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Medieval Anti-Semitism, Weimar Social Capital, and the Rise of the Nazi Party: A Reconsideration

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Medieval Anti-Semitism, Weimar Social Capital, and the Rise of the Nazi Party: A Reconsideration

Abstract

The persistence literature in economics and related disciplines connects recent outcomes to events long ago. This influential literature marks a promising development but has drawn criticism. 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 broader lesson is that research of this type works best when it incorporates careful econometrics, serious consideration of underlying mechanisms (including formal theory), and, most important, scrupulous attention to history and to the limitations of historical data.

JEL-Codes: C180, D710, D720, D850, D910, L140, N010, N130, N140, Z100, Z120.

Keywords: historical persistence, medieval pogroms, social capital, culture, networks, Nazism, voting behavior, anti-Semitism, political parties, religion, empirical economics, data based estimates, econometrics.

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https://hoffman.sites.caltech.edu/documents/22791/Guinnane_Hoffman_appendix.pdf.

Much influential economic history today aims to show that long-ago events have consequences for more recent history. 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.¹

This paper considers an influential example of this genre: studies that invoke historical causes to explain the Nazi Party and anti-Semitic behavior in Germany in the 1920s and 1930s.² 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 anti-Semitic activities in the Weimar Republic and Nazi Germany. 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 are widely cited.³

These two papers 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. Current political events make the topic and these two papers’ explanation of great interest.⁴ 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.

¹ For an overview of the early development of this literature, see Nunn (2009).

² 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).

³ PP has 803 Google Scholar citations and BF 241 (as of September 19, 2022).

⁴ Edsall (2022), a thoughtful *New York Times* opinion piece, cites BF.

Some reflect tendentious specifications. Second, each paper's argument suffers from the lack of a model, mathematical or verbal, that would clarify the implicit assumptions and possible alternative explanations. Third, misinterpretations of history compound the econometric and modelling problems. The two papers largely ignore the influence of political and religious authorities who shaped both the regressors and the outcome variables. Anti-Jewish pogroms in the fourteenth century depended not just on anti-Semitic attitudes, but on the willingness of local authorities to protect Jewish communities. Likewise, some Weimar institutional actors actively discouraged joining the Nazi party. Finally, both articles code published data in ways that fail to respect the limitations of the historical sources.

Analogous weaknesses afflict other examples of the persistence literature, as Kelly (2019) shows in in critique of spatial errors in PP and in the persistence literature more generally. The remedy, from our perspective, is for persistence studies to be serious about models, about history, and about historical data. Otherwise, researchers risk being snared by explanations that are appealing but ultimately unsupported.

We first examine PP to ask whether the evidence supports the claim that medieval culture explains 1920s and 1930s anti-Semitism. We then turn to BF and ask whether social capital from the nineteenth century caused variations in Nazi Party membership. The answer for both PP and BF is that the evidence does not support the claims the papers make. In the conclusion, we suggest how economic historians can avoid troubles of this sort when they invoke causes in the past. To reduce the length of our text, we describe many of our econometric results briefly and then refer readers to the appendix for full documentation.

How Robust is the Evidence in PP?

PP's authors claim that anti-Semitism in 1920s and 1930s Germany reflected 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 across time.⁵ Similar claims about enduring behavior and attitudes underlie other econometric studies, and there are theoretical models that could support culture's persisting in this way.⁶ 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 to concentration camps; 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. PP's authors stress the t-ratio associated with their pogrom indicator, POG1349. If persistent culture causes anti-Semitic behavior, POG1349 should have a positive and statistically significant coefficient in all six regressions.

Our Table 1 reconsiders the results for one of those outcomes: votes for the Nazi Party in the May 1928 federal election. Column (1) replicates the regression reported in PP (Table VI). POG1349 had a significant positive coefficient for the 1928 election, but a partial regression plot that includes the outliers from Column (1) (Figure 1) suggests that the result is driven by these outliers, many of which are in Bavaria, Germany's second largest federal state.⁷ We come back to the Bavarian problem below. Column (2) re-estimates Column (1) as a quantile (median) regression, a consistent check for outliers. Nothing in

⁵ 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 a brief overview of the debate, see Deák (1997) and Herbert (1999).

⁶ For basic models of cultural persistence, see Bisin and Verdier (2001), Richerson and Boyd (2008), and the other works cited in PP. For a persuasive application of these models in economic history, see Mokyr (2016).

⁷ In the regression reported in Column (1) of Table 1, there are 16 observations for which the studentized residual has an absolute value greater than 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.

the PP argument implies that the right way of estimating the coefficient is a conditional mean instead of a conditional median, and in this case, POG1349 has little effect on the conditional median. This difference shows that the PP result was driven by outliers, to which OLS is sensitive. In Columns (3) - (5) we re-estimate the model excluding outliers defined by three different criteria. Column (3) excludes observations in the top 1 percent of the absolute value of residuals; Column (4) excludes the top 2 percent; and Column (5) removes those with studentized residuals greater than 2.0. Dropping as few as six observations (out of 325) as in Column (4) yields an estimate for POG1349 that is halved and no longer statistically significant. Column (6) drops all observations from Bavaria. Without the Bavarian observations, even the OLS regression with the remaining outliers fails to support the idea that areas with a medieval pogrom were more likely to vote for the Nazis in 1928.

There is, in short, no relationship between POG1349 and the 1928 vote outside of Bavaria. Adding a full set of fixed effects for the German states and their interactions with POG1349 leads to the same conclusion, as we show in Appendix A.5. Bavaria is the only federal state with a significant relationship between the medieval pogrom and the 1928 vote for the Nazis. One might argue that our POG1349 point estimates are not significantly different from the estimate in PP. This is not, however, the issue. Rather, we want to know whether the true effect of cultural antisemitism is different from zero.⁸ While our point estimates are positive, only one is significantly different from zero (column 3), and their magnitudes are highly sensitive to the exclusion of the outliers.

