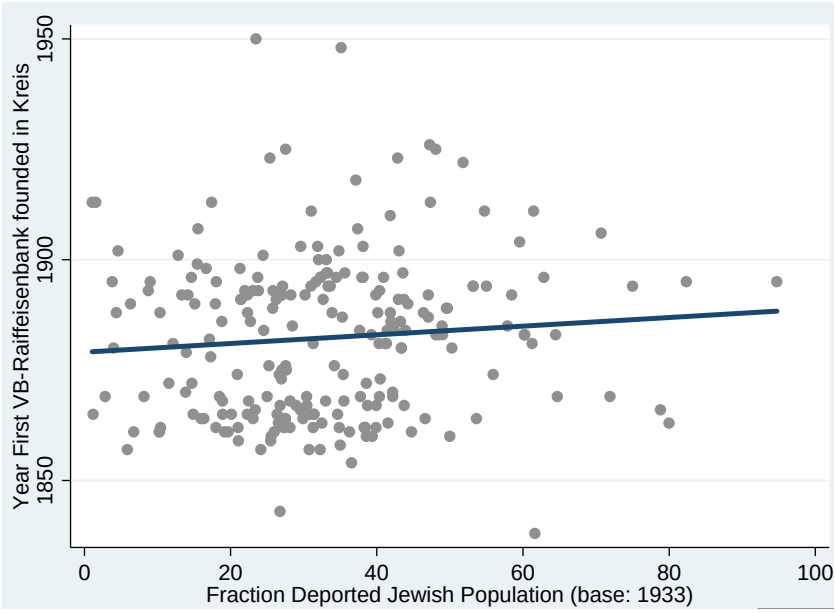
Each graph in Figure 6 reports the sample mean of the outcomes described on the vertical axes split in three groups sorted by the respondent's education level: *Below High School* includes respondents who have a *Realschule* degree or lower level of education; *High School* includes respondents who have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

Figure 7: **Supply-side Channel 1: Jewish Persecution and the Diffusion of Credit Unions**

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



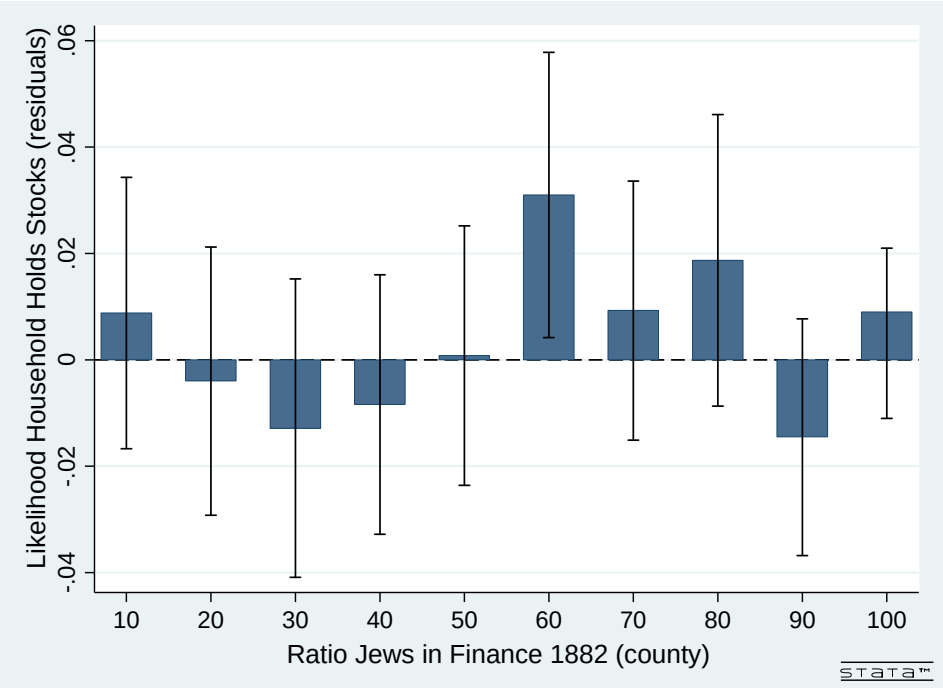
(b) Jewish Persecution and the Foundation of Credit Unions



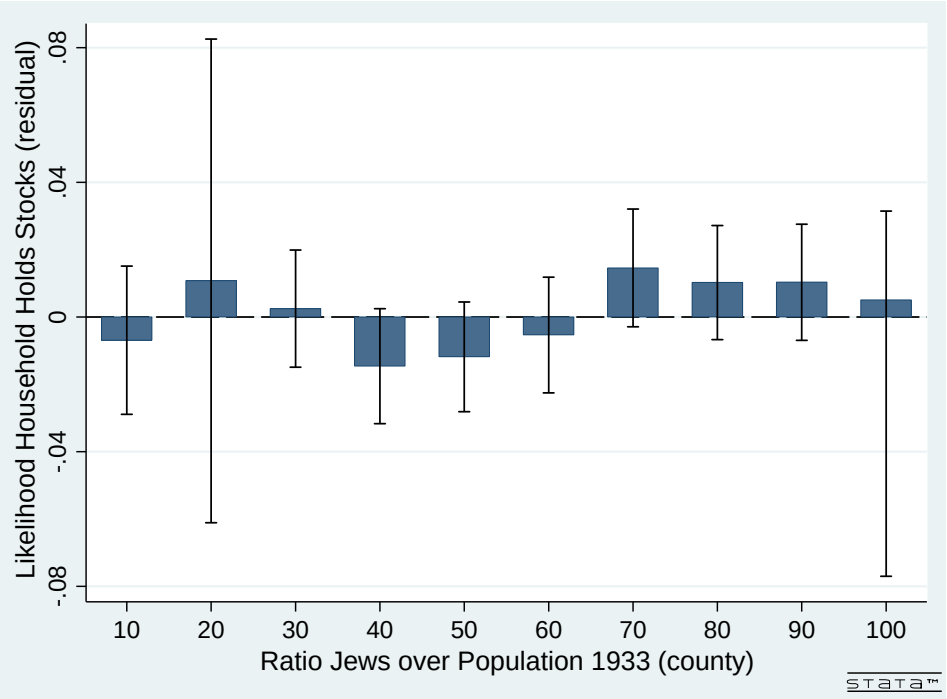
Panel (a) of Figure 7 plots the Germans counties where credit unions were first introduced at different points in time. Panel (b) of Figure 7 plots the correlation between the year when the first credit union of a county was founded and the ratio of Jews deported during the Nazi period over the total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

