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# Persistence and Historical Evidence: The Example of the Rise of the Nazi Party

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## Abstract

The persistence literature in economics and related disciplines connects recent outcomes to events long ago. Although this influential literature is promising, it raises serious questions about how to distinguish deep causal factors that persist across time from alternative explanations derived from the rapidly changing historical context or misuse of historical sources. We discuss two prominent examples that ground the rise of the Nazi Party in distant historical roots. Several econometric, analytical, and historical errors undermine the papers' contention that deeply rooted culture and social capital fueled the Nazi rise. The general lesson for persistence studies is that beyond careful econometrics and serious consideration of underlying mechanisms (including formal theory), they must pay scrupulous attention to the historical context and the limitations of historical data.

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Much influential economic history today aims to demonstrate the persisting influences of long-ago events. Melissa Dell (2010), for instance, ties poverty in Latin America in recent years to institutions established under colonialism. Nathan Nunn (2008) claims to link slow economic growth in late twentieth-century Africa to the devastation of the slave trade. Similar efforts have spread into political science: Avidit Acharya (2016) and his coauthors use tools from economics to connect differences in political attitudes in the United States today to the prevalence of slavery more than 150 years ago. This literature has earned praise but it is open to criticism, a topic we revisit in the conclusion.

Here we consider an influential example of this genre: studies that invoke earlier historical events to explain the Nazi Party and anti-Semitic behavior in Germany in the 1920s and 1930s.[1] In "Persecution Perpetuated" (henceforth PP), Nico Voigtländer and Hans Joachim Voth (2012) argue that differences in the local culture of anti-Semitism in the Middle Ages explain cross-sectional patterns in votes for the Nazis and other anti-Semitic activities in the early twentieth century. In "Bowling for Fascism" (henceforth BF), Shankar Satyanath, Voigtländer, and Voth (2017) claim that social capital formed in the nineteenth century accounts for cross-sectional differences in Nazi Party membership in the 1920s and 1930s. Both papers argue for the persistent effects of causes in the past and both are widely cited.[2]

These two articles address one of the central events of the twentieth century, the rise of a regime that triggered a world war and tried to exterminate the entire Jewish people. Careful scrutiny of the two papers, however, shows that both suffer from a number of interrelated weaknesses. First, the econometric results are fragile. Many results depend on outliers or are not robust to reasonable alternative specifications. Some reflect tendentious specifications. This fragility stems, in part, from flawed use of historical evidence. Second, each article's argument suffers from the lack of a model, mathematical or

[1] Noteworthy econometric and statistical studies of voting for the Nazi Party and Party membership include Van Riel (1993), King et al. (2008), Spenkuch and Tillmann (2018), and Brustein (1996). For party competition in the Weimar parliaments, see the roll call analysis in Hansen and Debus (2012).

[2] PP has 1006 Google Scholar citations and BF 378 (as of February 16, 2023).

verbal, that would clarify the implicit assumptions and suggest possible alternative explanations that would more accurately fit the historical evidence. Third, misinterpretations of the historical context compound the econometric and modeling problems. Finally, both articles do injustice to the historical literature and code published data in ways that fail to respect the limitations of the historical sources.

Some of the econometric issues we discuss reflect specification problems. But standard econometric techniques do not overcome the more general weaknesses we identify. At bottom, the issue is how to address the influence of slowly changing, deep causal factors that persist across time when there are alternative explanations derived from the rapidly changing historical context. Not appreciating historical context (which includes coincidences not taken into account in econometric specifications) can lead to spurious empirical relationships between modern outcomes and deep factors from the past. So can misuse of historical sources. Both PP and BF make such mistakes. Those mistakes may in turn hide sources of true persistence. In any case, we see little firm evidence for enduring social capital or cultural anti-Semitism. Lasting regional differences in politics and religion provide a more promising alternative explanation for the results in both articles. Germany's historiography has long stressed the importance of regions.

The remedy, from our perspective, is for persistence studies to be serious about models, about historical data, and about doing the necessary historical research. That should be essential for all persistence studies. Otherwise, researchers risk being snared by explanations that are appealing but ultimately unsupported.

This paper raises questions about the general persistence literature by focusing in detail on two specific journal articles. We take this approach in part because others have written survey articles about persistence, but also because the focus allows us to dig into questions that are too specific to discuss in the context of a survey. Both PP and BF appeared in leading economics journals and function as models for others doing this kind of research. Our discussion, we hope, warns the economic history and broader social-science history literature by pointing to specific limitations. We should also note that Voigtländer

and Voth (2022) wrote a reply to this paper's first version. That reply did not discuss most of what we said, and they have not updated their reply to account for our revisions since. But we take some space here to discuss their initial reactions to our criticisms.

*How Robust is the Evidence in PP?*

PP's authors claim that anti-Semitism in 1920s and 1930s Germany derived from an enduring culture of hostility to Jews that can be traced back to the Middle Ages. This cultural anti-Semitism varied from place to place within Germany, but it persisted in a given place across time for six centuries.[3] Similar claims about persistent behavior and attitudes underlie other econometric studies and can be derived from theoretical models.[4] One obvious difficulty is how to measure anti-Semitic attitudes in the Middle Ages. PP uses as a proxy an indicator variable equal to one for Jewish communities that fell victim to pogroms during the Black Death (1347-1351).

PP's core results (PP Table VI) test the effect of this pogrom proxy variable, POG1349, on six different outcomes: two measures of voting for the Nazis and other extremists in the 1920s; two sets of violent attacks against Jews in the 1920s and 1930s (including the *Reichkristallnacht*); deportations of Jewish residents; and anti-Semitic letters to the Nazi periodical *Der Stürmer*. Each regression controls for the locality's population size and religious composition from the period 1924-33. If persistent culture causes anti-Semitic behavior, POG1349 should have a positive and statistically significant coefficient in all six regressions. PP's authors stress the t-ratio associated with their pogrom indicator, POG1349.They do not ordinarily discuss the effect's size.

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[3] The spatial variation distinguishes PP from the claim about widespread and uniform anti-Semitism in Goldhagen (1996). That claim (as PP notes) has been controversial. For an overview of the debate, see Deák (1997) and Herbert (1999).

[4] For models of cultural persistence, see Bisin and Verdier (2001), Richerson and Boyd (2008), and the works cited in PP. For an application in economic history, see Mokyr (2016).

Our Table 1 reconsiders the results for two of those outcomes, plus a composite of all six. (Our Appendix A discusses each of the other four outcomes reported in PP Table VI.) Let us begin with votes for the Nazi Party in the May 1928 federal election. Column (1) replicates the regression reported in PP (Table VI, Column (2)). POG1349 had a significant positive coefficient for the 1928 election, but a partial regression plot from our Column (1) (Figure 1, Panel A) shows that the result is driven by outliers, many of which are in Bavaria, Germany's second largest federal state.[5] Column (2) re-estimates Column (1) as a quantile (median) regression, a standard check for outliers. POG1349 has little effect on the conditional median. This difference shows that the PP result was driven by the outliers. To better-explore possible regional diffeences, we add to the PP specification a full set of fixed effects for the German states along with their interactions with POG1349. The result (see WP, Appendix A.5) shows that Bavaria is the only federal state with a significant relationship between the medieval pogrom and the 1928 Nazi vote, a point we return to below.[6]

Our Table 1 reveals analogous problems with PP's composite measure, the first principal component (p.c.) of all six outcomes in PP Table VI. The p.c. is supposed to capture "a broader, underlying pattern of attitudes" (PP, p. 1370). Table 1, Column (3), replicates PP Table VII, Column (1), the specification that corresponds to the models presented in their Table VI. The partial regression plot for this specification (Figure 1, Panel B) shows that this result, too, is driven by outliers, primarily in Bavaria. (The p.c. is uncorrelated with the Letters, Deportations, and *Kristallnacht* indicators; it is somewhat correlated with the 1920s pogrom variable and highly correlated with only the 1924 and 1928 voting outcomes. Thus is it not really a "broader measure," which is why the two panels of Figure 1 look

[5] In the regression reported in Column (1) of Table 1, there are 16 observations with a "studentized" residual greater than or equal to 2. Fourteen are in Bavaria; the other two are in Baden. 70.6 percent of Bavarian communities experienced a pogrom; overall, this figure is 72.3 percent. In this paper and in the PP data, Bavaria's borders are those of the Weimar Republic. This Bavaria therefore included more territory than the medieval Duchy of Bavaria, including in Franconia.

[6] The issue here is whether the effect of cultural antisemitism is different from zero, and not whether our estimates differ from PP's (see Appendix Section A.2 for an explanation).

so similar. See Appendix Section A.4.) The pogrom coefficient is not significant in a quantile regression for the principal components variable (Table 1, Column 4). Including fixed effects in OLS models for this dependent variable shows that the pogrom variable has a significant effect only in two tiny states. (Appendix Sections A.4 and A.5)

Table 1 includes one more example from PP's Table VI, deportations. For this outcome alone, PP adds an additional and redundant control, the log of the Jewish population. This model (like the others in their Table VI) already includes the population and the percentage Jewish. Comparing our Columns (5) and (6) shows that the significant result PP reports depends entirely on adding this superfluous regressor. Appendix Sections A.1-A.5 discuss related problems in the PP specifications. Although the reported results are consistent with PP's hypothesis, diagnostic tests (such as examining outliers or considering more general functional forms) imply that the pogrom proxy, with one exception, does not have a robust effect on twentieth-century anti-Semitic behavior.

That one exception is the model for the *Reichkristallnacht* attacks (PP Table VI, Column 6). This example, however, misreads the history by ignoring political and religious actors. Medieval pogroms reflected not just cross-sectional variation in anti-Semitism in 1349, as PP assumes, but the actions of political and religious leaders at the time. The same goes for anti-Semitic outrages in the twentieth century. Historians in fact argue that the *Kristallnacht* attacks were a government and Nazi Party operation that did not mirror the local populace's anti-Semitism. Here the dependent variable itself ignores the historical context. (See Appendix A.1)

A placebo exercises raises serious doubt about the pogrom proxy in general. That indicator supposedly proxies for a long history of anti-Semitic views, but in regressions analogous to PP Table VI, the proxy also raised the 1924 vote share of the liberal DDP party, which attracted strong Jewish support. The DDP results for 1924 thus cast serious doubt on the pogrom proxy's interpretation because the

pogrom-indicator idea fails simple placebo tests.[7] Appendix A.6 reports similar results for all Weimar coalition parties, as well as the extremist parties, in 1924, 1928, and 1933.