Placebo tests raise a different and more general question about the pogrom variable. Table 2 uses specifications analogous to PP's voting models (Table VI, columns (2) and (3)) to ask whether POG1349

⁸ If we ignore controls and measure variables as deviations from their means, then PP measure the effect of anti-Semitism on the 1928 vote y by estimating β in $y = \beta * POG1349 + f$ where f is the error term. The PP estimate of β (call it β_1) may not be significantly different from our estimate β_2 , a point raised by Voigtländer and Voth, who cite Gelman and Stern (2005). But the issue is whether the effect of cultural anti-Semitism is significantly different from zero. The answer is no, as we show below, is no. That β_1 is sensitive to outliers pointed us in this direction.

influences the vote for parties that did not have an anti-Semitic profile. The text table focuses on 1924 votes for two liberal parties, the DDP and the DVP, which formed core parts of the Weimar coalition. The DDP vote constitutes an especially useful placebo. Jewish Germans overwhelmingly supported this party, which had among its leaders a large number of (usually non-observant) Jews. Liepach (1969, pp. 119-120) notes considerable overlap between the leadership of the DDP and that of the major German Jewish organization of the time. Right-wing elements smeared the DDP as the *Judenpartei* (Frye 1976, pp. 43-44; Niewk 1980, p.28). Yet the pogrom indicator, which supposedly proxies for a long history of anti-Semitic views, shows that a history of medieval violence raised the DDP's vote share. The DDP results for Germany as a whole cast serious doubt on the pogrom proxy's interpretation. The DVP results for Prussia and for Germany-less-Bavaria suffer from a similar problem, particularly when we combine the vote shares of the two liberal parties. Thus POG1349 fails a basic placebo tests: using PP's specifications, the proxy for medieval anti-Semitism is significantly correlated with conduct that has nothing to do with anti-Semitism. Appendix A.6 reports similar results for all Weimar coalition parties, as well as the extremist parties, in 1924, 1928, and 1933.

The Appendix (Sections A.1 – A.5) extends this reconsideration to each of the five other outcome variables reported in PP Table VI, as well as to the first principal component of all six outcome variables. The PCA is supposed to capture “a broader, underlying pattern of attitudes” (PP, p. 1370). The results are similar. The regressions PP reports are consistent with the authors' hypothesis, but 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. Bavaria is a problem in many specifications, including those that add state fixed effects and their interaction with POG1349, the medieval pogrom. The pogrom proxy tends to be significant in only a subset of German states, usually Bavaria (Appendix Section A.5).

The coding of the pogrom indicator reflects a deeper problem. The underlying source for this variable consists of two compendia summarizing the history of Jewish communities across Germany. PP

quotes the example of Heiligenstadt, calling it a “typical entry.” As appendix C.1 shows, the entry for Heiligenstadt is in fact highly atypical; in most cases the historical sources are far less certain of events than PP implies. There are other examples where PP does not follow its own rules for deciding between conflicting sources.

More generally, PP assumes that cross-sectional variation in POG1349 reflects degrees of anti-Semitism in 1349. But this proxy also reflects the actions of political and religious authorities who could encourage or stop anti-Semitic behavior in the Middle Ages. The same is true for the measures of anti-Semitic behavior in the 1920s and 1930s that are PP’s outcome variables: they too are shaped by unobserved actions of political and religious authorities. The importance of these omitted variables is clear in the only regression in PP Table VI that survives econometric scrutiny: the *Reichkristallnacht* attacks. The government organized the *Kristallnacht* attacks, and, as we discuss in Appendix A.1, historians argue that the attacks did not reflect local initiative or attitudes.

Understanding the Outliers: The Role of Political and Religious Authorities

To grasp why outliers drive the regression results (particularly in Bavaria), 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 issues involved: the 1349 pogrom in Strasbourg, which is mentioned in PP (p. 1347) but is not in the dataset.⁹ 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.

⁹ Strasbourg was not in the dataset because (as we explain below) by the 1920s it was no longer part of Germany.

Strasbourg thus seems a clear illustration of the anti-Semitic attitudes at issue in PP, and it is used as an example in S. K. Cohn's (2007) historical analysis of the European-wide pogroms.¹⁰ The story, though, is not just bigotry: persecution always required the cooperation of political and religious authorities. T. Finley and M. Koyama (2018) show that pogroms during the Black Death were in general 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.¹¹ 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. Anti-Semitism may have been a constant in Strasbourg, but authorities could encourage anti-Semitic attacks or (at least sometimes) stop them, even in the Middle Ages.¹²

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

¹⁰ Our sources for Strasbourg include Mentgen (1995); Ephraim (1923, 1924); Ginsburger (1908); and the documents published in Witte and Wolfram (1896).

¹¹ 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).

¹² Another example of Jews being successfully defended against attack in 1349 comes from the German city of Regensburg. There the defenders were citizens of the city who owned rights to tax the local Jews: Kirmeier (2014).

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).

In short: Strasbourg's political and religious authorities organized a pogrom in 1349; worked to stop one eleven years earlier; and then blocked anti-Semitic outrages in the 1920s and 1930s. These agents play no role in PP's discussion. This omission ends up undermining the empirical evidence in PP and helps to account for the outliers in the regressions for the 1928 elections, at least in Bavaria.

The Bavarian difference derived both from Bavaria's medieval experience and from its role as the home of the Nazi Party. Bavaria was different in the fourteenth century because after the death of the Duke of Bavaria (and Holy Roman Emperor) Louis IV in 1347, political authority in Bavaria was divided among his sons, who found their power and revenue even more limited than it had been under their father (Holzapfel 2013; Uhlhorn and Schlesinger 1970, pp. 186-88; Immler 2016). As in Strasbourg, divided authority made it difficult to protect local Jews. In Bavaria, the sons in power not only allowed the pogroms but actually organized them, with a clear goal of financial gain (Kirmeier 2012, 2014).

Bavaria was different in the twentieth century because Hitler started there. His initial followers resided in Bavaria. When the government lifted a ban on the Nazis in 1925, the party held its first rally in Bavaria. Although the party would soon gain more support in other parts of Germany, in 1928 the Nazis benefitted from having well-organized district offices already at work in Bavaria. 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.¹³ 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

¹³ Hoser (2007); Ziegler (2019b). In the PP replication data, for the election of May 1928, the Nazi Party had a 6.1 percent vote in Bavaria versus 2.5 percent in the rest of Germany.

vote share in 1928.¹⁴ 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 our Table 1, Column 6): the Black Death pogroms struck throughout Germany. An alternative possibility that better fits the data is the historical coincidence of Bavaria's having both fractured political authority after 1347 and an effective 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 α 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 β 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:

¹⁴ 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.

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

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 β , the estimate for POG1349, will simply be an attenuated estimate of the true coefficient α , a standard result for measurement error in a regressor in a linear model.¹⁵ 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, which would give the Nazis a higher percentage vote. This historical coincidence created the Bavarian outliers that biased the coefficient for POG1349. 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

¹⁵ Textbook treatments include Greene (2018, pp.102-3 and pp. 281-88) as well as Wooldridge (2010, pp.78-82).

would seize Jewish assets. The expected financial gains would depend both on any unobserved economic variables that would enter into u and 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 or Strasbourg, stopping the pogrom would be less likely. Since 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.