Figure 8: **Supply-side Channel 2: Jews in Finance and Stockholdings**

(a) Ratio of Jews in Finance in 1882 and Stockholdings



(b) Ratio of Jews in 1933 and Stockholdings



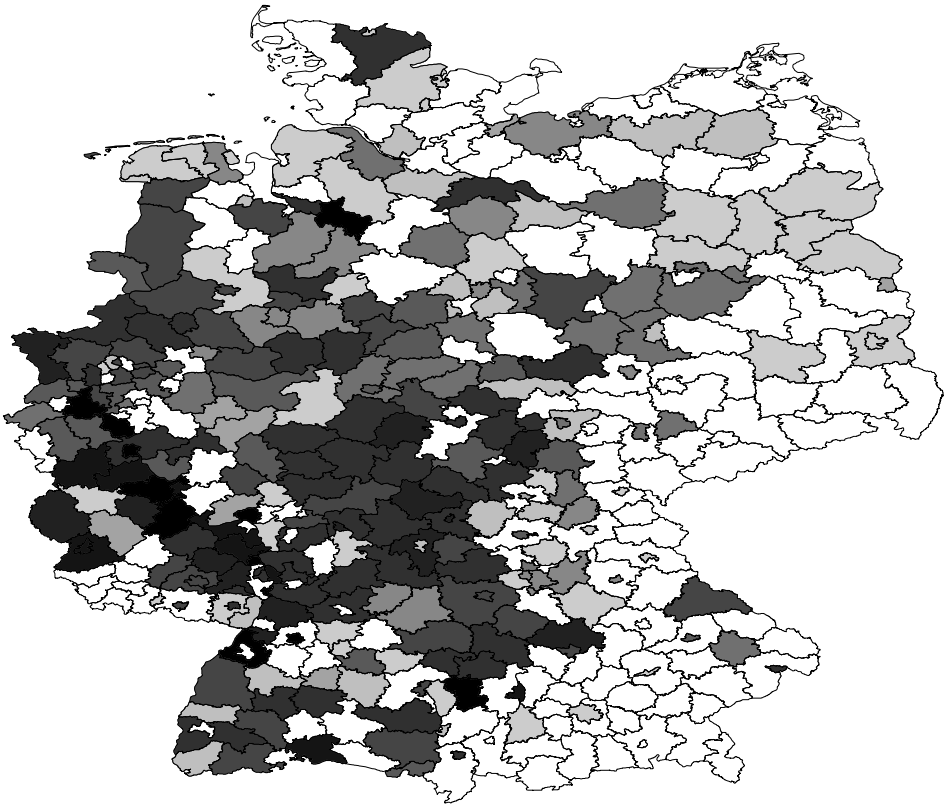
Panel (a) and Panel (b) of Figure 8 plot the average residuals from the following OLS equation,

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

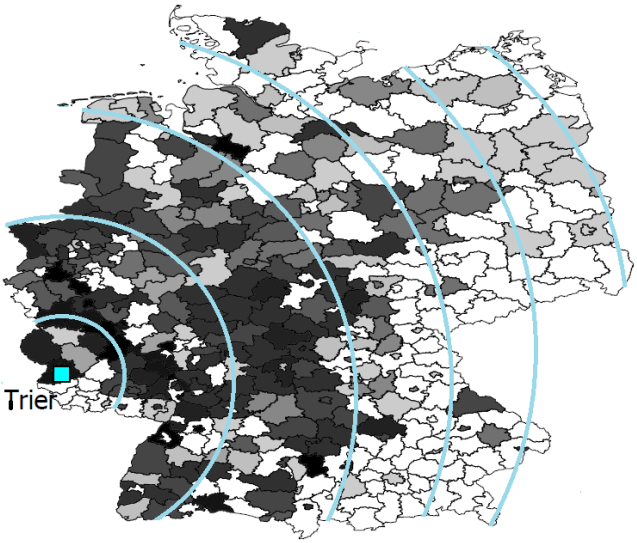
across the deciles of the distribution of the share of financial employees of the Jewish religion in a county as of 1882 and of the ratio of Jews over the total German population in a county as of 1933. Intervals represent 95% confidence intervals for the estimated averages.

Figure 9: **Distance from the Rhine and Jewish Communities in the Middle Ages**

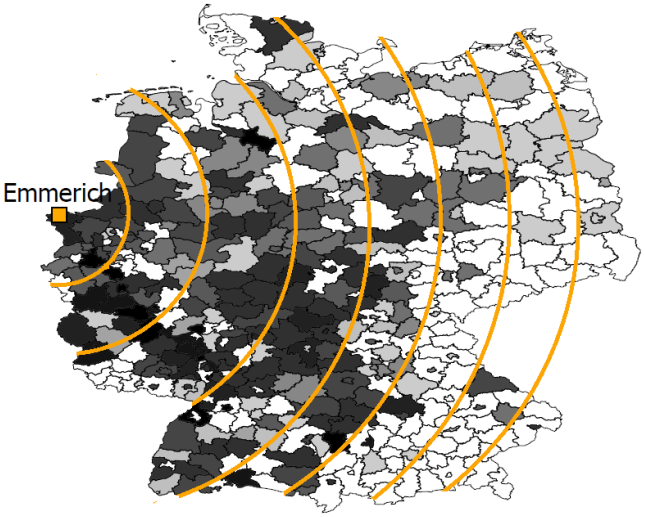
(a) Year when the first Jewish community was documented



(b) Counties at similar distance from Trier



(c) Counties at similar distance from Emmerich



In the maps of Figure 9, 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 bottom maps show the location of the cities of Trier, on the Mosel, and Emmerich, on the northern end of the German Rhine. The isodistance curves out of the two cities emphasize which counties are at the same distance from Trier or from Emmerich.