*Understanding the Outliers*

We leave these other problems aside and focus on Bavaria, the main source of outliers in the 1928 election, the p.c. results, and other PP regressions. In our specifications that add state fixed effects and their interaction with POG1349, the medieval pogrom proxy tends to be significant in only a subset of German states (Appendix A.4 and A.5). PP argue that enduring anti-Semitism explains Weimar-era outcomes in Germany. The econometric evidence instead supports the historiographical stress on differences across German regions. To understand why, we have to examine the role that political and religious authorities played in both the medieval pogroms and the anti-Semitic behavior in the 1920s and 1930s

One example illustrates the role those actors played: the 1349 pogrom in Strasbourg, which is mentioned in PP (p.1347), although it is not in the dataset because after World War I, Strasburg returned to France. In 1349 Strasbourg's thriving Jewish community was rounded up and burned to death even before the plague reached the city. In 1390 Jews who had returned were expelled. Jews only returned four hundred years later under the influence of the French Revolution. Strasbourg thus seems a clear illustration of the anti-Semitic attitudes at issue in PP; S. K. Cohn (2007) uses it as an example in his historical analysis of the European-wide pogroms.[8]

[7] Since the placebo regression controls for the Jewish population, the result is not an ecological fallacy produced by the reaction to the existence of a Jewish community. And there were far too few Jewish Germans voting in 1924 for this result to reflect their own votes.

[8] Our sources for Strasbourg include Mentgen (1995); Ephraïm (1923, 1924); Ginsburger (1908); Haverkamp (1981); and the documents published in Witte and Wolfram (1896).

The story, though, is not just bigotry: persecution always required the cooperation of political and religious authorities. Strasbourg's 1349 massacre occurred only after three municipal leaders had been deposed and the city's chief magistrate driven from the city. These authorities were not necessarily philo-Semites; they simply tried to uphold a promise the city had made to protect the Jewish community in return for fiscal benefits. They failed because the city's influential butchers' guild, as well as regional nobles and Strasbourg's bishop, wanted to get rid of the Jews. Had all the local authorities united to oppose violence against the Jews, there would have been no massacre. This is not just speculation: eleven years earlier, the regional nobles and the same bishop joined the city's leaders to stop a pogrom in the surrounding region. More generally, T. Finley and M. Koyama (2018) show that pogroms during the Black Death were more likely where political authority was fragmented, because the rents from taxing the Jews were divided, so any single authority had less incentive to protect the Jewish community.[9]

Something similar can be said for Strasbourg in the 1920s and 1930s. Despite the deep roots of anti-Semitism in Strasbourg, in the 1920s and 1930s the city did not witness any of the anti-Semitic violence seen in other hotbeds of cultural hostility to Jews. Strasbourg was French again after a period of German control between 1871 and the end of World War I. The French authorities protected the Jewish population, even when the authorities themselves were anti-Semitic (Goodfellow 1993; Caron 1998).

The religious and political authorities at the center of the Strasburg story play no role in PP's discussion. Similar authorities mattered elsewhere too, for instance in the Bavarian cities of Nuremberg and Regensburg (Haverkamp 1981, 67-77, 91-92). Power over Nuremberg's 2000 or so Jews was divided, particularly in 1349, between the Holy Roman Emperor and the city council, which opposed the emperor and wielded more influence locally. As the plague approached, the emperor, fearing a pogrom in Nuremberg, sold his rights to Jewish property there. The city council had pledged to protect the Jews

[9] There were other important causes at work in the 1347-51 Black Death pogrom: the spread of rumors, the severity of the local plague, whether the Jewish community played an important local economic role, and the politics of religious identity. See Cohn (2007), Anderson, Johnson, and Koyama (2017), Johnson and Koyama (2019), and Jedwab, Johnson, and Koyama (2019).

in return for tax revenue, but when a new city council took over in the fall of 1349, the city's debts convinced them to sell the Jews out. With the emperor's permission, the council let the pogrom happen (Avneri 1968, 2: 598-613; Haverkamp, 71-73). In Regensburg, by contrast, the city's mayor, council, and leading citizens joined together in 1349 to carry out their promise to protect the Jewish community in return for tax revenue. They thwarted residents who had gathered to attack the Jews and defended the the Jews against attacks by the Bavarian Duke (Kirmeier 2014; Avneri, 2: 679-691).

The Bavarian difference in the 1920s results reported in PP Table VI derived both from Bavaria's medieval experience and from its role as the home of the Nazi Party. At the time of the Black Death, the territory that became the Weimar Bavarian state was different because it was severely fragmented politically, even by the standards of late medieval Germany. For the portion of Weimar Bavaria that lay in the medieval Bavarian duchy, political authority splintered after the 1347 death of the Duke (and Holy Roman Emperor) Louis IV, who divided his power and revenue among his sons (Holzapfel 2013; Uhlhorn and Schlesinger 1970, pp. 186-88; Immler 2016). As in Strasbourg or Nuremberg, divided authority made it difficult to protect local Jews. The rest of modern Bavaria was politically even more fragmented in the fourteenth century, especially Franconia, the site of many of the outliers in Figure 1, Panel A. If we consider these outliers to be the 16 observations with studentized residuals greater than or equal to 2, then 14 were in modern Bavaria, and of these, 10 were in Franconia. The historical literature implies that at least 10 of these communities were fragmented politically at the time of the plague, and probably all 14.[10]

[10] For evidence that all 14 were fragmented, see Holzapfl (2013); Immler (2016); Avneri (1968); Flachenecker and Lochbrunner (2021); Hofacker (2015); Laschinger (2011); Müsegades (2016); Ullmann (2012). If we apply the measures of divided authority used in Finley and Koyama (who rely on somewhat different sources), then at least 9 were fragmented; the other 5 either did not meet their criteria or were not in their data set. If we combine their criteria with our reading of Avneri, then at least 10 were fragmented. Again, the other 4 either did not meet that standard or were not described in sufficient detail in Avneri.

Bavaria was different in the twentieth century because it was where Hitler first became known and where his party first spread beyond right-wing extremists in the Bavarian city of Munich. Although the party gained support early on in other parts of Germany, in 1928 the Nazis benefitted from having well-organized district offices already at work in Bavaria and from having Hitler able to speak and raise money locally for election propaganda (he was banned from doing so in Prussia). In addition, the Party had a well-known Bavarian general (Franz Ritter von Epp) on their ballot, who helped Hitler raise money and reportedly attracted votes from veterans otherwise reluctant to vote for the Nazis. Their intense electoral propaganda won the party an above-average vote share in Bavaria in the 1928 elections, even though the total Nazi vote there and elsewhere remained small. The party's vote share was particularly high in cities in the part of Bavaria that had been part of Franconia. There, active party offices were established early on through the efforts of Julius Streicher, the regional party leader and the publisher of *Der Stürmer.*[11]

Bavaria was not the only part of Germany where authority was splintered in the medieval period, so it did not have more Black Death pogroms than the rest of Germany. Bavaria was unusual, however, in having both Black Death pogroms and a high Nazi vote share in 1928.[12] That combination produced the Bavarian outliers that stand out in Figure 1. If PP's regression indeed demonstrates the influence of enduring anti-Semitism in the Weimar Republic, then it is surprising that POG1349 has no effect when Bavaria is excluded from the estimation (see Appendix A.5). The Black Death pogroms struck throughout Germany. An alternative possibility that better fits the data is the historical coincidence of Bavaria's

[11] Hoser (2007); Ziegler (2019b); Pridham (1973); Selb and Munzert (2018); Greif (2007); Braun (2020). Selb and Munzert find no direct effect of Hitler's speeches on Nazi voting, but his talks did raise money for expensive printed propaganda. In the PP replication data, for the election of May 1928, the Nazi Party had an average 8.8 percent vote in Bavarian districts versus 2.3 percent in the rest of Germany. The vote share averaged 15.3 percent in Oberfranken-Mittelfranken, the part of Franconia where the party organization was particularly strong.

[12] In the PP data set, 17.7 percent of Bavarian towns had Black Death pogroms versus 18.3 percent outside Bavaria. However 16.6 percent of Bavarian towns had both a pogrom and an above median Nazi vote share in May of 1928, versus 8.3 percent of towns outside Bavaria.

having both fractured political authority after 1347 and an effective and better funded local Nazi party organization in 1928. That coincidence would also explain why the fixed effects regressions (Appendix A.5) typically show a relationship in Bavaria but nowhere else.

To see this issue more precisely, consider the proxy *p* that is used to measure persistent cultural anti-Semitism *s* in a town in PP. We cannot observe this latent variable *s*; we only see the dichotomous proxy *p*, which equals one if the Jewish community in the town suffered a pogrom in 1348-50. Proxy variables are by definition mismeasured: if $p = s + u$, then *u* is the measurement error, which includes the factors other than anti-Semitism that gave rise to pogroms in 1348-50. If the claim in PP is correct, then the true model for the 1928 vote is $y = \alpha s + e$, where $\alpha$ is the effect of enduring anti-Semitism. The error term *e* represents the other factors affecting the 1928 Nazi vote.

To use the medieval pogrom as a proxy, the regressions in PP estimate the equation $y = \beta p + f$. (We will develop this intuition abstracting from other controls, but return to them below.) Because the true model for *y* is $y = \alpha s + e$, the estimate $\beta$ in PP is:

$$\frac{\sum yp}{\sum p^2} = \frac{\sum(\alpha s + e)(s + u)}{\sum(s + u)^2} \tag{1}$$

If the variables in the sums are i.i.d, have finite means and are measured relative to their means, then the expression to the right of the equal sign in Equation (1) converges to the following as *n* increases:

$$a\left(\frac{cov(s,u) + var(s)}{var(s) + 2cov(s,u) + var(u)}\right) + \frac{cov(s,e) + cov(u,e)}{var(s) + 2cov(s,u) + var(u)} \tag{2}$$

where *cov(s, u)* is the covariance of *s* and *u*, *var(s)* is the variance of *s*, etc. If all the covariances in equation (2) are zero, then $\beta$, the estimate for POG1349, will simply be an attenuated estimate of the true coefficient $\alpha$, a standard result for measurement error in a regressor in a linear model. The covariances in

equation (2) are unlikely to be zero, however, because PP omits a role for political and religious authorities.