Adding Controls and the Role of Political and Religious Authorities

With the right controls, the covariances we discussed earlier could be driven closer to zero: what appears as part of the error term in PP's regressions would be partialled out. Such controls would include variables that pin down the political and religious situation. Religion is relevant not just in 1348-50 but also in the 1920s and 1930s. J. L. Spenkuch and P. Tillmann (2018) show that, until the Vatican reached an accommodation with the Nazis in 1933, many Catholic bishops successfully pressured their faithful not to cast Nazi ballots and to vote instead for the centrist Catholic *Zentrum* Party and its Bavarian sister party, the *Bayerische Volkspartei* (BVP). The chief exception was in villages where Catholics were influenced by priests who resisted the bishops and "openly sympathized with the Nazis" (Spenkuch and Tillmann 2018, pp. 20-21, 28-29; Spicer 2008). Thus, the influence of local Catholic leaders is especially important in the 1924 and 1928 elections.

PP incorporates some of the necessary controls: population, the variation in major religious affiliations in 1925-1933, and, in some regressions, an occupational breakdown (PP 2012, Table A.5) or characteristics of cities in the Middle Ages (PP 2012, Tables VIII, A19). But the medieval characteristics PP incorporates do not account for fragmented political authority in 1348-50, as in Bavaria. They therefore cannot capture whether local medieval authorities had an incentive to protect the Jews. And the

religious affiliations in 1925-1933 lump together Catholics with the non-religious in a residual category, so they cannot take into account whether the non-religious (a bigger fraction of the population than the Jews) affected anti-Semitic behavior: for example, by diluting the bishops' anti-Nazi message in Catholic areas.¹⁶ Nor do the PP regressions account for the open Nazi sympathizers among local Catholic priests, who could counter the bishops' directives against supporting the Nazi party.

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 also 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, sometimes only a few miles away (Figure 2).¹⁷ Such a sharp local difference seems incompatible with the idea of local culture, which would presumably diffuse over neighboring communities as people sought marriage partners, went to market, or sought work as servants. 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 1348-50.¹⁸ 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 restraining (or not) anti-Semitic conduct in the

¹⁶ Spenkuch and Tillmann (2018, Online Appendix Table A.1) estimate that 3.64 percent of the population was nonreligious in 1925, versus 0.97 percent that was Jewish.

¹⁷ In an extreme example, PP codes Mönchengladbach as experiencing a pogrom and Odenkirchen as escaping this violence. The two places are 7.4 km apart. Although culture does vary over space, the examples Voth and Voigtländer cite in reply to our argument here either involve longer distances (languages, ethnic groups, use of the plough), migration (which would work against the argument in PP), or the sort of political authorities missing in PP (medieval Italian cities). And while low in-migration might preserve a town's attitudes, it is hard to see how they would survive higher rates of migration in the nineteenth or early twentieth century.

¹⁸ Among the many examples after the Black Death pogroms, Jews returned to Gießen in 1375, Guben in 1354, Halle in 1368, and Hamm in 1370. The town id numbers are 479, 532, 546, and 559; Avneri's entries are in Volume I (1968, pp. 278, 307, 320, 354).

1920s and 1930s. Catholic cities witnessed fewer of these later pogroms before 1933, even controlling for having a history of a medieval attack on Jews. Once the Catholic Church reached an accommodation with the Nazis in 1933, however, Catholic cities experienced more attacks on Jews. This rapid change is hard to reconcile with a predominant role for deeply rooted cultural anti-Semitism.

Although persistent culture may still matter, it counts for much less than the PP regressions suggest. In particular, the outliers in Bavaria are not themselves evidence for the persistence of culture. Rather, they are simply the result of historical coincidence that correlates those omitted variables across time.¹⁹

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 voluntary associations first created in nineteenth-century Germany. The idea of social capital spread in the social sciences thanks in large part to the work of Robert Putnam (R. Leonardi, R. Y. Nanetti, R. Y. and R. Putnam 1992; Putnam 2000). The concept has proven difficult to define, prompting questions about whether it is precise, useful, or even novel (Guinnane 2005). Some have worked to pin down its meaning via the sociology and economic theory of networks (Jackson et al. 2021; Jackson 2019). The main empirical measure of social capital used by Putnam and others (including BF) is the density of “civil society” organizations. BF addresses an older literature to argue that during the Weimar Republic, the Nazi Party drew on social

¹⁹ On this point, it should be stressed that medieval anti-Semitism in Bavaria was not the chief reason Hitler got his start there. His Bavarian success had far more to do with political events that struck Bavaria in particular: a revolution in 1918 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 coup in 1920, which until 1924 made Bavaria a haven for right wing extremists fleeing after a failed right-wing coup in Berlin (Gelberg 2007; Ziegler 2019b).

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.²⁰ The authors' regressions test whether social capital measured in this way explains the percent of the population who joined the NSDAP from each city in this period. The regressions also control for city size and religious composition, as in PP, as well as the percentage of the work 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 they consider 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 they label as "stable" had about 15 percent of the total German population and about 20 percent of the sample. BF's authors appear not to appreciate this remarkable finding; their results, taken at face value, imply that social capital only affected Nazi recruiting in one-third of Germany. Put differently, this is another case of results driven by outliers: 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.²¹

BF devises a stability index as the first principal component of three variables, all defined 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

²⁰ BF counts what in German is called a *Verein*. The word can mean "association," "society," or "club." We use these terms interchangeably.

²¹ 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. In the Appendix Section B.1, we show that BF's main results (as presented in their Table 3) reflect the influence of outliers.. Dropping Bavaria does not change the sign of the main result, but the standardized regression coefficient for the social-capital variable declines by 40 percent.

(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).²²

This index suffers from conceptual and empirical errors. First, 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. Second, the party that headed Bavaria’s government for much of the Weimar period (the BVP) had agreements with a Weimar coalition party (the *Zentrum*) that meant the *Zentrum* did not stand for office in Bavaria. In BF’s scoring, this means no Weimar coalition was possible in Bavaria and for the third element in the stability index Bavaria thus receives a zero by definition.²³ Third, 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)).” Creating the indicator variable throws away information, yet they do not defend this procedure. Their description of the binary indicator is also inaccurate. The replication code shows the authors 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, affect the results.