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 2010. 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 are not missing 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)
	Principal Component Measures of Persecution			Log Deported Jews (*extensive margin)*		Ratio Deported Jews (*intensive margin)*		Pogrom 1349	
	Probit	Probit	OLS	Probit	OLS	Probit	OLS	Probit	OLS
Persecution of Jews *(column heading)*	-0.011 *0.005***	-0.012 *0.004****	-0.013 *0.004****	-0.005 *0.002***	-0.006 *0.002****	-0.032 *0.017**	-0.033 *0.017**	-0.021 *0.009***	-0.021 *0.009***
Log Jews 1933	0.045 *0.047*	-0.054 *0.052*	-0.017 *0.052*	0.022 *0.038*	-0.014 *0.038*	-0.020 *0.056*	-0.032 *0.052*	-0.025 *0.037*	-0.031 *0.037*
% Catholics 1925	0.029 *0.013***	0.006 *0.011*	0.004 *0.012*	0.003 *0.011*	-0.002 *0.011*	0.004 *0.011*	0.002 *0.011*	0.001 *0.011*	-0.001 *0.011*
Age		-0.002 *0.001**	-0.002 *0.001**	-0.003 *0.001***	-0.003 *0.001****	-0.002 *0.001**	-0.002 *0.001****	-0.003 *0.001****	-0.003 *0.001****
Age^2 /100		0.004 *0.001****	0.004 *0.001****	0.004 *0.001****	0.005 *0.001****	0.004 *0.001****	0.004 *0.001****	0.005 *0.001****	0.004 *0.001****
Female		-0.036 *0.060*	-0.010 *0.061*	-0.014 *0.060*	-0.020 *0.060*	0.003 *0.006*	-0.010 *0.061*	-0.015 *0.060*	-0.020 *0.060*
Single		0.058 *0.010****	0.052 *0.009****	0.057 *0.010****	0.052 *0.009****	0.058 *0.010****	0.052 *0.010****	0.058 *0.010****	0.052 *0.009****
College		0.058 *0.021****	0.091 *0.032****	0.058 *0.021****	0.091 *0.031****	0.058 *0.021****	0.091 *0.032****	0.059 *0.020****	0.093 *0.031****
Eastern Germany		-0.028 *0.021*	-0.024 *0.019*	-0.035 *0.021*	-0.031 *0.019*	-0.029 *0.022*	-0.026 *0.020*	-0.026 *0.020*	-0.021 *0.018*
Income p.c. 2005		0.033 *0.016***	0.037 *0.017***	0.035 *0.016***	0.039 *0.017***	0.037 *0.017***	0.040 *0.017***	0.028 *0.017**	0.031 *0.017**
% College graduates 2005		0.128 *0.142*	0.110 *0.142*	0.162 *0.141*	0.145 *0.140*	0.129 *0.145*	0.113 *0.146*	0.090 *0.128*	0.065 *0.126*
Income deciles	X	X	X	X	X	X	X	X	X
Other historical controls	X	X	X	X	X	X	X	X	X
Wave groups f.e.		X	X	X	X	X	X	X	X
Regional controls		X	X	X	X	X	X	X	X
Observations	17,360	13,596	13,596	13,867	13,867	13,596	13,596	13,867	13,867
N. of clusters	287	261	261	270	270	261	261	270	270
(Pseudo-) R^2	0.01	0.11	0.09	0.11	0.09	0.11	0.09	0.11	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 2010. In all columns, the dependent variable is a dummy that equals 1 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: **Homogeneous Counties and Robustness**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Homogeneous Counties						Robustness			
	Within States	Within 9 Virtual States	Within 16 Virtual States	Within II World War Zone	Only if community	County level	No poorest counties	No least educated counties	No Hanseatic League	No Free Imperial Cities
Principal Component	-0.009	-0.010	-0.009	-0.011	-0.010	-0.017	-0.018	-0.020	-0.010	-0.012
Persecution of Jews	*0.004***	*0.004****	*0.004***	*0.004***	*0.004****	*0.005****	*0.005****	*0.006****	*0.004***	*0.005****
Individual contr.	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	X
Observations	13,599	13,599	13,599	13,599	9,881	261	9,269	8,228	11,289	10,817
N. of clusters	261	261	261	261	190		153	164	235	216
(Pseudo-) R^2	0.10	0.08	0.11	0.09	0.09	0.23	0.10	0.10	0.09	0.09

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

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

adding the geographic fixed effects described on the column headings (columns (1)-(5), or across subsamples defined as described by the column headings (columns (6)-(10)). Each observation is a German household interviewed by the SOEP for the first time between 1984 and 2010. In all columns, the dependent variable is a dummy that equals 1 if the household holds stocks. The main covariate of interest, *Persecution*, is the principal component of the three measures of Jewish persecution described in the main text. S.e. are clustered at the county level. Statistical significance is reported as follows: *10%, **5%, ***1%.

Table 4: **Jewish Persecution and Present-day 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

Table 4 reports the coefficients for estimating four tobit specifications whose outcome variable is an integer measure between 0 and 100 of trust in financial markets reported by 1,000 Germans surveyed by the authors through the company *Clickworker*. The sample is restricted to respondents in counties for which the data on Jewish persecution in the Middle Ages are available, and in counties for which we had at least five respondents. 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%.

Table 5: **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 5 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 2010. In all columns, the dependent variable is a dummy equal to 1 if the household holds stocks. The main covariate of interest, *Persecution*, is the measure of Jewish persecution 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 6: **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 6 reports average marginal effects computed after estimating the following probit specification:

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

Each observation is a German household interviewed by the SOEP for the first time between 1984 and 2010. In all columns, the dependent variable is a dummy equal to 1 if the household holds stocks. The main covariate of interest, *Persecution*, is the measure of persecution of Jews described at the top of each column. In Panel A, *Persecution* is interacted with a set of dummy variables each including four waves of SOEP surveys. In Panel B, it is interacted with dummy variables for three cohorts of households: those who experienced the Nazi period, those born after the Second World War, and those born after 1965. S.e. are clustered at the county level (Kreis). Statistical significance is reported as follows: *10%, **5%, ***1%.

Table 7: **Distance from the Rhine Valley 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 7 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,

$$DistanceRhine_k = \alpha + K_k' \times \delta + \epsilon_k, \tag{6}$$

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 6, 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 8: **Placebo Tests with the Distance from the Rhine Valley**

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 8 reports the average marginal effects computed after estimating the following probit specification,

$$Pr(HoldsStocks_i|X_i, K_k) = \Phi(\alpha + \beta_1 \times DistanceRhine_k + \beta_2 \times PersecutionJews_k + X_i' \times \delta + K_k' \times \delta + \eta_t + \epsilon_{ik}), \quad (7)$$

where *DistanceRhine* is the measured 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 8 reports the coefficient on the distance measures for estimating Equation 7 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 9: **Instrumenting Jewish Persecution: Three-stage OLS**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	First stage: Existence Jewish community in 1096			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.
Residual Distance Rhine	-0.136	-0.213	-0.148						
	*0.044***	*0.043***	*0.037***						
Existence Jewish community in 1096				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 controls	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