We discuss these issues as they pertain to Bavaria, where the historical evidence is clearest. But the issues are not limited to Bavaria. First, *cov(u, e)* is not zero because the error terms affecting medieval pogroms and 1928 Nazi vote were correlated in Bavaria when the role of political and religious actors is omitted. One might assume that they would not be correlated because *u* pertains to 1348-50 and *e* to 1928. But in Bavaria historical coincidence connected them. In Bavaria, splintered political authority would make *u* large by adding to the factors other than anti-Semitism that gave rise to medieval pogroms. Historical coincidence inflated *e* as well in Bavaria, because Hitler got his start there, could raise funds for election propaganda in 1928, and had the support of active party offices, all of which would give the Nazis a higher percentage vote. This historical coincidence created the Bavarian outliers that biased the coefficient for POG1349.[13] And as Figure 1 shows, there were outliers outside Bavaria as well. The lesson is that just because a potential causal variable lies in the past does not mean it is exogenous, particularly when political factors can affect observations across time and regions.

Second, the covariance *cov(s, u)* between anti-Semitism *s* and the proxy error term *u* is also unlikely to be zero because *u* will include political factors that affect the chances of a pogrom. Strong local anti-Semitism could make it easier for elites with financial motives to find allies for a pogrom that would seize Jewish assets. The expected financial gains would depend on the odds of resistance by local authorities, and hence on unobserved political questions such as how divided local political authority was in 1348-50. If authority was fragmented, as in Bavaria, stopping the pogrom would be less likely. Since

[13] We stress that medieval anti-Semitism in Bavaria was not the chief reason Hitler got his start there. His Bavarian success had more to do with political events that struck Bavaria in particular: a 1918 revolution that toppled a monarchy and established a republic; a failed attempt to establish a Soviet-style regime, which was violently overthrown and caused political sentiment to swing to the right; and a 1920 coup, which until 1924 made Bavaria a haven for right wing extremists fleeing a failed right-wing coup in Berlin. See Gelberg (2007); Ziegler (2019b); Pridham (1973).

greater local anti-Semitism *s* would make it easier to win support for such a pogrom, *cov(s,u)* would not be zero.

PP requires that POG1349 is exogenous to behavior in the 1920s and 1930s. The nonzero covariances we discuss show this is not true. Just because something is in the past does not make it exogenous.

*The Role of Political and Religious Authorities*

With the right added controls, the covariances we discussed earlier could be driven closer to zero: what appears as part of the unobserved error term in PP's regressions would be explained by the added controls. Such controls would include variables that pin down the changing political and religious context, both for the Middle Ages and the 1920s and 1930s. For the Middle Ages, PP does incorporate characteristics of medieval cities (PP, Tables VIII, A19). But those controls do not account for the sort of fragmented and varying political authority we found in Bavaria. They therefore cannot capture whether local medieval authorities had an incentive to protect the Jews.

Religion matters too, not just in 1348-50 but also in the 1920s and 1930s, as others have shown (J. L. Spenkuch and P. Tillmann 2018; Spicer 2008). If the actions of religious and political authorities (either in the 1300s or in the 1920s and 1930s) better explain anti-Semitism than does persistent culture, then it would be easier to account for three troublesome patterns in the PP data. First, many towns with a Black Death pogrom were close to places that did not have such a pogrom, as we show in Appendix A.8. Such a sharp local difference seems incompatible with the idea of local culture, which would presumably diffuse over neighboring communities as people went to market, sought marriage partners, or looked for work. By contrast, this geographic pattern would fit quite well with the fragmentation of local political authority in the late Middle Ages. Second, Jews soon returned to communities that experienced a pogrom in 1348-50. Their return to such communities implies that the pogrom reflects not so much enduring bigotry as the actions of local political and religious authorities. The Jews might return when new urban

magistrates, bishops, or seigneurial lords offered them credible protection. Third, Spenkuch and Tillmann (2018, Table 6 and p. 31) show a clear role for the Catholic Church in explaining rapid changes in anti-Semitic conduct in the 1920s and 1930s. These swift fluctuations in behavior are hard to reconcile with a predominant role for deeply rooted cultural anti-Semitism.

*Do the Results in BF hold?*

We now turn to a second paper that links the horror of the Nazi period to deeply-rooted features of the past. BF seeks to explain cross-sectional differences in Nazi Party membership by appealing to another historical cause, variations in the "social capital" embodied in the voluntary associations that flowered in nineteenth-century Germany. The idea of social capital spread in the social sciences thanks to the work of Robert Putnam (R .Leonardi, R.Y. Nanetti, R.Y. and R. Putnam1 1992; Putnam 2000). The concept has proven difficult to define. Some have worked to pin down its meaning via the sociology and economic theory of networks (Banerjee et al. 2019; Jackson 2019). Putnam (and BF) use the density of "civil society" organizations as their main empirical measure of social capital. BF addresses an older literature to argue that during the Weimar Republic, the Nazi Party drew on social capital to boost recruits: "… an important strand of the literature on the rise of totalitarianism has argued that the weakness of German civic society facilitated the rise of the Nazis. Our results demonstrate that the opposite is closer to the truth" (BF 2017, p. 482).

BF constructs a proxy for social capital by counting the number of civil-society associations per capita in a sample of 229 German cities in the mid-1920s.[14] The authors' regressions test whether this proxy explains the percent of the population who joined the NSDAP from each city in this period. The regressions control for city size and religious composition, as in PP, as well as the percentage of the work

[14] BF counts what in German is called a *Verein.* The word can mean "association," "society," or "club." We use these terms interchangeably.

force that is blue collar. The main results (BF 2017, Table 3) imply that more social capital leads to more Nazis, but additional tests (BF 2017, Table 7) show this was true only in federal states BF considers politically "unstable." In Prussia and other "stable" states, there is no such relationship. Prussia was the largest single federal state, accounting for 60 percent of Germany's 1925 population and 52 percent of the BF sample cities. The other states BF labels as stable had about 15 percent of the total German population and about 20 percent of the sample. So BF's results, taken at face value, imply that social capital only affected Nazi recruiting in one-third of Germany. This result contradicts their primary claim, something BF's authors appear not to appreciate. BF's results for "Germany" reflect, if anything, only the "unstable" states. Here we focus on the main results and the stability issue, which is a serious challenge to BF's results.[15]

BF devises a stability index as the first principal component of three variables, all defined at the state level for the period October/November 1918-May 1932: (1) the percentage of that period the longest-serving government was in power; (2) the percentage of that period the longest-serving party was in power (possibly in different coalitions); and (3) the percentage of that period the state was ruled by the "Weimar coalition :" the Social Democrats (SPD), the *Zentrum,* and liberal German Democratic Party (the DDP).[16]

BF's authors never explain the logic for the third component. Their argument stresses turnover in state-level leadership, not connections to the federal government. In addition, the party that headed Bavaria's government for much of the Weimar period (the *Bayerische Volkspartei*, BVP) had agreements with a Weimar coalition party (the *Zentrum*) that meant the *Zentrum* did not stand for office in Bavaria. In

[15] BF drop from consideration the territories that were allocated to Poland and Russia after World War II. The figures for population in 1925 pertain to the entire country.

[16] This definition for the third element appears in the notes to BF Table 7 and underlies the values of the three index elements used in their analysis. BF p. 508 defines the third element in a different and conflicting way: "governed by at least one party from the Weimar coalition."

BF's scoring, Bavaria has a zero value for the third element by definition.[17] Most important, instead of using the stability index itself in their regressions, BF's authors convert it into an indicator variable: "we split the non-Prussian part of Germany into a stable and an unstable half (with above- and below-median stability, respectively). (BF (p.508))." Their description of the binary indicator does not correspond to the way they code the variable, however: their empirical exercises include the median values as part of the unstable group. Many observations bunch around the index's median, so allocating those median observations to the "above" or "below" groups can, and in this instance does, drive the results.

Our Table 3 re-estimates the regression models reported in BF's Table 7.[18] Column (1) replicates BF's column (3) for states they consider "unstable." As BF stresses, in unstable states, more social capital means more Nazis. Our Column (2) estimates the model in Column (1) as a median regression. The clubs variable is not significant. Once again, BF's results even for the unstable states reflects the effect of outliers in OLS regressions. Column (3) drops the "Weimar party" element from the index but retains BF's binary definition of stability. The indicator is no longer significant, showing that the BF result requires that third element. Column (4) uses BF's version of the index, but defines the binary indicator to include the median values among the "stable" states rather than among the unstable ones. This change affects Bavaria alone, which has the median value of the stability index. Bavaria has 23 of the 106 non-Prussian observations, which is why shifting it from one binary category to the other matters so much. The estimate for the social-capital variable is, once again, not significant. BF's results, in short, hinge on that third element, which is unexplained and historically inappropriate, on using that binary indicator, and on assigning the median values to the "stable" group.

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[17] The Zentrum and BVP (the *Bayerische Volkspartei*) were two Catholic parties. The BVP emerged from the regional wing of the *Zentrum* during World War I. With only a few exceptions, the two parties cooperated in ways that lead some scholars to call them "sister" parties.

[18] Appendix Table B.3.1 allocates the states by alternative definitions of the stability indicator.

The Appendix (Section B.3) reports additional checks for the specifications reported in Table 3 as well as the related robustness checks reported in BF's Appendix. When we define the stability indicator to assign the median to the "stable" group, none of BF's results survive. BF's Table 7 also includes specifications that use the entire sample and interact everything with the BF stability indicator. Our Appendix B.3 shows those regressions are equally sensitive to the problems in the stability index.

Finally, we pose an obvious question: why take a continuous index and turn it into a binary indicator? This procedure just throws away information and has no justification in this case. In Table 3, Column (5), we estimate BF's model using the interaction of social capital and the continuous stability index. Social capital's interaction with stability has no effect. In fact, the net effect of social capital computed from that regression implies that social capital only matters in the *stable* states, precisely the opposite of the BF argument (Appendix Table B3.6) In short, BF own results show that the social capital indicator fails to affect Nazi recruitment in most of Germany. BF's finding of an effect in a minority of German cities relies on inappropriate econometrics and tendentious historical interpretation.[19]

*The BF Data*

BF's authors construct their measures of Nazi Party joiners from a public-use sample created by earlier researchers. They created the social-capital proxy, however, by counting the number of associations listed in the directories published for most German cities in this period. This proxy raises two distinct issues. First, can BF's sample capture the idea underlying that proxy? Second, is the distribution of associations in the 1920s exogenous? The answer to both questions has to be yes for the causal argument in BF to work, as BF's authors recognize. To justify their yes answers, they rely on a

[19] Our Table 3, Column (5) is identical to the regression that underlines BF Appendix Figure A7. They do not report the regression in the appendix. Their figure uses the wrong critical values for the confidence intervals; when corrected, the results are, in fact, not significantly different from zero. See our Appendix Section B.3.

persistence argument that is critical for their paper: differences in the density of associations in the 1920s were driven by "deep historical factors that have no direct link with Nazi Party entry" (BF 2017, p. 487).