Our Table 3 re-estimates the regression models reported in BF’s Table 7.²⁴ Column (1) replicates BF’s column (3) for states that are “unstable” by their definition and index. This is the result they stress: in unstable states, more social capital means more Nazis; in stable states there is no relationship. Our Column (2) estimates the model in Column (1) as a median regression. The point estimate for the clubs

²² 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.”

²³ 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.

²⁴ Appendix Table B.3.1 allocates the states by alternative definitions of the stability indicator.

variable is not significant. Column (3) drops the “Weimar party” element from the index but retains BF’s binary definition of stability. Column (4) uses BF’s version of the index, but defines the binary indicator so that median values are now included among the “stable” states rather than among the unstable ones. This change affects Bavaria alone, which has the median value of the stability index. The estimate for the social-capital variable is not significant in either of the regressions that correct problems in the BF stability index. Their results hinge on that third element, which is unexplained and inappropriate, and on assigning the median values to the “stable” group. The problem is not just inference: the point estimates and standardized regression coefficient (beta) change considerably with these corrections to the stability definition.

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. Bavaria consistently acts as an outlier that drives the results. BF’s Table 7 also includes specifications that use the entire sample and interact everything with the BF stability indicator. Those regressions are equally sensitive to the problems in the stability index. Finally, we pose an obvious question: what is the point of taking a continuous index and turning it into a binary indicator? As we show in the Appendix, using the stability index itself as a continuous regressor undermines the idea that political stability affected the role of social capital. The entire relationship BF stresses depends on turning the index into a binary indicator.²⁵

The Data in BF

²⁵ BF Appendix A7 reports an exercise using the stability index as a continuous variable. Some of the calculations underlying that exercise are incorrect. See our Appendix B.3.

BF's authors construct their measures of Nazi Party joiners from a public-use sample created by earlier researchers.²⁶ The social-capital proxy, however, they created themselves by counting the number of associations listed in the directories published for most German cities in this period. The social-capital 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?

BF's appendix provides a list of cities in their sample but they do not state precisely which year's edition they used, so we cannot examine the actual directories that underlie their data.²⁷ Our Figure 3 reproduces part of the relevant section from a directory for Worms (one of BF's cities) from 1925.²⁸ The directory divides the associations into functional categories; our figure shows the last page of the group that includes charities and cooperatives (*Gemeinnützige Vereine und Genossenschaften*) and the first page of choral and music societies (*Gesang- u. Musikvereine*).

The Appendix (Section C.3) discusses possible bias in the selection of cities drives BF's results. To construct their sample, 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 note in Appendix C.3, German libraries hold directories from other cities, so these volumes are available in the current Federal Republic.) BF's authors then contacted "libraries and archives" in remaining cities, asking for information on the number of clubs in each city in the late 1920s (BF 2017, pp. 490-91). Their final sample includes only 197 places from this 547. To assess the possibility of selection bias, BF Table 1 does compare vote

²⁶ Brustein (1996). BF does not mention that many joiners soon quit; about 40 percent of the 1.4 million people who joined the Nazis prior to 1933 had quit by that date (Brustein 1996, pp. 10-15). If clubs attracted new Nazi Party members who had no serious attachment to the Party, then their joining would add little to Party's strength as a political force.

²⁷"We use any surviving directory from the 1920s; where several are available, we take the directory nearest in time to 1925" (BF, p. 491).

²⁸ *Adreßbuch Stadt und Kreis Worms 1925*. Worms: Buchdruckerei Eugen Kranzbühler.

shares and socioeconomic statistics for their sample cities and Germany as a whole, but as Appendix C.3 makes clear, this comparison of observables is not enough. It is easy to imagine city characteristics that would be correlated with the probability of producing a directory in the 1920s or that would affect the survival of such directories to the present. Those characteristics could in turn be correlated with local political conditions and with social capital, both today and in the 1920s. Appendix C.3 discuss strategies for additional historical research that would address these problems.

In addition to the selection problem at the level of cities, there is a selection problem in the clubs the directories report. To be a useful measure of social capital, the directories have to either include all relevant clubs, or present unbiased samples of such clubs. The historiography suggests otherwise. According to a study of Tübingen in the late 1920s, the city's directory "normally covered nearly two-thirds of all local voluntary associations." (Koshar 1982, p. 32) The implicit process that selected clubs for listing could involve a host of issues correlated with the error term in BF's regressions, meaning that the measurement error is not classical.

Directories, for instance, 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.²⁹ 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 total in the BF data.³⁰

²⁹ *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.

³⁰The 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.

We also doubt BF's use of the history of associations to defend two important assertions. For their study, it is important that the clubs they count are not ideologically akin to the Nazis; in that case, joining the Nazi Party would reflect a political orientation rather than social capital. In addition, the distribution of associations across cities (measurement issues aside) has to be exogenous. To support both assumptions, BF's authors rely upon a tendentious reading of the history of German associations. In their view, their data for the 1920s reflects a persistent "culture of associational life" created back 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). In other words, associations had nothing to do with the nationalism that drew people into the Nazi Party, and the density of the associations came into being far enough in the past to make it uncorrelated with the error term in the BF regressions.

BF pushes both arguments too far. Nineteenth-century nationalism might have been less xenophobic than the Nazis, but 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.³¹ 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 invited nationalist speakers. Even choral societies had an ideological bent.

A more specific worry concerns direct Nazi participation in some of the clubs that BF use to proxy for social capital. According to Rudy Koshar (1987, p. 20), "After Hitler's 1923 coup attempt failed, the [Nazi] party dissolved into sports clubs, sharpshooting associations, and hiking organizations." Koshar's claim implies something closer to the reverse of what BF argues. The Nazis later regained the

³¹ 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.

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.

What about BF's argument that the culture of associational life was set in the middle of the nineteenth century and was therefore exogenous in the 1920s? BF (pp. 481-84) emphasizes 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. Thereafter BF points to the 1848 national Parliament's proposal for a constitution that guaranteed freedom of association and maintains (BF, p. 484) that after 1848, "earlier prohibitions never returned with full force." If in fact German governments had respected freedom of association after 1848, then the distribution of clubs in the 1920s would have still been shaped by events and clearly not exogenous. The assertion also ignores a continued history of repression, one that distorted the location and types of associations Germans could form.³² The number of clubs grew dramatically from 1848 to 1918, and featured organizations for working-class people in particular. The growing number and changing types of associations undermines BF's assumption (p. 484) that "the state- and city-level factors driving variation in the repeal of restrictions are plausibly exogenous to NSDAP entry in the 1920s and 1930s." The historical evidence contradicts this assumption.