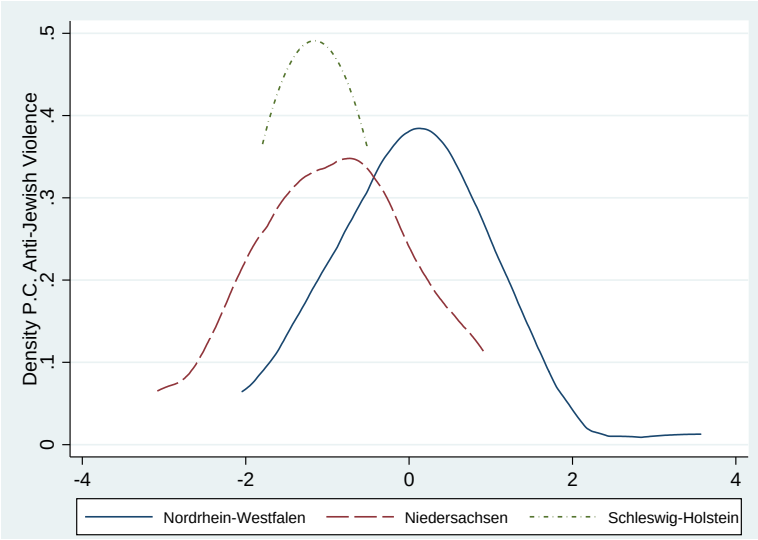
Table 9 reports OLS coefficients for the three-stage county-level instrumental variable procedure described in Section 4. In the first stage (columns (1)-(3)), the probability that a Jewish community existed in 1096, before the Black Death, in each German county is predicted by the residuals of the distance of the county from the Rhine Valley regressed on observables for the medieval economic conditions of counties. In the second stage (columns (4)-(6)), the principal component of the measures of Jewish persecution is predicted by 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%.

Online Appendix
to
Distrust in Finance Lingers: Jewish Persecution and Households' Investments

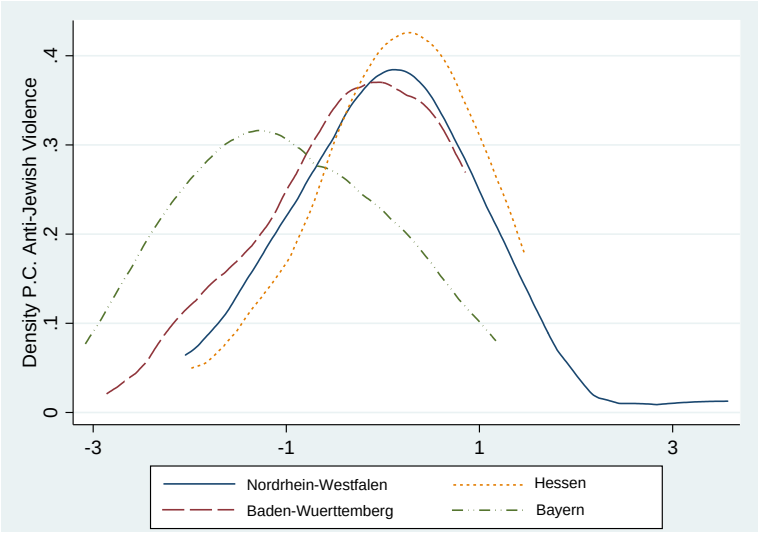
Francesco D'Acunto Marcel Prokopczuk Michael Weber