BF lists the cities in their sample but does not state precisely which year's edition they used, so we can neither examine the actual directories that underlie their data nor add additional information drawn from the directories they use.[20] Our Figure 2 reproduces part of the relevant section from a directory for Worms (one of BF's cities) from 1925.[21] The directory divides the associations into functional categories; our figure shows the last page of the group that includes charities and cooperatives (*Gemeinnutzige Vereine und Genossenschaften*) and the first page of choral and music societies (*Gesang- u. Musikvereine*).

The Appendix (Section C.3) discusses possible sample-selection bias in the selection of cities that appear in BF's data. BF's authors started with the 547 cities that had populations over 10,000 in the 1925 census. They dropped 65 cities now in Poland or Russia, claiming "towns and cities in the formerly German areas of Eastern Europe rarely preserved marginal library holdings such as city directories" (BF, p. 490, footnote 14). They provide no support for this claim, and, as we show in Appendix C.3, some 70 percent of these places have an extant directory today, usually in a German library. BF's authors then contacted "libraries and archives" in the remaining cities (BF 2017, pp. 490-91). Some did not reply and others said they had no directories. Their final sample thus includes only 197 places from the original 547 cities. Among the striking omissions are Berlin and 11 of Germany's other largest cities. Our research, however, located directories in German libraries for most of the missing cities, both large and small (Appendix C.3).

To assess the possibility of selection bias in the sample of cities, BF Table 1 compares vote shares and socioeconomic statistics for their sample cities and Germany as a whole, but as our Appendix C.3

[20] "We use any surviving directory from the 1920s; where several are available, we take the directory nearest in time to 1925" (BF, p. 491).

[21] *Adreßbuch Stadt und Kreis Worms* (1925). Worms: Buchdruckerei Eugen Kranzbühler.

makes clear, this comparison of observables is not completely satisfactory. It makes a major assumption: namely, there are no other city characteristics that are correlated with local political conditions and social capital and that affected the odds of producing a directory in the 1920s and having it survive until today.

A second selection problem arises from the clubs a given directory actually reports. To be a useful measure of social capital, the directories have to either include all relevant clubs or report unbiased samples of such clubs. The historiography says they do not. A study of Tübingen in the late 1920s states that the city's directory "normally covered nearly two-thirds of all local voluntary associations." (Koshar 1982, p. 32) The selection of clubs to list in the directories is probably correlated with their suitability as Nazi recruiting grounds. Directories may systematically undercount the sorts of groups that would be hostile to Nazi recruiting efforts. Workers' organizations are a clear example, for as BF acknowledges (p. 518 and appendix E.5), they would not be fertile ground for Nazi members. A 1925 directory for Bonn, for instance, includes almost no associations whose members were likely to have been working class.[22] Workers' organizations proliferated in the 1920s. Yet in the BF data set, some large, industrial cities have suspiciously few clubs: Essen (population in 1925, 630,000) has 13 clubs total in the BF data.[23] BF does not discuss the issue, but as Appendix C.3 shows, the authors could have checked their club listings against external sources.

We also doubt BF's use of the history of associations to defend two important assertions. First, the distribution of associations across cities (measurement issues aside) has to be exogenous. Second, the clubs they count cannot be ideologically akin to the Nazis; in that case, joining the Nazi Party would reflect a political orientation rather than social capital. To support the first claim, BF claims their data for

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[22] *Einwohner-Buch der Stadt Bonn* (1927) Bonn: Druck und Verlag J.F. Carthaus. Professional and business groups account for about one-third of all associations listed in the 1925 directory for Worms (see Appendix Sections C.3 and C.4). BF apparently excludes these groups from the social-capital proxy, although BF does not say that explicitly.

[23] The appendix to BF (Section E.5) notes that workers associations "are at best weakly associated with Nazi Party entry," but does not discuss the possibility that such bodies are undercounted in the directories.

the 1920s reflects a persistent "culture of associational life" created in the nineteenth century (BF 2017, pp. 483-87). "After controlling for city size, the share of Catholics, and the proportion of workers, we believe that differences in the density of associations are reasonably exogenous for the purpose of our study (i.e., driven by deep historical factors that have no direct link with Nazi Party entry)." (BF 2017, p. 487 BF (pp. 481-84) emphasizes (correctly) that the pre-March Revolution period (1815-1848) saw both a flowering of liberal and democratic associations and concerted effort to suppress many of them. The number of clubs then grew dramatically from 1848 to 1918.

To support the exogeneity claim BF's authors report a regression for 39 of the 229 cities in their data. They know the number of delegates that local associations in these places sent to the 1848 Democratic Congress in Berlin (BF 2017, Appendix F). This variable explains 13 percent of the number of *Turnverein* (gymnastic club) members in 1863 and 46 percent of their clubs per capita variable for the 1920s. For the early 1860s, they also construct an index using numbers of *Turnverein* members and attendees at a choral festival. The index explains about 20 percent of the variation in the BF Nazi recruitment variable for 1925-1933 for the 150-odd cities for which this information is available.

These statistical results do not reassure. First, they pertain to only part of the BF sample of 229 cities, which, because they did not locate most extant directories, represents less than half of the universe of cities. Second, the groups extant in 1848 had a different social, confessional, and political basis than those in the 1920s, after a period when associations proliferated dramatically (Berman 1997, p 413). The former survived restrictions imposed by authoritarian governments; the latter arose under very different political conditions. BF exacerbates this problem by dropping all religious clubs. Catholic associational life in particular took off in the later nineteenth century. BF's 1848 clubs would include almost no Catholic groups, which (so we will see) were (later) usually hostile to the Nazi Party. In addition, both Koshar (1982, p. 33) and Tenfelde (2000, pp.95-96) stress that Weimar witnessed the growth of increasingly diverse associations, often devoted to working-class members. It would be very difficult to believe that all these developments are exogenous in the 1920s.

BF's second important assumption asserts that the clubs were not ideologically close to the Nazis. While nineteenth-century nationalism might have been less xenophobic than the Nazis', the rich literature on German associational life in the period from 1848 to World War I stresses a rapid growth of civil-society organizations devoted to nationalist goals such as a fleet to challenge Britain and colonies in Africa and elsewhere.[24] Such nationalist concerns continued to permeate associational life in the Weimar period, as one careful local study (Allen 2014, pp. 16-19) observes. Gardening clubs hosted nationalist speakers. Even choral societies split along ideological lines. The Nazis used some of these associations as hiding places once their party was banned. Koshar (1987, p. 20) notes that "After Hitler's 1923 coup attempt failed, the [Nazi] party dissolved into sports clubs, sharpshooting associations, and hiking organizations." Anheier (2003, 66-71) makes a similar observation. The Nazis later regained the right to recruit members, but the strength of those ersatz-Nazi groups reflected the Party not through the mechanism stressed in BF, but directly: some were, temporarily, little more than the Nazi Party in disguise.

*Associations and the Roman Catholic Church*

BF excludes two sets of clubs from their social-capital proxy: the "political" and "religious." They define neither, and the the directories do not clearly identify such associations, either. Figure 3 lists a school association whose leader is a minister (*Pfarrer*).[25] Was this school religious? (In contrast, the prior entry *says* it is a Catholic association.) This directory (like others) has a separate section for religious organizations, but many bodies listed elsewhere, such as leisure-time groups or, in this case, a charity, had the backing of a political or religious body. Dropping clubs (as BF does) biases the political

[24] For the colonial associations, see Conrad (2011, pp.25-27) and Speitkamp (2014, pp. 19-20). The far larger Navy League (*Flottenverein*) agitated for a German fleet that could challenge British seagoing supremacy. Several organizations created to honor the memory of the 1870/71 victory over France eventually morphed into right-wing political organizations.

[25] The association is the *Kinderschulverein*, seventh from last on the directory's page 493.

orientation of sample clubs in unpredictable ways. In addition, simply dropping these associations makes it impossible to understand potential differences between social capital in a political organization and social capital in a choral society. This question warrants an empirical test, not exclusion from the sample.[26]

BF excludes religious organizations because "we are interested in the 'bottom-up' characteristics of grassroots organizations, not in ready-made sociality created by members of the church hierarchy" (BF 2017, p. 486). BF does not follow the logic of this argument. Many if not most of the other clubs in their social-capital proxy were in fact branches of a regional or national organization. More important, it is unclear why "bottom up" associations in general would involve more social capital or have a greater impact on Nazi recruiting.

The argument stresses the Catholic Church in particular. While certainly hierarchical, the German Catholic Church's overt and well-documented hostility to the Nazis in the 1920s means that omitting Catholic associations is far from neutral. The historiography leaves little doubt about the role of Catholic associations in this period. According to H. Mommsen (1988, p. 353), "In Catholic regions, as opposed to their Protestant counterparts, the NSDAP was only rarely able to penetrate the network of middle-class clubs and associations that had played such an important role in its expansion in northern Germany." Z. Zofka (1979, pp.168-169) notes that many Catholic associations strongly discouraged members from joining the Nazis and shows that in Bavaria, areas with strong local Catholic bodies had fewer Nazi members.[27]

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[26] BF tests for the difference between several types of associations, but since what they view as political and religious clubs are not in the data, they could not check to see whether political and religious groups are different. Nor can we.

[27] BF (2017, p.489) quotes Zofka as saying the chairmen of local associations "and other opinion leaders increasingly converted to the Nazi creed and induced other members" of associations "to follow." But Zofka stresses that Catholic associations remained hostile to the Nazi party. Brustein makes the same point (1996, pp. 166, 171).