To support the exogeneity claim BF's authors do report a regression that in their view demonstrates the deep historical roots of associational density in the 1920s. For 39 of the 229 cities in their data, they know how many delegates local associations sent to the 1848 Democratic Congress in Berlin (BF 2017, Appendix F). For those 39 cities, 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

³² See Brooks and Guinnane (2017). Freedom of association did not have legal guarantees until 1908. Some of the great political battles of the later nineteenth century involved suppression of associations.

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.

We have serious reservations about these statistical results. First, they pertain to only part of the BF sample of 229 cities. Second, associations extant in 1848 (so the history demonstrates) had a different social, confessional, and political basis than those in the 1920s. The former survived restrictions imposed by authoritarian governments. BF exacerbates this problem by dropping all religious clubs. Catholic associational life especially 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, "The number of local voluntary associations grew throughout the 1920s, reaching extremely high levels as measured by both historical and comparative standards." (Berman 1997, p. 413). Discussing the Weimar period, Koshar (1982, p. 33) notes "the appearance of soccer clubs and shooting clubs with predominantly working-class memberships promoted and reflected deepening class divisions." Tenfelde (2000, pp. 95-96) also stresses the growth and diversity of clubs devoted to working-class memberships, especially during the Weimar Republic. By then clubs were drawing members from very different social and religious groups and with what were likely be different political leanings as well. It would be very difficult to believe that all these developments are exogenous to NDSAP recruitment in the 1920s and 1930s.

Associations and the Roman Catholic Church

BF's authors decided to exclude two sets of clubs, the "political" and "religious." They do not report sensitivity tests to assess whether this decision affects their results. There are two drawbacks to this decision. First, it may not be possible to identify some such groups from the information in city directories. In Figure 3, for instance, the first page lists a school association whose leader is a minister

(*Pfarrer*).³³ The organization probably had some type of affiliation with a church. (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. Thus the procedure introduces measurement error. Second, the wider logic justifying these omissions is unclear. One might want to understand the difference between social capital in a political organization and social capital in a choral society. This question warrants an empirical test, not dropping a large number of clubs from the sample.³⁴

BF exclude Catholic 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). The Catholic Church was certainly hierarchical, but its history in the nineteenth and early twentieth century (and before) is full of local organizations created by the local laity, from traditional confraternities to charitable organizations, women’s associations, and groups linked to professions. More important, it is unclear why “bottom up” associations in general would involve less social capital or have less of an impact on Nazi party recruiting. Many of the other associations in underlying their proxy were in effect the local branch of a national organization. And even an association that is little more than a branch of some larger entity could create relationships among members and help recruit Nazis.

Because of the German Catholic Church’s overt and well documented hostility to the Nazis, the omission of Catholic associations could seriously bias BF’s estimates. Members of the numerous Catholic clubs would be told not to have anything to do with the NSDAP. 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) stresses that many Catholic associations strongly

³³ The association is the *Kinderschulverein*, seventh from last on the directory’s page 493.

³⁴ 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.

discouraged members from joining the Nazis. He shows that in Bavaria, areas with strong local Catholic bodies had fewer Nazi members.³⁵

BF's regressions all include a control for the percentage of the city's population that was Catholic. But unless the city's religious composition perfectly predicts the number of missing Catholic associations, excluding the Catholic clubs from the social capital count could easily bias the results in favor of BF's conclusions. The BF social capital proxy could simply count associations whose members were more likely than average to be favorable to the Nazi Party. To the extent this is true, BF's results reflect the authors' definition of the social-capital proxy alone. In theory, one could test whether excluding Catholic and political associations mattered for 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.³⁶

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;

³⁵ See in particular Zofka, pp. 168-69. 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).

³⁶ While the omission of Catholic organizations is most glaring, given what we know about the history, the omission of other religious organizations, as well as political groups, is also unfortunate.

Banerjee 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.

That could happen if the political leanings of associations' members varied from group to 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.³⁷ 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 would have nothing to

³⁷ 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.

do with 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. This interpretation would survive BF's omission of political and religious organizations. 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. To see why, suppose that there are a total N_t of these clubs in the town, and for the sake of simplicity, assume that they are all of the same size. Note that $N_t = N * s$, the town's club density N multiplied by the town's population s . Assume that if the recruiter employs the first method (taking advantage of social capital) and knows who the association's gossips are, then he will enroll k new Nazi Party members from the association. If he does not know the identity of the gossips, he will enroll no one. To avoid wasting his efforts, he will therefore recruit only from clubs where he can identify the gossips (ones where he is a member). If he belongs to a fraction p of the city's associations, he can expect to enroll $kpNs$ recruits. Expected per capita recruits will be kpN . The expected number would be the same if the Nazi Party had several recruiters in the town, only p would now be the fraction of the town's associations with at least one recruiter as a member. If we allow for other correlates z of party membership and presume a linear relationship, then the equation linking per-capita Nazi Party recruiting y and the association density N would be: $y = \alpha kpN + \beta z + e$, where z are other correlates of party membership, e is the error term, and α and β are coefficients.

What if the recruiter relies on the second method and exploits not social capital but information about the associations' members? Assume that he recruits k' new members if the club's associates are

politically receptive, but no new members if the club's associates lean in the other direction. To minimize effort, he will canvas only in clubs that are receptive. He enrolls $k'p'Ns$ party members, where p' is now the probability that a club's members are politically receptive. We will end up with a similar linear relationship between per-capita Nazi recruiting y and the association density N : $y = \alpha'k'p'N + \beta'z + e'$, where α' and β' are the new coefficients and e' is the new error term. Here there is no social capital and no identifying the gossips. The Nazi recruiter just has to know the likely political opinions of each club's members. If the Nazi party has some recruiters who use the first method and some who rely on the second, then

$$y = (\alpha'p'k' + apk)N + (\beta' + \beta)z + e' + e \quad (3)$$

An estimate of Equation (3) would only tell us whether $\alpha'p'k' + apk > 0$. It might be the case that social capital does boost Nazi Party membership ($apk > 0$), as BF claims, but it could also be the case that social capital does nothing ($apk = 0$) and the regressions reflect the information about groups ($\alpha'p'k' > 0$).

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. Places that had more associations had more Nazis, and that 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).³⁸

The Nazi Party succeeded by crafting nationalistic economic 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 they themselves belonged to? Or did they recruit from groups they themselves had not joined?