Figure A.1: **Densities of County-level Persecution across German States**

(a) Out of Rhine Valley towards North



(b) Out of Rhine Valley towards South



(c) Farthest Distance from Rhine Valley in Both Directions

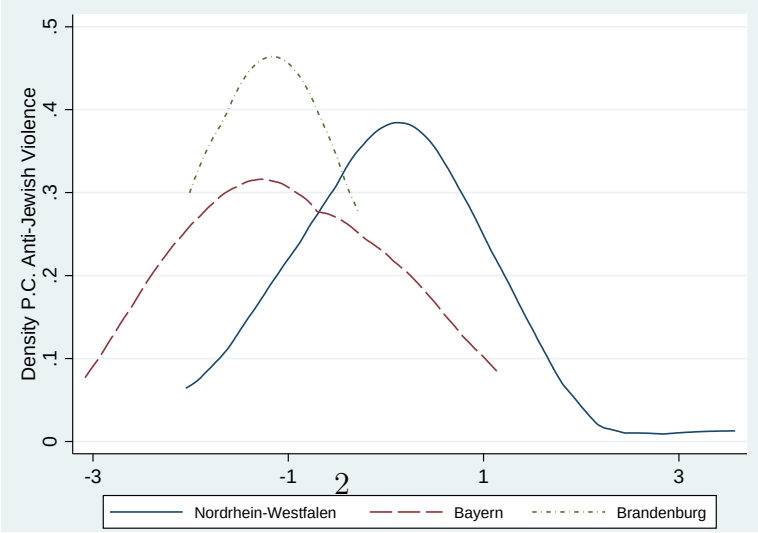


Figure A.2: **Raw Data - Correlations**

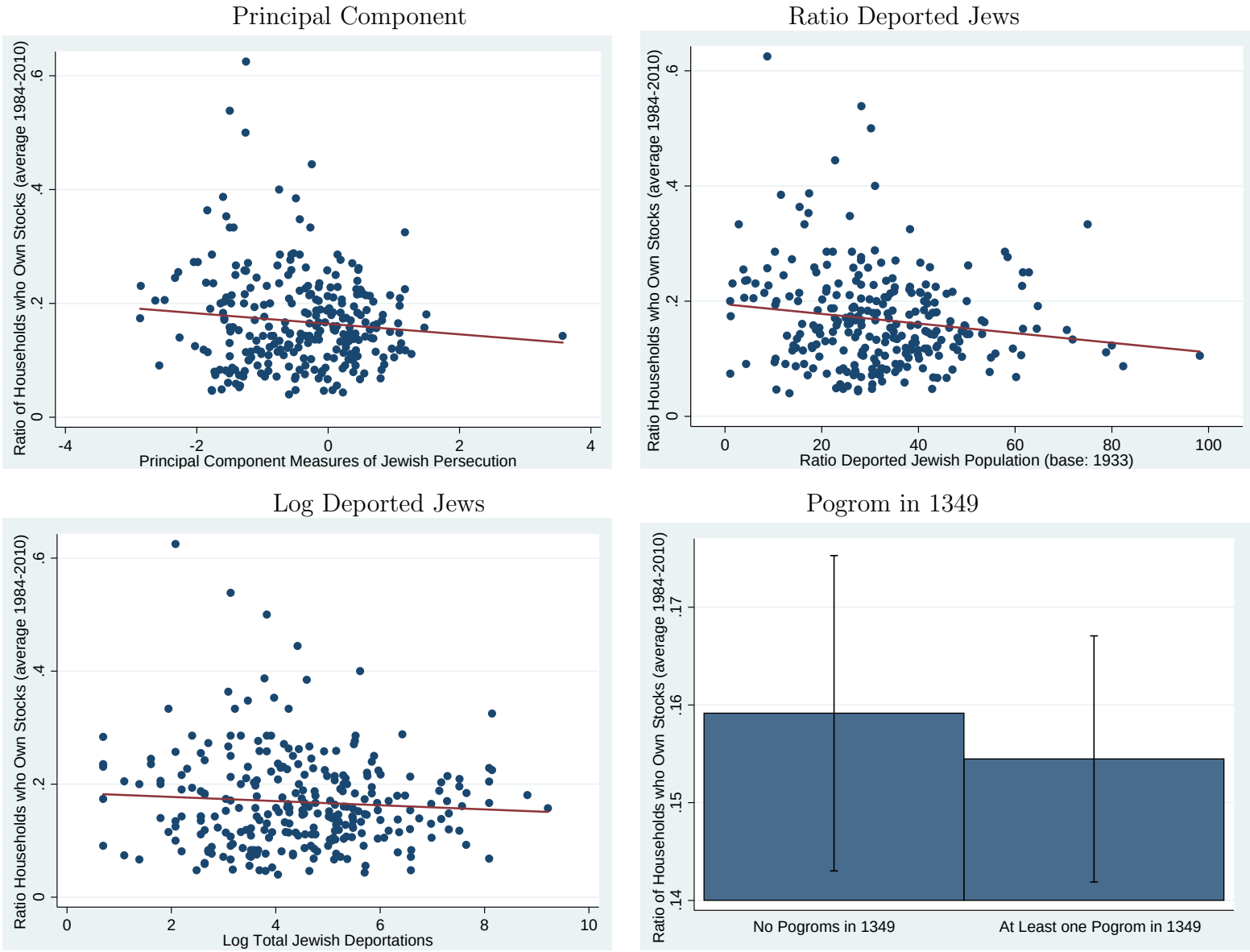


Figure A.3: **Raw Data - Stockholdings, Principal Component by Pogrom in 1349**

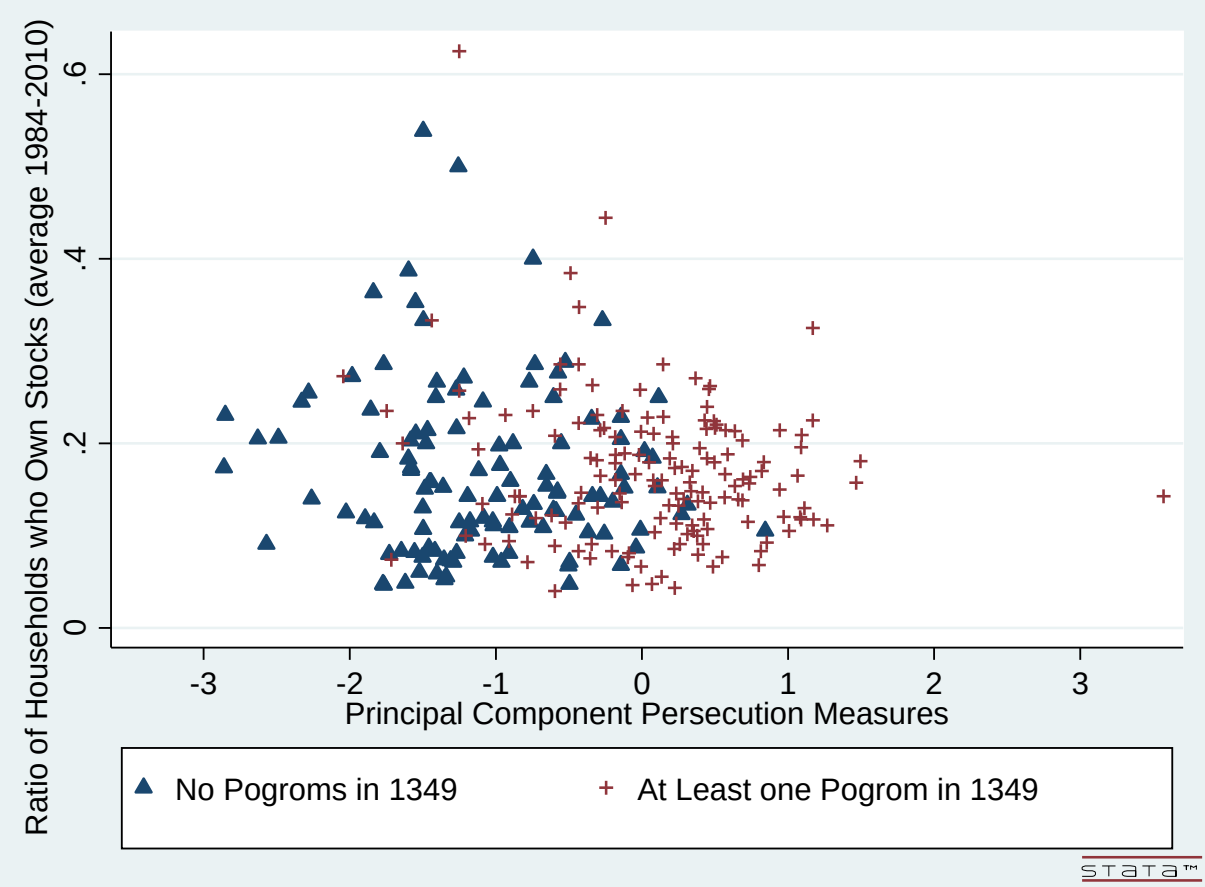
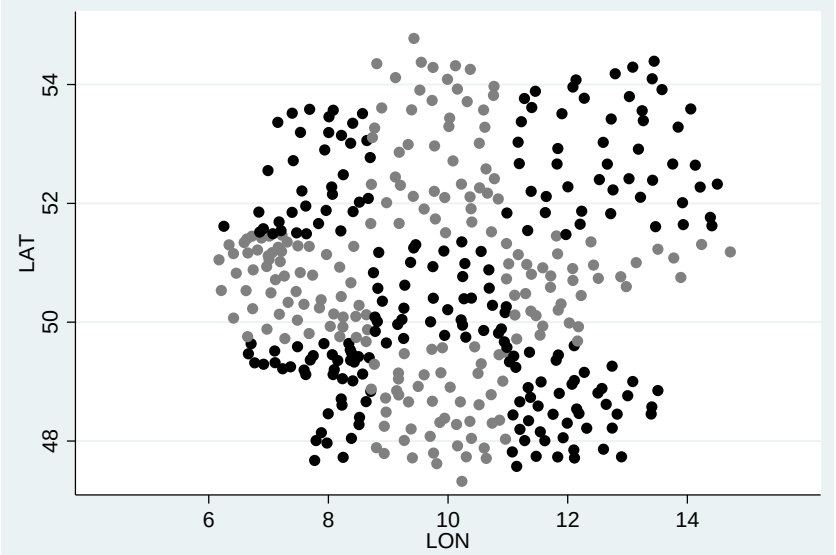