BF's regressions all include a control for the percentage of the city's population that was Catholic. Unless the city's religious composition perfectly predicts the number of missing Catholic associations, however, excluding the Catholic clubs from the social capital proxy could easily bias the results in favor of BF's conclusions. In theory, one could test whether excluding religious and political associations affects the results in BF. We cannot do so because BF did not include the relevant counts in their replication data, nor do we know precisely which directories they used.

*What Do BF's Results Say about Social Capital?*

Do the results in BF necessarily imply anything about the role of social capital in Nazi support? BF does not model how the social capital embedded in these associations might have promoted Nazi recruiting, except to say that "associations facilitated Nazi recruitment" by spreading the party's message (BF 2017, p. 480, 490). The economics literature on social capital and networks suggests that the most effective way to use social capital to recruit people into the Nazi Party would be for a Nazi recruiter to join the association and ask other association members to identify the best sources of information in the group (the "gossips" in the association, in the language of an experimental study) (Jackson 2019; Banerjee et al 2019). The recruiter would then pass favorable information about the Nazi Party to these gossips: for instance, telling them about an upcoming Nazi speaker, an effective tactic used by the party (Brustein 1996, p. 163; Allen 2014, pp. 80-82). That would be more efficient than approaching each association member individually or (according to the experiments) going to the group's leaders, and it would use the association's social capital, the connections between the members. The result would be the relationship highlighted in BF between associations and Nazi Party recruitment.

That is not, however, the only possible interpretation of an empirical relationship between associational density and Nazi recruitment. Social capital is about ties among people, here proxied by membership in organizations. A different explanation is equally consistent with the findings and has

nothing to do with interpersonal ties and thus social capital. It would simply require that Nazi recruiters know something about what sort of person would join which group.

Historical studies (so we have seen) suggest that was the case in German towns and that information about groups' membership and their probable political sympathies was often common knowledge, even for groups that were not overtly political. Memberships usually aligned internally along class or religious lines that would make it easy to guess at political leanings.[28] Recruiters could exploit this information and use it for recruiting without ever joining groups or making use of the associations' social capital, the connections between members. They could, for instance, just give members of a promising group leaflets about Nazi speakers or invite them to a Nazi talk. The tactic would be no different from, say, an American political campaign publicizing a Republican candidate among gun owners or a Democratic candidate advertising on MSNBC. It would involve no social capital, because it did not rely on connections among club members. Yet the statistical relationship between Nazi recruitment and the number of clubs would be the same as in BF, because more clubs would give recruiters more chances to find associations whose members would find the Nazi Party appealing.

Nazi recruiters could exploit this information about memberships even without prospecting among openly political groups. If anything, excluding the Catholic groups might make the remaining ones even more likely to have an above average number of Nazis, and so reinforce the relationship between associations and party membership, all without any involvement of social capital.

Either method of recruiting (via social capital or via knowledge about membership) would lead to a positive correlation between party recruitment and the number of associations in a town, as we show using a simple model in Appendix B.4. If the Nazi party has some recruiters who use the first method and

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[28] Allen (2014, pp. 16-19); Brustein (1996, pp. 163-71); Koshar (1982, pp. 31-36). Tenfelde (2000) recounts the history of the Hessian town of Eschau, with two competing sets of clubs. Members of a given club would patronize a given pub, hairdresser, etc. According to Tenfelde, the political associations of the two sets of clubs post-date World War II, but the example serves to show that someone could tell a lot about a person by knowing which associations they belonged to.

some who rely on the second, then BF's regression coefficients would simply add the effect of the two methods of recruiting. If this sum were positive and significant, that would say nothing about social capital, because the whole effect could simply be the other method of recruiting. This problem of interpreting the coefficients' meaning is serious.

Here one might object that this distinction between recruiters' knowledge and social capital is interesting but not really a problem for BF's claims. The number of clubs is a standard proxy for social capital, and it does not really matter what the connections were between members of associations. BF argues that places with more associations had more Nazis and that this evidence says something important about the town and about social capital. We would agree that such a relationship would say something about the *town*. But it would not necessarily reveal anything about social capital unless it involved the connections between the members of associations. To argue otherwise runs counter to the economic theory of social capital and to the broader social science research on social capital. Ties between members of groups figure prominently in all that research, and they are essential if we want to pin down what precisely social capital is (Jackson 2019).

The Nazi Party succeeded by crafting nationalistic proposals that attracted a core group of members and then efficiently marketing this program to a broader group of voters (Brustein 1996, pp. 1, 9, 57-60, 118-119, 157-182). In recruiting members, it may have targeted receptive audiences, much as modern political campaigns do, or harnessed connections between individuals. Yet only the second path relied on social capital, and BF's evidence cannot tell us which path was taken. Only additional historical research about Nazi recruiting would reveal which one it was. Did Nazis draw new party members from associations to which they themselves belonged? Or did the Nazis recruit from groups they themselves had not joined? A careful reading of local historical studies might provide an answer.[29]

[29]As BF (4887-489) notes, Anheier (2003) shows that Nazi recruiters relied on social connections to attract new members, at least in places with no party office or district organization. But the associations in question here were far right groups, and many had been covers for the Nazi Party when it had been banned, precisely the sort of connection that undermines BF's argument.

*Conclusion*

PP and BF muster evidence to argue that deep, slowly changing historical forces played an important role in the extreme anti-Semitism that underlay so much of Weimar political life, including the rise of the Nazi Party. Little of this evidence stands up to scrutiny, however. Our discussion of the flaws in PP and BF does not rule out a role for persistent social capital or a longstanding culture of anti-Semitism. These factors may well help explain the rise of the Nazi Party in the 1920s and 1930s and also be important for questions in other times and places. The evidence that PP and BF offer just does not demonstrate this was the case in Weimar Germany.

The persistence literature, of which PP and BF are two examples, has exploded, but it has faced criticisms. Many critiques concern data. Guinnane (2023), for example, criticizes the historical population data used in many persistence studies. Others raise different concerns. Dippel (2021) points to the potential lack of historical expertise when general-interest economics journals referee economic history papers. This worry does not just apply to persistence papers, of course. Abad and Maurer voice concerns that resemble our criticisms of BF and PP: they worry about the misuse of historical sources as well as the vague mechanisms invoked when authors do not include informal or formal models. Voth distinguishes the econometric problems that arise when the dependent variable and the explanatory variable in a persistence study are conceptually close (as with anti-Semitic attitudes in the 1920s and the fourteenth century in PP) in contrast to instances when the dependent and explanatory variable are different (as when the dependent variable is income today and the explanatory variable is a non-economic variable in the past).[30] Yet it is harder to find broader lessons that would strengthen all this literature. What general insights can we offer?

[30] For bibliographies of the persistence literature, see Abad (2021); Dippel (2021); Cioni (2022); Nunn (2021 ); Voth (2021). Other notable data criticisms include Albouy's (2012) concerns about the instrument used in

The first is that authors should model what they analyze and do so with careful attention to history. Many persistence studies do appeal, at least implicitly, to models drawn from cultural evolution.[31] But authors tend not to take these models seriously, particularly to explain different equilibria, even though multiple outcomes are very much a part of cultural evolution. More surprising, persistence studies rarely consider models drawn from other areas of economics.[32] Those models could provide alternative explanations for persistence that could better fit both the history and the evidence. Comparing both sorts of models would make the choice clear.

Thinking carefully about the causal relationships in these models requires understanding the history but it can in turn improve both the theory and the econometrics. PP, for example, did not consider the relationship between the crucial pogrom proxy and later history. Our analysis uncovered the reason for the outliers that drove many of the PP regressions and also yielded an historically superior explanation for the results: political coincidence across time. Of course, we all have trouble considering alternatives while in the midst of discovery. We may have unearthed new data and results, are fired up about the causal relationships we have uncovered, and are eager to satisfy potential referees and editors. Those editors and referees tend to stress methods of establishing causation that are standard in empirical economics rather than a model from elsewhere in economics that is a better match for both the data and the history.

Similarly, BF does not consider connections between civil-society organizations and the Nazi Party that have nothing to do with social capital. Rather than joining a particular club and using the social ties within the group, as in BF's account, Nazi recruiters could exploit their information about what sort

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Acemoglu (2001), as well as Austin's (2008) doubts about the same paper, the related paper Acemoglu (2002), and the data in Nunn (2008).

[31] See Bowles (2021) and Nunn (2021).

[32] One exception is Voth (2021), who points out that economic geography could provide an alternative explanation for some results in persistence studies.

of people belonged to the club and use that knowledge to hand out Nazi campaign literature. That would be no different from political consultants' exploiting advertising information in a modern electoral campaign. Such use of information fits the history and yields the same econometric results, as we show formally in Appendix B.4. Only further historical research could distinguish the two models.

Our second lesson suggests greater attention to the assumptions underlying the econometrics. Sometimes we cannot verify these assumptions internally, as with the condition that an instrumental variable is uncorrelated with a regression error term. Here greater attention to the history and to the processes that generated our sources can help a lot. Similarly, econometric results may reflect outliers. We have exploited several standard techniques to assess the outlier problem in PP and BF. Taking the outlier problem seriously can help protect us from fragile conclusions, and also suggests where the history may reveal an alternative explanation.

Persistence studies may be especially vulnerable to the problems created by non-random sampling. Authors typically check that the observations in their historical sample match known data from the entire country, economy, or statistical universe. BF's authors, for instance, compared their sample cities to all German cities. This is a useful step, but it is not sufficient. Comparisons of observables tell us nothing about unobservable variables, and the unobservables can drive the results. This is even more likely when (as in BF) the selection of observations from the possible universe involves mechanisms we cannot investigate. Only a careful examination of the history can tell us whether such unobservables are likely to cause problems. The same applies to assumptions that our data arose via a process that was exogenous as far as our dependent variable goes, at least once we have added our controls. BF makes this assumption, and in this case, the history casts serious doubt on it.

We offer one more and related lesson: we should investigate the history seriously before we assuming either that treatments in the past are exogenous or "seemingly random." Saying that a treatment in the past is "seemingly random" just confesses ignorance. We should instead probe the history. That history can also tell us more about how the data came to be and thus how it might not tell us what we

think it does. As our discussion of both PP and BF shows, understanding the history is not just a matter of accurate description or context. It guides our theoretical understanding, our econometric identification, our understanding of potential data problems. .