Conclusion

PP and BF muster evidence which they argue shows that history played an important role in the extreme anti-Semitism that underlay so much of Weimar political life, including the rise of the Nazi Party. We have established, however, that much of this evidence does not stand up to scrutiny. Our discussion of the flaws in PP and BF does not rule out a role for social capital or a longstanding culture of anti-Semitism. These factors may well help explain the rise of the Nazi Party in 1920s and 1930s and be important for questions in other times and places. The evidence that PP and BF offer, however, does not demonstrate this was the case in Weimar Germany.

One might argue that we are asking for too much robustness. Our demands (so the argument might go) might raise the evidentiary bar so high that we would mistakenly reject the evidence for the true role played by medieval anti-Semitism and Weimar social capital in the rise of the Nazi Party. Such an

³⁸ The clubs proxy might also be picking up preference heterogeneity across locations. In locations with more heterogeneity, one would expect fringe parties to do better, and a larger share of voters to have extreme views. At the same time, there would demand for a larger number of social clubs, each catering to different member preferences.

argument, however, would not be persuasive. The robustness checks we apply to the important conclusions in PP and BF are simple steps one would expect in any empirical research. Moreover, the weaknesses in the two papers are not just a matter of statistical inference. Neither PP nor BF consider placebo checks that would verify their interpretation of the proxies in question. Similarly, BF's stability results reflect conceptual flaws as well as inappropriate econometric decisions. And a more powerful econometric test in BF (for instance, a regression with a larger sample) could never tell us whether the Nazi Party relied on social capital or simply on information about social groups when enrolling new members because the regression coefficient measures the sum of the effect of the two methods of recruitment. Only more historical research can solve that identification problem, just as only historical research can explain why Bavaria causes trouble in both papers.

We have stressed the role of political actors in both the medieval pogroms and the later Weimar political developments, actors who ranged from medieval political authorities to Weimar-era Catholic bishops to the Nazis who recruited others into the Party. These actors played a crucial role in the outcomes we observe. Ignoring them makes it impossible to understand the role of anti-Semitism and social capital in the Nazi takeover in 1920s and 1930s Germany. As generations of historians have stressed, medieval pogroms erupted in villages and towns throughout Western Europe. Anti-Semitism was widespread in Europe before World War II, as was complicity with occupying Nazi forces. But there was only one Third Reich.

Weaknesses of this sort are, unfortunately, not unique to PP and BF. They afflict other examples of the persistence literature as well. Analyzing these specific persistence studies, however, does yield some general warnings for avoiding such pitfalls in the future. We see three types of concerns.

First, treatments have to be exogenous. PP assumes that the medieval pogrom can be treated as exogenous because it happened long ago. That misses the correlation arising from the coincidence of two unrelated things in the federal state of Bavaria. BF claims that the distribution of civil-society

organizations across German cities was fixed in the middle of the nineteenth century, but does not marshal convincing evidence for that claim.

Second, not having at least a verbal model opens the door to all sorts of trouble. A simple model of relevant actors would have made the authors of PP consider the political and religious authorities who could encourage or discourage anti-Semitic behavior in the fourteenth century and the 1920s and 1930s. Omitting them (so we show) created the spurious correlations across time. Similarly, BF does not say explicitly how Nazi recruiters used associations to enroll new party members. As our simple model showed, even if there is a correlation between Nazi party membership and associational density, it could be caused by a completely different mechanism which does not involve social capital.

Third, econometric exercises must reflect both an interest in letting the data speak and careful handling of the underlying data. Despite robustness checks in both papers, important empirical results are sensitive to small and reasonable changes in definitions or use of controls, and the conclusions vary considerably with modest alternatives. Both papers rely on proxies but neither one considers placebo checks to verify the proxies. As for the data, PP's coding of the medieval information does not respect the many expressions of doubt in the sources. BF does not list specific sources for its main proxy, and its results reflect a decision to drop many of the organizations that would naturally be part of the proxy. BF does not mention any tests for the implications of that core decision.

The most important task for persistence studies, however, is getting the history right. Failing to do that can undermine the econometrics, the data, and the modeling. We have noted several instances in which PP and BF ignore historian's conclusions or over-simplify a debate in the interest of presenting a striking result. "History" is not a series of details; it creates the building blocks for causal connections, the context for the treatment variables, and the world in which the actors at stake actually lived. We cannot understand whether history affected later events if we do not have the history right.

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TABLE 1
VOTING FOR THE NAZIS IN THE 1928 ELECTION

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	1	2	3	4	5	6
pog1349	0.0142** (0.00567)	0.00294 (0.00279)	0.0102** (0.00514)	0.00655 (0.00463)	0.00447 (0.00405)	0.00685 (0.00435)
log_pop	-0.00254 (0.00219)	0.00121 (0.00100)	-0.000660 (0.00194)	0.000969 (0.00154)	0.000766 (0.00143)	0.000213 (0.00124)
perc_JEW25	0.00174 (0.00190)	0.000705 (0.00127)	0.00131 (0.00168)	0.00138 (0.00164)	0.00183 (0.00140)	-0.000920 (0.00110)
perc_PROT25	0.000290*** (8.84e-05)	0.000138*** (4.42e-05)	0.000223*** (7.92e-05)	0.000136** (6.27e-05)	0.000138*** (5.07e-05)	0.000230*** (5.31e-05)
Constant	0.0340* (0.0195)	-0.00295 (0.00992)	0.0203 (0.0178)	0.00943 (0.0153)	0.00812 (0.0141)	0.00539 (0.0113)
Observations	325	325	322	319	309	257
Adjusted R-sqr	0.043		0.029	0.013	0.024	0.057
Model	Replication	Replication	1 percent	2 percent	RStud<2	No Bav
Estimator	OLS	QR [median]	OLS	OLS	OLS	OLS
Mean (med) dep var	0.0365	0.0174	0.0340	0.0319	0.0279	0.0229

Note: The dependent variable is the percentage vote for the Nazi Party in 1928. Column (1) replicates PP Table VI, Column (2). Column (2) estimates a median regression model. Column (3) drops the observations corresponding to the largest 1 percent of the absolute value of the residuals from Column (1). Column (4) drops two percent of the sample defined analogously. Column (5) omits observations that in Column (1) have an absolute “studentized” residual of more than 2.0. Column (6) drops observations from Bavaria. Appendix Sections A.1-A.3 report similar analysis of the other outcome variables reported in PP Table VI.