Figure A.4: **Virtual States**

Virtual States 1: 9 arbitrary partitions



Virtual States 2: 16 arbitrary partitions

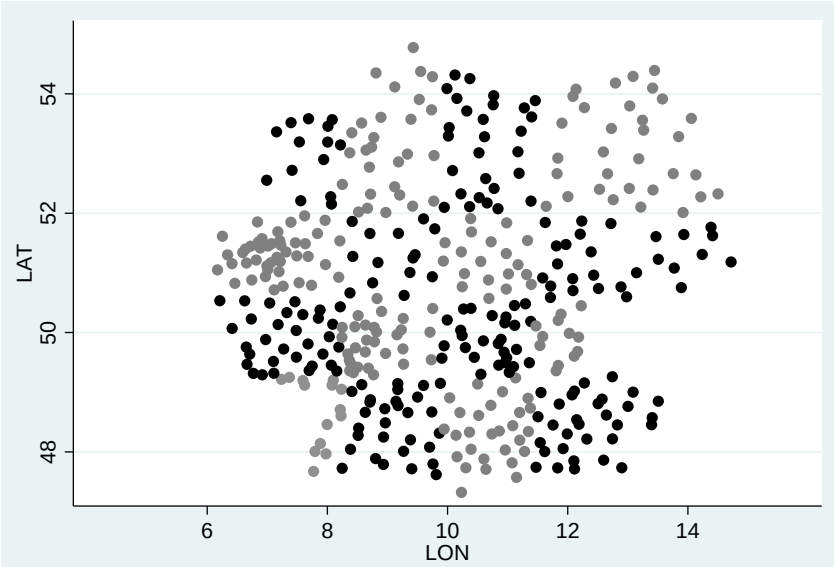
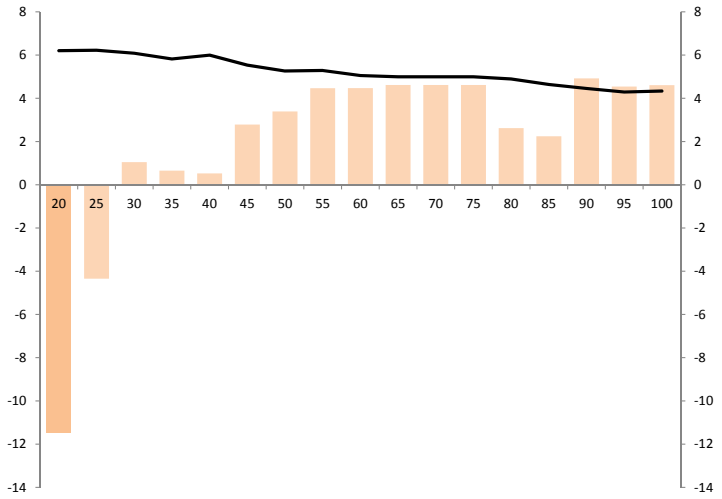
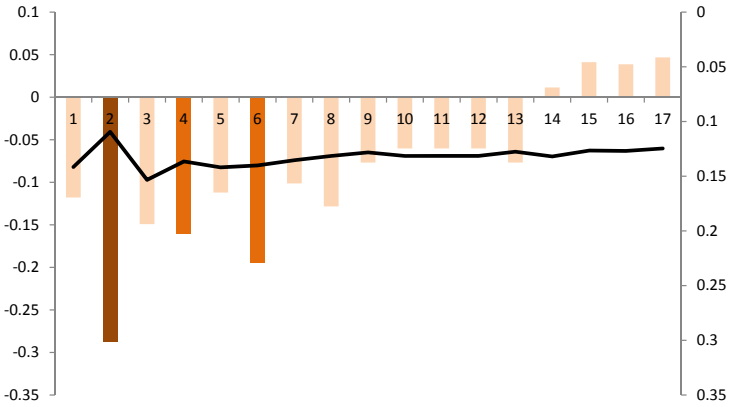


Figure A.5: **Economic Crisis and Alternative Outcomes**

(a) Households' Head Age



(b) Home Owner



(c) High School Degree or Higher Education

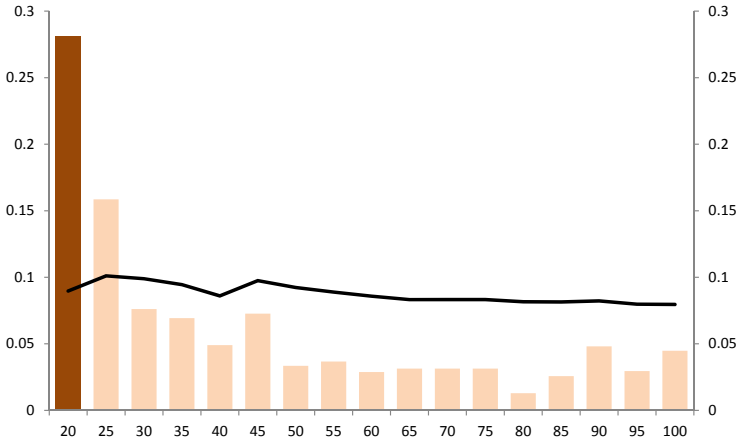
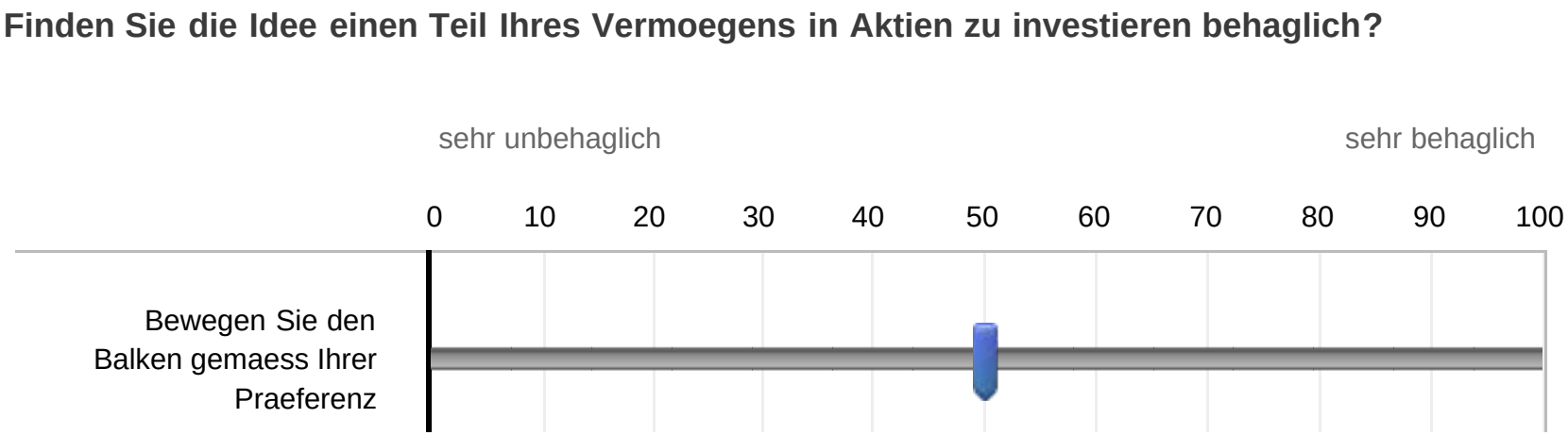
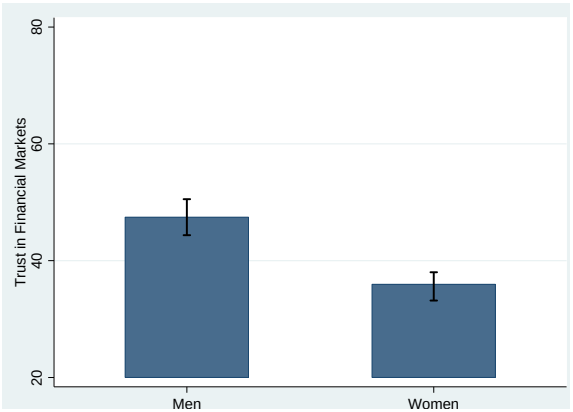