We close with an example that is not a persistence study but that illustrates these issues in another context related to the Nazi regime's power. A well-published study used a regression discontinuity design applied to World War II France to determine whether the Germans who occupied the country militarily faced more resistance in areas they controlled directly or in places where they combined their military presence with a regime of French collaborators (the Vichy Government).[33] The authors ask whether, in the neighborhood of the line separating Vichy from the rest of France, anti-German attacks were more likely against targets under direct German control or where the Germans also had the Vichy regime. The authors found fewer anti-German attacks in the Vichy area. Their research design assumed that the armistice line dividing France into two zones "may be plausibly viewed as quasi-random." A more careful historical investigation, however, belied this assumption (Kocher and Monteiro 2016). Major dual-track railroads ran along the German-occupied side of the border. The Wehrmacht chose that dividing line to ensure that Germans controlled these railroads, which supplied German troops on the Atlantic. Many of the attacks that are the paper's outcome variable in turn targeted those railroad lines. Thus the border was not assigned randomly. The German-occupied side of the border experienced more attacks because the railroads were military targets and remained under their direct control because the German army wanted to protect its supply lines. The relationship uncovered by the regression discontinuity design was therefore military and reflects the reasons for the line's placement. It had little to do with the advantages

[33] Ferwerda and Miller 2014. The authors analyze the period from November 1942 to September 1944 when Germany occupied all of France militarily but left the Vichy authorities in place in the south, which had been unoccupied before 1942.

of an occupier's direct control versus cooptation. Failing to investigate the history here undermined all the evidence behind the authors' claim.

**Table 1: Replication and sensitivity in PP**

| VARIABLES | (1) NSDAP28 | (2) NSDAP28 | (3) PCA_stnd | (4) PCA_stnd | (5) Deported | (6) Deported |
|---|---|---|---|---|---|---|
| Pogrom | 0.0142** | 0.00294 | 0.290** | 0.0588 | 0.142** | 0.135 |
| | (0.00567) | (0.00283) | (0.132) | (0.0670) | (0.0706) | (0.137) |
| LogPop | -0.00254 | 0.00121 | -0.0875 | -0.0433 | 0.241*** | 1.135*** |
| | (0.00219) | (0.000900) | (0.0646) | (0.0296) | (0.0841) | (0.0311) |
| Jewish_pc | 0.00174 | 0.000705 | 0.0215 | 0.0601 | 0.0743** | 0.384*** |
| | (0.00190) | (0.00131) | (0.0971) | (0.0439) | (0.0348) | (0.0340) |
| LogJews | | | | | 0.815*** | |
| | | | | | (0.0822) | |
| Prot_pc | 0.000290*** | 0.000138*** | 0.284*** | 0.254*** | -0.0039*** | -0.00431** |
| | (8.84e-05) | (4.06e-05) | (0.0757) | (0.0322) | (0.00116) | (0.00178) |
| Constant | 0.0340* | -0.00295 | -0.0801 | -0.341*** | -2.612*** | -7.613*** |
| | (0.0195) | (0.00856) | (0.106) | (0.0668) | (0.462) | (0.372) |
| Observations | 325 | 325 | 311 | 311 | 278 | 278 |
| Estimated by | OLS | QR | OLS | QR | Poisson | Poisson |

Source: All models estimated using PP replication data

Notes: Column (1) replicates PP Table VI Column (2). The dependent variable is the Nazi vote share in the 1928 election. Column (2) estimates Column (1) as a quantile (median) regression. Column (3) replicates PP Table VII Column (1). The dependent variable is the first principle component of the six outcome variables in PP Table VI. Column (4) estimates Column (3) as a quantile (median) regression. Column (5) replicates PP Table VI Column (4). The dependent variable is the number of Jews deported from the place. Column (6) estimates the same model but drops the superfluous "LogJews" regressor. Column (6) uses the same sub-sample as Column (5); see text for discussion of coding error that unnecessarily drops observations from PP's Table VI Column (5). The precise definitions of the controls varies across specifications; this table always uses the definition that underlies the model in PP. In every case, the Pogrom proxy is defined as in the text, and "Prot_pc" is the percentage Protestant in 1925. In Columns (1) and (2), the city population and Jewish percentage are from the 1925 census. In Columns (3) – (6) they are from the 1933 census. In Columns (3) and (4) all variables, including the dependent variable, have been standardized. See the Appendix for additional checks that consider provincial interactions with the pogrom proxy as well as functional-form issues in the poisson models.

**Table 2: The liberal parties as placebos**

| | Dependent variable | Pogrom point Estimate | SE | Obs | Adj R-sq | Model |
|---|---|---|---|---|---|---|
| | 1924 election | | | | | |
| 1 | DDP24 | 0.0109** | (0.00544) | 325 | 0.265 | OLS |
| 2 | DDP24 | 0.00682 | (0.00523) | 325 | | QR |
| 3 | DVP24 | 0.00955 | (0.00799) | 325 | 0.233 | OLS |
| 4 | DVP24 | 0.0167 | (0.0109) | 325 | | QR |
| 5 | DDP_DVP24 | 0.0205* | (0.0110) | 325 | 0.306 | OLS |
| 6 | DDP_DVP24 | 0.0294** | (0.0116) | 325 | | QR |

Note: The table presents placebo checks for models analogous to PP Table VI, Column (3). We report the point-estimate and standard error for the pogrom proxy; every regression includes all the controls in VV's analogous model. The DDP and DVP grew out of the Wilhelmine-era National Liberal and Progressive parties. DVP_DDP is the sum of the two party's vote shares. Appendix Tables A6.1-A6.4 for other parties and elections. In 1928, both the DDP and DVP had drifted right. The DVP in particular had shared some electoral lists with a right-wing party that had some ideological overlap with the Nazis (the *Volksnationale Reichsvereinigung*). The DVP results for 1928 are different from what we show here. Tables A6.1 – A6.4 also indicate that the effect of the pogrom proxy on electoral outcomes varies by region.

**Table 3: Using alternative definitions of the stability index**

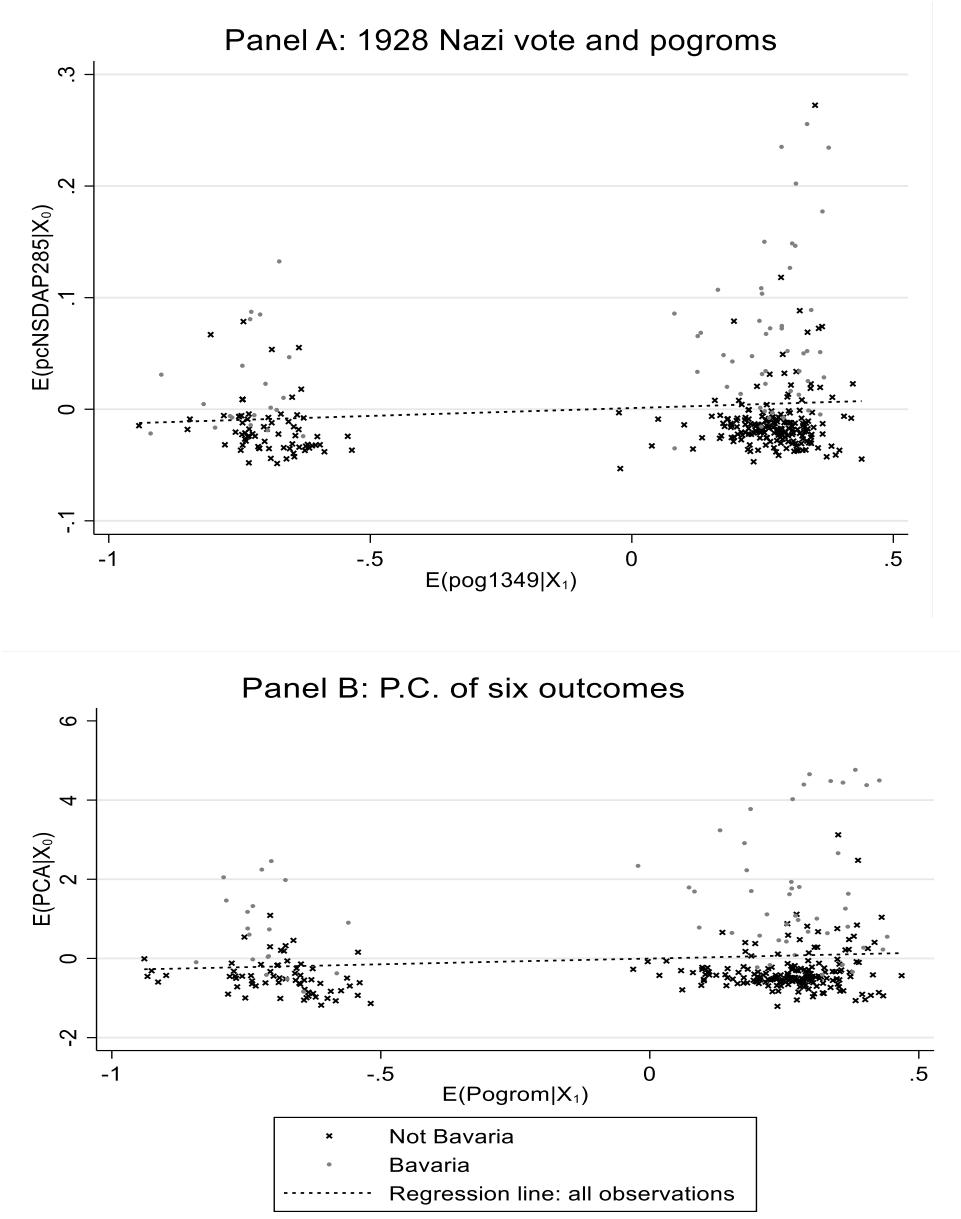
| VARIABLES | (1) Nazi_entry | (2) Nazi_entry | (3) Nazi_entry | (4) Nazi_entry | (5) Nazi_entry |
|---|---|---|---|---|---|
| Clubs_all_pc | 0.349*** | 0.263 | 0.0999 | 0.198 | 0.134** |
| | (0.128) | (0.183) | (0.147) | (0.183) | (0.0524) |
| Stability index | | | | | 0.741 |
| | | | | | (0.631) |
| Stability index x clubs_all_pc | | | | | -0.0424 |
| | | | | | (0.0329) |
| LnPop25 | 0.192 | 0.371* | 0.0324 | -0.0164 | 0.136** |
| | (0.134) | (0.218) | (0.125) | (0.168) | (0.0512) |
| Cath_pc25 | -0.525 | 0.0644 | -0.998** | -1.490** | -0.804*** |
| | (0.388) | (0.554) | (0.442) | (0.597) | (0.115) |
| BCollar_pc25 | -0.272 | 1.287 | -0.553 | -1.511 | -1.883*** |
| | (1.929) | (2.128) | (1.427) | (1.497) | (0.385) |
| Stability index x: | | | | | |
| LnPop25 | | | | | -0.0224 |
| | | | | | (0.0465) |
| Cath_pc25 | | | | | -0.239** |
| | | | | | (0.0924) |
| BCollar_pc25 | | | | | -0.955*** |
| | | | | | (0.282) |
| Constant | -2.239 | -4.869* | -0.0791 | 0.529 | -0.599 |
| | (1.833) | (2.712) | (1.683) | (2.206) | (0.693) |
| Observations | 58 | 58 | 54 | 35 | 225 |
| Adjusted R-squared | 0.108 | | 0.055 | 0.178 | 0.217 |
| Estimator | OLS | QR | OLS | OLS | OLS |
| Mean (med) dep var | 0.463 | 0.463 | 0.00923 | 0.0266 | 0.0266 |
| Reg beta | 0.440 | 0.332 | 0.141 | 0.265 | 0.265 |