Table 2: The liberal parties as placebos

	Dependent variable	Program Estimate	SE	Obs	Adj R-sq	Sample	Model
1924 election							
1	DDP24	0.0109**	(0.00544)	325	0.265	Replication	OLS
2	DDP24	0.00915	(0.00750)	139	0.266	Prussia	OLS
3	DDP24	0.0151**	(0.00675)	257	0.249	-Bavaria	OLS
4	DDP24	0.00682	(0.00523)	325		Replication	QR
5	DVP24	0.00955	(0.00799)	325	0.233	Replication	OLS
6	DVP24	0.0233**	(0.0102)	139	0.276	Prussia	OLS
7	DVP24	0.0171**	(0.00860)	257	0.296	-Bavaria	OLS
8	DVP24	0.0167	(0.0109)	325		Replication	QR
9	DDP_DVP24	0.0205*	(0.0110)	325	0.306	Replication	OLS
10	DDP_DVP24	0.0324**	(0.0155)	139	0.324	Prussia	OLS
11	DDP_DVP24	0.0322**	(0.0126)	257	0.374	-Bavaria	OLS
12	DDP_DVP24	0.0294**	(0.0116)	325		Replication	QR
1928 election							
13	DDP28	0.00301	(0.00490)	325	0.291	Replication	OLS
14	DDP28	-0.00111	(0.00572)	139	0.387	Prussia	OLS
15	DDP28	0.00726	(0.00599)	257	0.258	-Bavaria	OLS
16	DDP28	0.00528	(0.00435)	325		Replication	QR
17	DVP28	0.0128	(0.00774)	325	0.290	Replication	OLS
18	DVP28	0.0191*	(0.0112)	139	0.323	Prussia	OLS
19	DVP28	0.0158*	(0.00832)	257	0.368	-Bavaria	OLS
20	DVP28	0.0193**	(0.00748)	325		Replication	QR
21	DDP_DVP28	0.0158	(0.0102)	325	0.381	Replication	OLS
22	DDP_DVP28	0.0180	(0.0158)	139	0.385	Prussia	OLS
23	DDP_DVP28	0.0231*	(0.0119)	257	0.438	-Bavaria	OLS
24	DDP_DVP28	0.0263**	(0.0103)	325		Replication	QR

Note: The table presents placebo checks for models analogous to PP Table VI, models (2) (for 1928) and (3) (for 1924). We report only 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 had been founded on the basis of the Wilhelmine-era National Liberal and Progressive parties. DVP_DDP is the sum of the two party's vote shares. In 1928, both parties had drifted right, and 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*). Thus the DVP in particular is less useful as a placebo in 1928. Appendix Tables A6.2 and A6.3 present results for the other Weimar coalition parties (the SPD and the Zentrum); the POG1349 indicator does not have the same effect there as it does here.

TABLE 3
THE STABILITY INDICATOR AND THE EFFECT OF SOCIAL CAPITAL ON NAZI
RECRUITMENT

VARIABLES	(1) Nazi_entry	(2) Nazi_entry	(3) Nazi_entry	(4) Nazi_entry
Clubs_all_pc	0.349*** (0.128)	0.263 (0.177)	0.0999 (0.147)	0.141 (0.0962)
LnPop25	0.192 (0.134)	0.371* (0.213)	0.0324 (0.125)	0.172* (0.0997)
Cath_pc25	-0.525 (0.388)	0.0644 (0.480)	-0.998** (0.442)	-0.384 (0.377)
BCollar_pc25	-0.272 (1.929)	1.287 (2.306)	-0.553 (1.427)	0.0443 (1.610)
SI_1				
Constant	-2.239 (1.833)	-4.869* (2.761)	-0.0791 (1.683)	-1.627 (1.453)
Observations	58	58	54	72
Adjusted R-squared	0.108		0.055	0.008
Model	Replication	Replication	Replication	Replication
Coded as	Unstable	Unstable	Unstable	Unstable
Estimator	OLS	QR	OLS	OLS
Mean (med) dep var	0.463	0.463	0.00923	0.419
Reg beta	0.440	0.332	0.141	0.224

Note: See BF Table 7. The dependent variable in all regressions is the standardized NSDAP entry per capita, 1925-July 1932. None of the regressions include observations for Prussia. "Reg beta" is the standardized regression coefficient. Column (1) includes only states BF labels as "unstable" and reproduces their column (3). Column (2) estimates the model in Column (1) as a quantile (median) regression. Column (3) drops the third element from the stability index but retains BF's definition of the resulting binary indicator. Column (4) defines the index itself as in BF but includes the median values among the unstable states rather than among the stable ones.