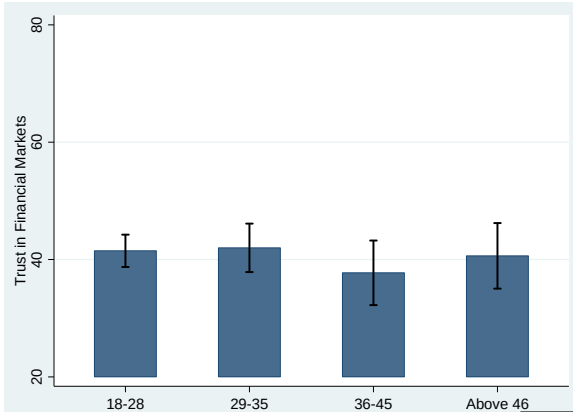
Figure A.6: **Trust in Finance: Survey and Demographics**



(a) Trust in Finance by Gender



(b) Trust in Finance by Age



(c) Trust in Finance by Education

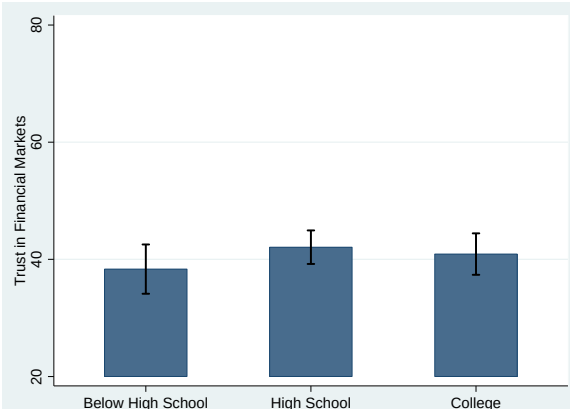
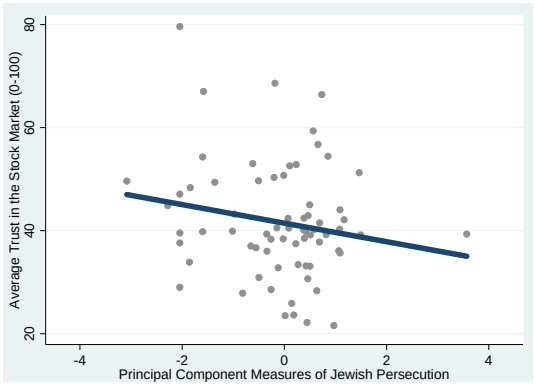
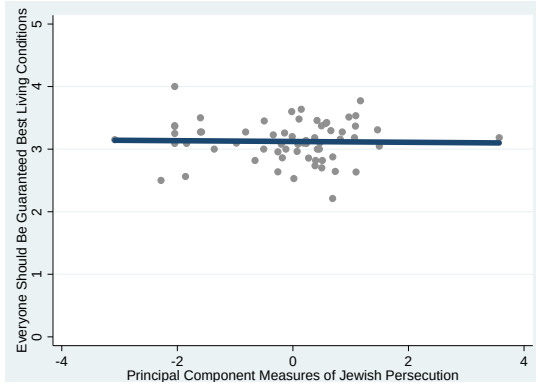
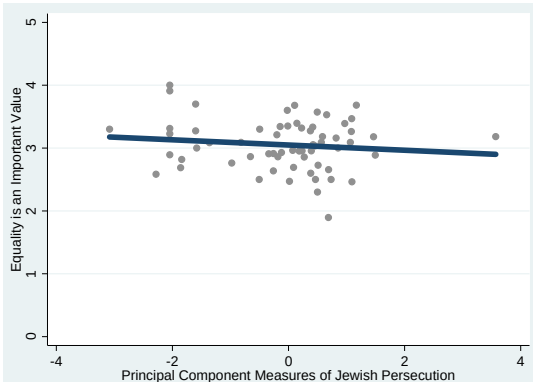


Figure A.7: **Distrust in Finance, Antisemitism, Backwardness, and Jewish Persecution**

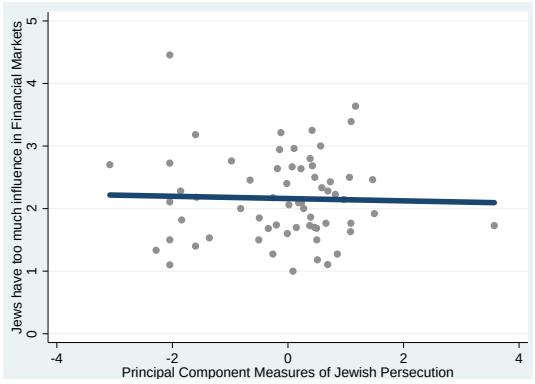
(a) Distrust in Finance and Neutral Values