Source: Computed from BF replication data

Note: Column (1) replicates BF Table 7, Column (3). The sub-sample includes only "unstable" states as defined by BF. Column (2) estimates the model in Column (1) by quantile (median) regression. Column (3) drops the third element from the stability index, but treats the median state as do BF's authors, assigning it to the "unstable" category. Column (4) defines the stability index as in BF but considers the median state to be "stable." Column (5) replicates the regression that underlines BF Appendix Figure A7. (BF does not report the actual regression). The sample for Column (5) is the entire dataset, including Prussia. The model uses the continuous stability index as defined in BF. See our appendix text (section B.3) for additional discussion of this model and BF Figure A7. Our

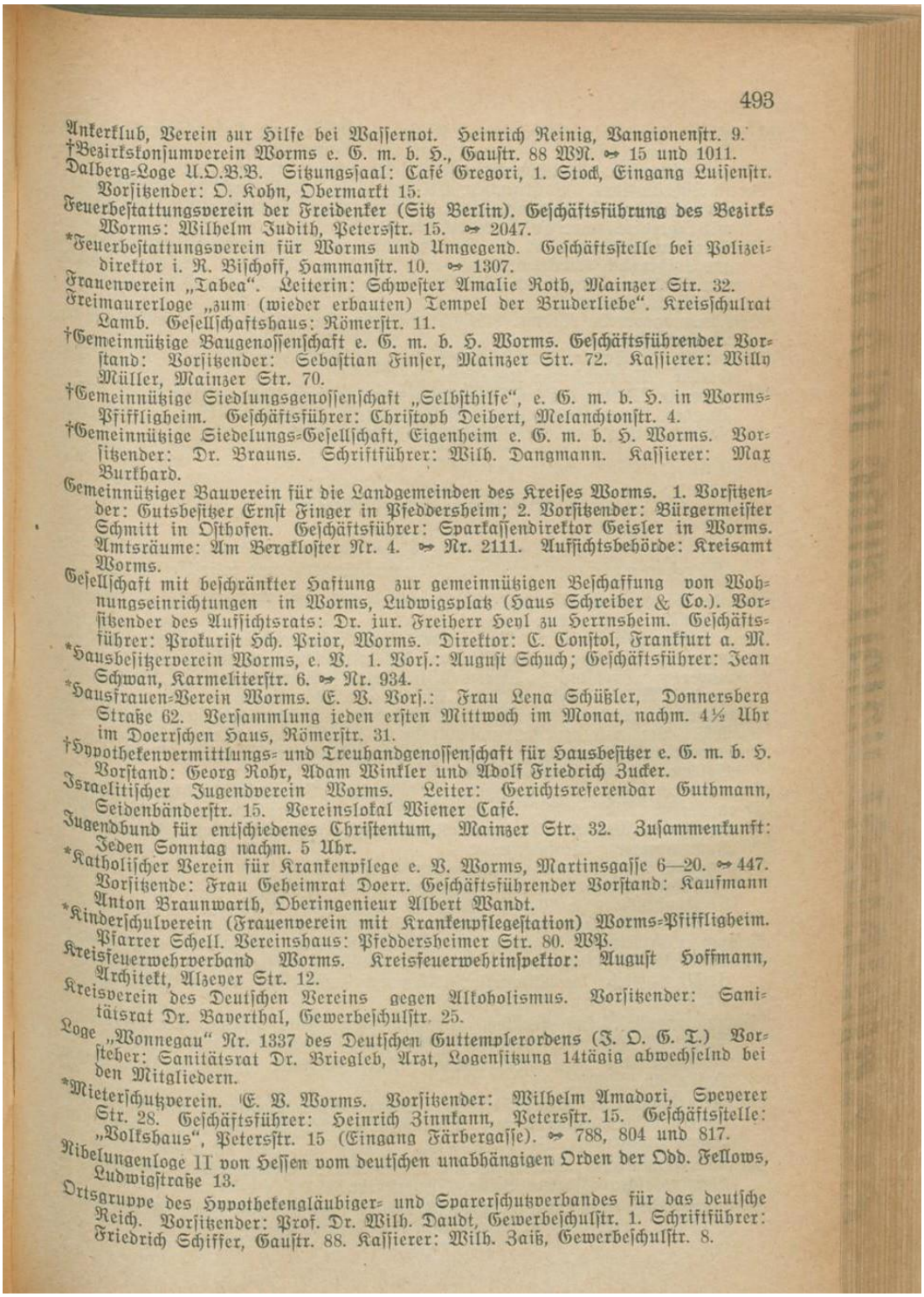
appendix Table B3.6 reports computations for the net effect of stability in selected states, showing that with this specification, social capital only affects Nazi recruitment in *stable* states.

Figure 1




Note: Each figure shows a partial-regression plot. See Belsey, Kuh, and Welsch (1980, p.30). The x-axis in Panel A plots the residuals from a regression of POG1349 on the other regressors ($X_1$), and the y-axis plots the corresponding residuals from a regression of the 1928 Nazi vote share on the independent variables other than POG1349 ($X_0$). The specification corresponds to PP Table VI, Column (x). The solid line plots the implied linear fit, which is (by construction) the regression reported in PP, Table VI, column (2): the 1928 Nazi vote share = .0142*POG1349, standard error = .00567). Panel B reports the same

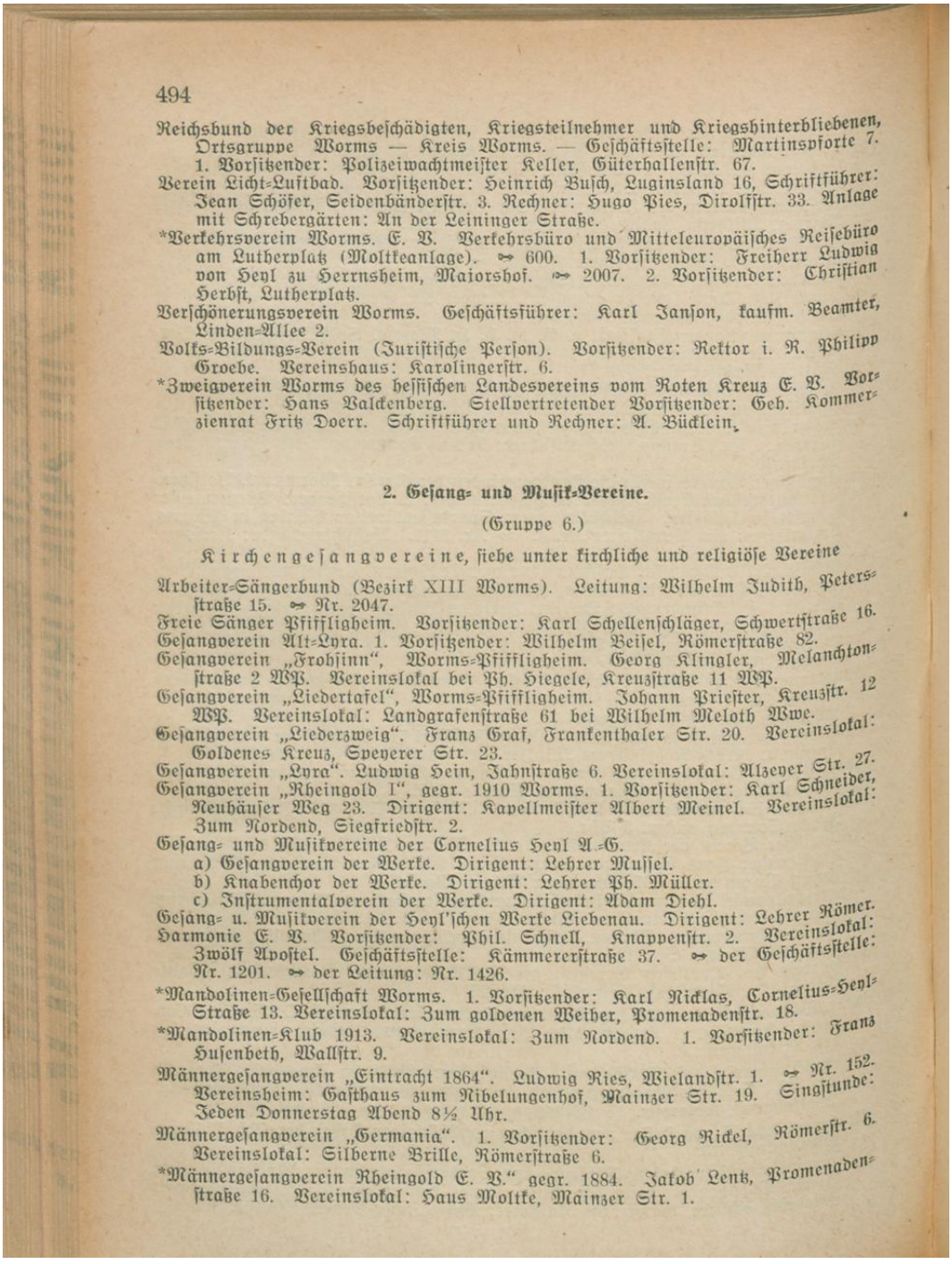
information for the regression reported in PP Table VII, Column (2). Here the dependent variable is the first principle component computed from the six outcome variables used in PP Table VI. All variables used in the regression underlying Panel B are standardized, as they are in PP.



493

Ankerklub, Verein zur Hilfe bei Wassernot. Heinrich Reinig, Vangionenstr. 9.
†Bezirkskonsumverein Worms e. G. m. b. H., Gaustr. 88 WN. ⇒ 15 und 1011.
Dalberg-Loge U.O.B.B. Sitzungssaal: Café Gregori, 1. Stock, Eingang Luisenstr. Vorsitzender: O. Kohn, Obermarkt 15.
Feuerbestattungsverein der Freidenker (Sitz Berlin). Geschäftsführung des Bezirks Worms: Wilhelm Judith, Petersstr. 15. ⇒ 2047.
*Feuerbestattungsverein für Worms und Umgegend. Geschäftsstelle bei Polizeidirektor i. R. Bischoff, Hammanstr. 10. ⇒ 1307.
Frauenverein „Tabea". Leiterin: Schwester Amalie Roth, Mainzer Str. 32.
Freimaurerloge „zum (wieder erbauten) Tempel der Bruderliebe". Kreisschulrat Lamb. Gesellschaftshaus: Römerstr. 11.
†Gemeinnützige Baugenossenschaft e. G. m. b. H. Worms. Geschäftsführender Vorstand: Vorsitzender: Sebastian Finser, Mainzer Str. 72. Kassierer: Willy Müller, Mainzer Str. 70.
†Gemeinnützige Siedlungsgenossenschaft „Selbsthilfe", e. G. m. b. H. in Worms-Pfiffligheim. Geschäftsführer: Christoph Deibert, Melanchtonstr. 4.
†Gemeinnützige Siedelungs-Gesellschaft, Eigenheim e. G. m. b. H. Worms. Vorsitzender: Dr. Brauns. Schriftführer: Wilh. Dangmann. Kassierer: Max Burkhard.
Gemeinnütziger Bauverein für die Landgemeinden des Kreises Worms. 1. Vorsitzender: Gutsbesitzer Ernst Finger in Pfeddersheim; 2. Vorsitzender: Bürgermeister Schmitt in Osthofen. Geschäftsführer: Sparkassendirektor Geisler in Worms. Amtsräume: Am Bergkloster Nr. 4. ⇒ Nr. 2111. Aufsichtsbehörde: Kreisamt Worms.
Gesellschaft mit beschränkter Haftung zur gemeinnützigen Beschaffung von Wohnungseinrichtungen in Worms, Ludwigsplatz (Haus Schreiber & Co.). Vorsitzender des Aufsichtsrats: Dr. iur. Freiherr Heyl zu Herrnsheim. Geschäftsführer: Prokurist Hch. Prior, Worms. Direktor: E. Constol, Frankfurt a. M.
*Hausbesitzerverein Worms, e. V. 1. Vors.: August Schuch; Geschäftsführer: Jean Schwan, Karmeliterstr. 6. ⇒ Nr. 934.
*Hausfrauen-Verein Worms. E. V. Vors.: Frau Lena Schüßler, Donnersberg Straße 62. Versammlung jeden ersten Mittwoch im Monat, nachm. 4½ Uhr im Doerrschen Haus, Römerstr. 31.
†Hypothekenvermittlungs- und Treuhandgenossenschaft für Hausbesitzer e. G. m. b. H. Vorstand: Georg Rohr, Adam Winkler und Adolf Friedrich Zucker.
Israelitischer Jugendverein Worms. Leiter: Gerichtsreferendar Guthmann, Seidenbänderstr. 15. Vereinslokal Wiener Café.
Jugendbund für entschiedenes Christentum, Mainzer Str. 32. Zusammenkunft: Jeden Sonntag nachm. 5 Uhr.
*Katholischer Verein für Krankenpflege e. V. Worms, Martinsgasse 6—20. ⇒ 447. Vorsitzende: Frau Geheimrat Doerr. Geschäftsführender Vorstand: Kaufmann Anton Braunwarth, Oberingenieur Albert Wandt.
*Kinderschulverein (Frauenverein mit Krankenpflegestation) Worms-Pfiffligheim. Pfarrer Schell. Vereinshaus: Pfeddersheimer Str. 80. WP.
Kreisfeuerwehrverband Worms. Kreisfeuerwehrinspektor: August Hoffmann, Architekt, Alzeyer Str. 12.
Kreisverein des Deutschen Vereins gegen Alkoholismus. Vorsitzender: Sanitätsrat Dr. Bayerthal, Gewerbeschulstr. 25.
Loge „Wonnegau" Nr. 1337 des Deutschen Guttemplerordens (J. O. G. T.) Vorsteher: Sanitätsrat Dr. Briegleb, Arzt, Logensitzung 14tägig abwechselnd bei den Mitgliedern.
*Mieterschutzverein. E. V. Worms. Vorsitzender: Wilhelm Amadori, Speyerer Str. 28. Geschäftsführer: Heinrich Zinnkann, Petersstr. 15. Geschäftsstelle: „Volkshaus", Petersstr. 15 (Eingang Färbergasse). ⇒ 788, 804 und 817.
Nibelungenloge II von Hessen vom deutschen unabhängigen Orden der Odd. Fellows, Ludwigstraße 13.
Ortsgruppe des Hypothekengläubiger- und Sparerschutzverbandes für das deutsche Reich. Vorsitzender: Prof. Dr. Wilh. Daudt, Gewerbeschulstr. 1. Schriftführer: Friedrich Schiffer, Gaustr. 88. Kassierer: Wilh. Zaiß, Gewerbeschulstr. 8.

FIGURE 2
TWO PAGES FROM THE CITY DIRECTORY FOR WORMS 1925





Reichsbund der Kriegsbeschädigten, Kriegsteilnehmer und Kriegshinterbliebenen, Ortsgruppe Worms — Kreis Worms. — Geschäftsstelle: Martinspforte 7. 1. Vorsitzender: Polizeiwachtmeister Keller, Güterhallenstr. 67.
Verein Licht-Luftbad. Vorsitzender: Heinrich Busch, Luginsland 16, Schriftführer: Jean Schöfer, Seidenbänderstr. 3. Rechner: Hugo Pies, Dirolfstr. 33. Anlage mit Schrebergärten: An der Leininger Straße.
*Verkehrsverein Worms. E. V. Verkehrsbüro und Mitteleuropäisches Reisebüro am Lutherplatz (Moltkeanlage). ⇒ 600. 1. Vorsitzender: Freiherr Ludwig von Heyl zu Herrnsheim, Maiorshof. ⇒ 2007. 2. Vorsitzender: Christian Herbst, Lutherplatz.
Verschönerungsverein Worms. Geschäftsführer: Karl Janson, kaufm. Beamter, Linden-Allee 2.
Volks-Bildungs-Verein (Juristische Person). Vorsitzender: Rektor i. R. Philipp Groebe. Vereinshaus: Karolingerstr. 6.
*Zweigverein Worms des hessischen Landesvereins vom Roten Kreuz E. V. Vorsitzender: Hans Valckenberg. Stellvertretender Vorsitzender: Geh. Kommerzienrat Fritz Doerr. Schriftführer und Rechner: A. Bücklein.

## 2. Gesang- und Musik-Vereine.

(Gruppe 6.)

Kirchengesangvereine, siehe unter kirchliche und religiöse Vereine

Arbeiter-Sängerbund (Bezirk XIII Worms). Leitung: Wilhelm Judith, Petersstraße 15. ⇒ Nr. 2047.
Freie Sänger Pfiffligheim. Vorsitzender: Karl Schellenschläger, Schwertstraße 16.
Gesangverein Alt-Lyra. 1. Vorsitzender: Wilhelm Beisel, Römerstraße 82.
Gesangverein „Frohsinn", Worms-Pfiffligheim. Georg Klingler, Melanchtonstraße 2 WP. Vereinslokal bei Ph. Hiegele, Kreuzstraße 11 WP.
Gesangverein „Liedertafel", Worms-Pfiffligheim. Johann Priester, Kreuzstr. 12 WP. Vereinslokal: Landgrafenstraße 61 bei Wilhelm Meloth Wwe.
Gesangverein „Liederzweig". Franz Graf, Frankenthaler Str. 20. Vereinslokal: Goldenes Kreuz, Speyerer Str. 23.
Gesangverein „Lyra". Ludwig Hein, Jahnstraße 6. Vereinslokal: Alzeyer Str. 27.
Gesangverein „Rheingold I", gegr. 1910 Worms. 1. Vorsitzender: Karl Schneider, Neuhäuser Weg 23. Dirigent: Kapellmeister Albert Meinel. Vereinslokal: Zum Nordend, Siegfriedstr. 2.
Gesang- und Musikvereine der Cornelius Heyl A.-G.
a) Gesangverein der Werke. Dirigent: Lehrer Mussel.
b) Knabenchor der Werke. Dirigent: Lehrer Ph. Müller.
c) Instrumentalverein der Werke. Dirigent: Adam Diehl.
Gesang- u. Musikverein der Heyl'schen Werke Liebenau. Dirigent: Lehrer Römer.
Harmonie E. V. Vorsitzender: Phil. Schnell, Knappenstr. 2. Vereinslokal: Zwölf Apostel. Geschäftsstelle: Kämmererstraße 37. ⇒ der Geschäftsstelle: Nr. 1201. ⇒ der Leitung: Nr. 1426.
*Mandolinen-Gesellschaft Worms. 1. Vorsitzender: Karl Nicklas, Cornelius-Heyl-Straße 13. Vereinslokal: Zum goldenen Weiher, Promenadenstr. 18.
*Mandolinen-Klub 1913. Vereinslokal: Zum Nordend. 1. Vorsitzender: Franz Husenbeth, Wallstr. 9.
Männergesangverein „Eintracht 1864". Ludwig Ries, Wielandstr. 1. ⇒ Nr. 152. Vereinsheim: Gasthaus zum Nibelungenhof, Mainzer Str. 19. Singstunde: Jeden Donnerstag Abend 8½ Uhr.
Männergesangverein „Germania". 1. Vorsitzender: Georg Rickel, Römerstr. 6. Vereinslokal: Silberne Brille, Römerstraße 6.
*Männergesangverein Rheingold E. V." gegr. 1884. Jakob Lentz, Promenadenstraße 16. Vereinslokal: Haus Moltke, Mainzer Str. 1.

Figure 2— continued

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