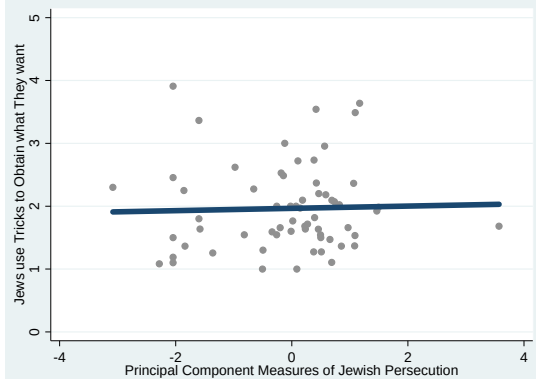


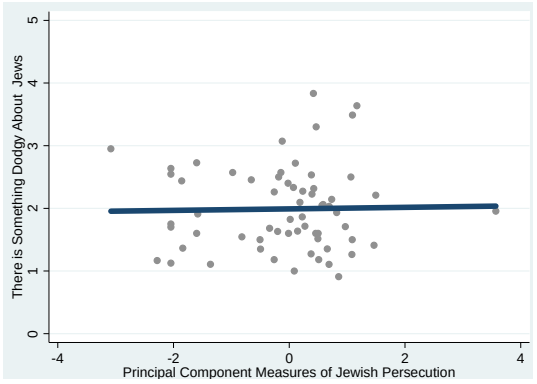




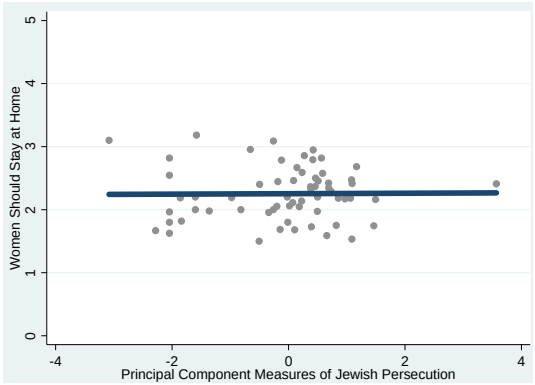
(b) Present-day Antisemitism

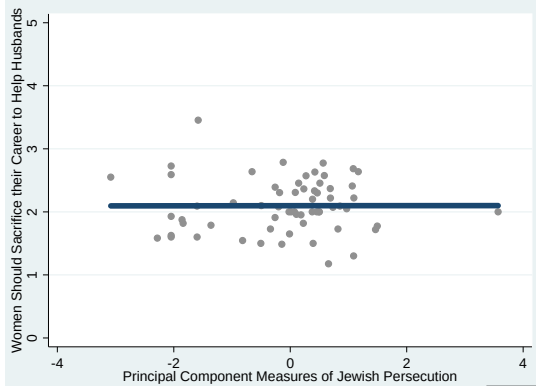


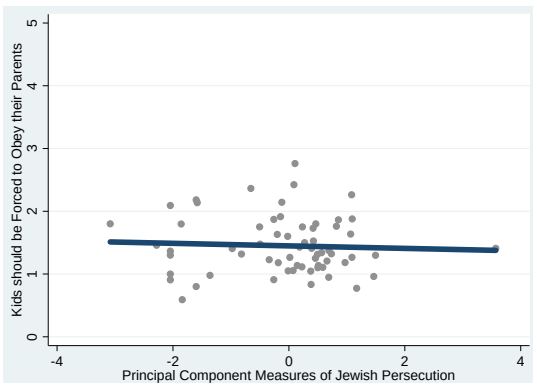




(c) Present-day Backwardness







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

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

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

$$RatioHoldStocks_k = \alpha + \beta Distance\_purged + K_k'\delta + \epsilon_k,$$

where

$$\alpha = \alpha_3 + \beta_3\alpha_2 + \beta_3\beta_2\alpha_1$$
$$\beta = \beta_3\beta_2\beta_1$$
$$\delta = \beta_3\beta_2\delta_1 + \beta_3\delta_2 + \delta_3$$
$$\epsilon_k = \beta_3\beta_2\epsilon_1 + \beta_3\epsilon_2 + \epsilon_3,$$

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

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

Because $Cov(Distance\_purged, \epsilon_1) = 0$ 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 A.1: **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 A.1 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,

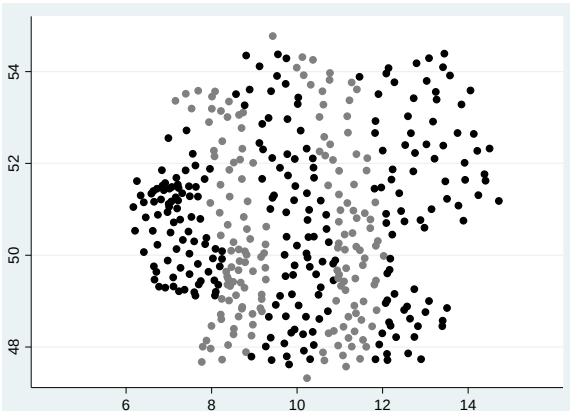
$$RatioHoldStocks_k = \alpha_3 + \beta_3 \times PersecutionJews_k + K_k' \times \delta_3 + \epsilon_{3k},$$

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

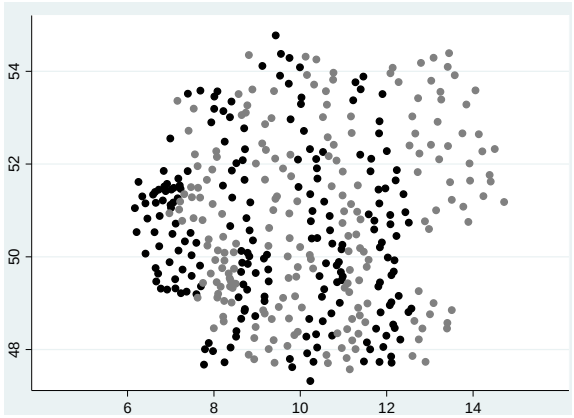
Table A.2: **Keeping the Distance from the Rhine Valley Constant: Within-distance-group analysis**

	(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

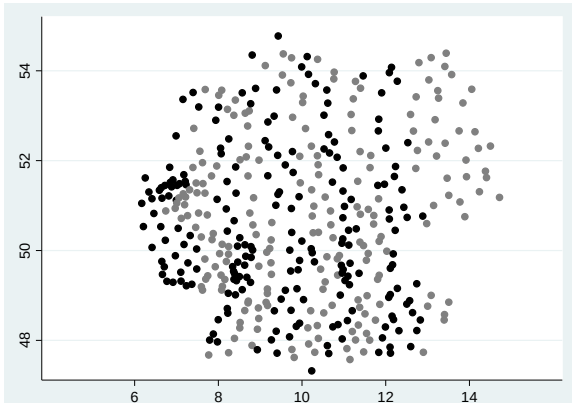


Table A.2 reports the average marginal effects computed after estimating the following probit specification,

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

Each observation is a German household interviewed by the SOEP for the first time between 1984 and 2010. In all columns, the dependent variable is a dummy that equals 1 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 distances from the Rhine Valley. S.e. are clustered at the county level. Statistical significance is reported as follows: *10%, **5%, ***1%.