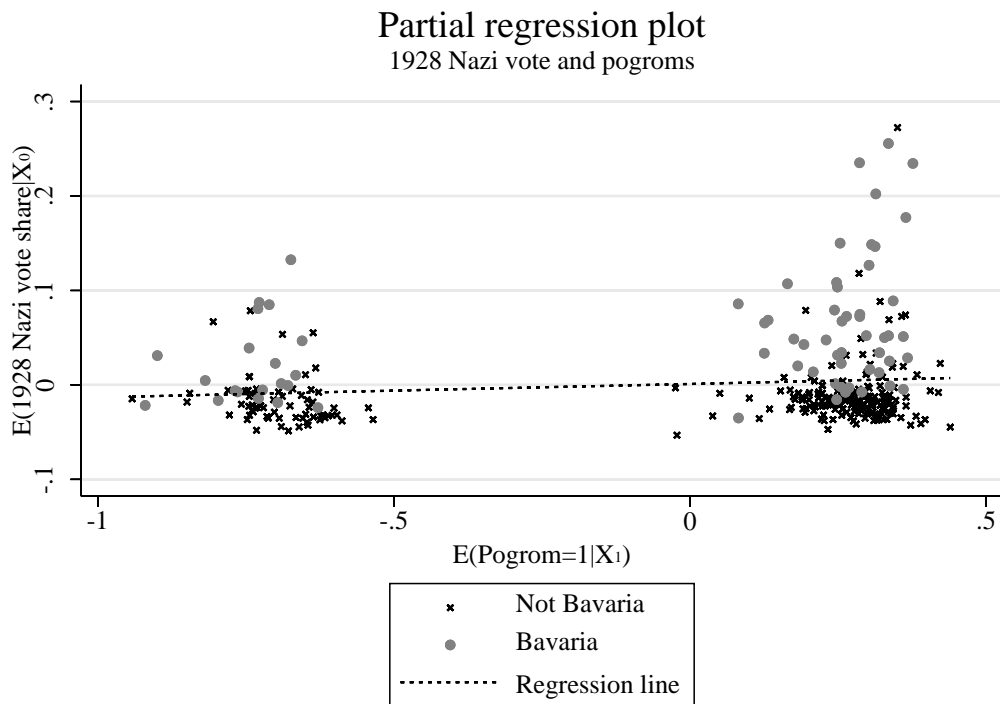
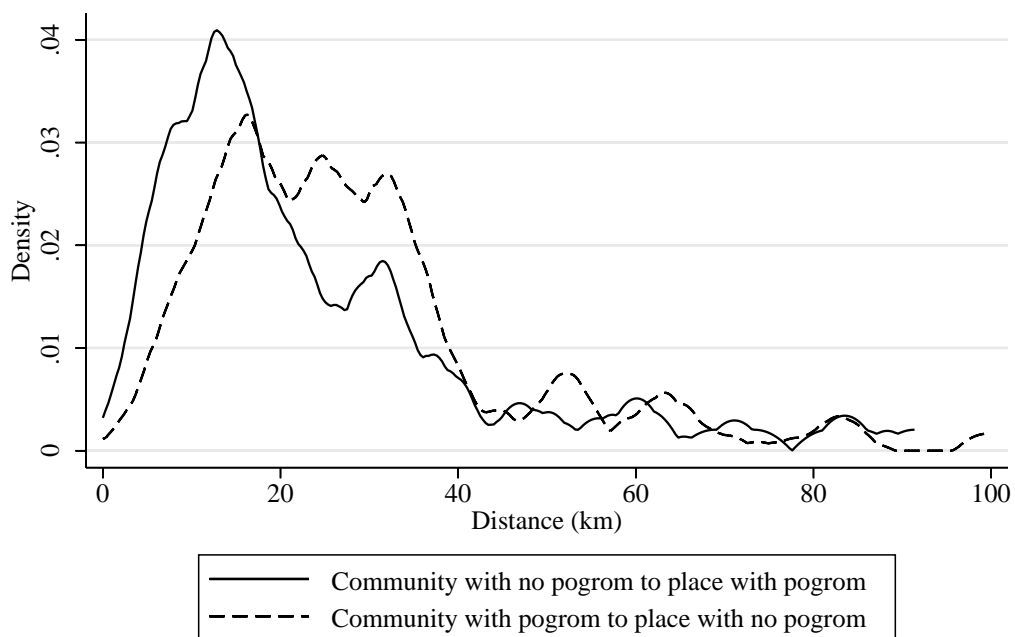


FIGURE 1

Note: The figure shows a partial-regression plot for the 1928 Nazi vote and the POG1349 variable. See Belsey, Kuh, and Welsch (1980, p.30). The x-axis here 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 Nazi vote variable on the independent variables other than POG1349 (X_0). 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 = $.0142 * \text{POG1349}$, standard error = $.00567$)

Distribution of distances between communities



The graph exclude instances where the distance exceeds 100 km

FIGURE 2

Note: The dashed line plots the probability density for the distance between a town with a medieval pogrom and the nearest town without one. The solid line plots the density of the distance from a town with no pogrom to the nearest town with one. Sample limited to the 325 observations for which the pogrom proxy is defined. The two distance distributions are not identical because 72 percent of communities are identified as experiencing a pogrom. Often the nearest community to such a place is another community that experienced a pogrom.

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 Vorsitzender: D. Kohn, Obermarkt 15.
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 Worms: Wilhelm Judith, Petersstr. 15. 2047.
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 *Kinderkassenverein (Frauenverein mit Krankenpflegestation) Worms-Pfiffligheim. Harrer Schell. Vereinshaus: Pfeddersheimer Str. 80. W.B.
 Kreisfeuerwehrverband Worms. Kreisfeuerwehrinspektor: August Hoffmann, Architekt, Alzeer Str. 12.
 Kreisverein des Deutschen Vereins gegen Alkoholismus. Vorsitzender: Sanitätsrat Dr. Bayerthal, Gewerbeschulstr. 25.
 Loge „Bonnegau“ Nr. 1337 des Deutschen Guttemplerordens (J. O. G. L.) Vorsitzender: Sanitätsrat Dr. Briegleb, Arzt, Logensitzung 14tägig abwechselnd bei den Mitgliedern.
 *Mieterschutzverein. E. B. Worms. Vorsitzender: Wilhelm Amadori, Speyerer Str. 28. Geschäftsführer: Heinrich Zinntann, Petersstr. 15. Geschäftsstelle: „Volkshaus“, Petersstr. 15 (Eingang Färbergasse). 788, 804 und 817.
 Ribelungenloge II von Hessen vom deutschen unabhängigen Orden der Odd. Fellows, Ludwigstraße 13.
 Ortsgruppe des Hypothekengläubiger- und Sparererschutzesverbandes für das deutsche Reich. Vorsitzender: Prof. Dr. Wilh. Daudt, Gewerbeschulstr. 1. Schriftführer: Friedrich Schiffer, Gaufr. 88. Kassierer: Wilh. Zaib, Gewerbeschulstr. 8.

FIGURE 3
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 Kessler, Güterhallenstr. 67.
- Verein Licht-Luftbad. Vorsitzender: Heinrich Busch, Luginsland 16, Schriftführer: Sean 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 (Molkereianlage). → 600. 1. Vorsitzender: Freiherr Ludwig von Heul zu Herrnsheim, Majorshof. → 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 Valkenberg. Stellvertretender Vorsitzender: Geh. Kommerzienrat Erik Doerr. Schriftführer und Rechner: A. Bücklein.

2. Gesang- und Musik-Vereine.

(Gruppe 6.)

- Kirchengefangvereine, siehe unter kirchliche und religiöse Vereine
- Arbeiter-Sängerbund (Bezirk XIII Worms). Leitung: Wilhelm Judith, Petersstraße 15. → Nr. 2047.
- Freie Sänger Pfiffliageim. Vorsitzender: Karl Schellenschläger, Schwertstraße 16.
- Gesangverein Alt-Lyra. 1. Vorsitzender: Wilhelm Beisel, Römerstraße 82.
- Gesangverein „Frohinn“, Worms-Pfiffliageim. Georg Klingler, Melanchtonstraße 2 WP. Vereinslokal bei Ph. Siegele, Kreuzstraße 11 WP.
- Gesangverein „Liedertafel“, Worms-Pfiffliageim. 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, Zahnstraße 6. Vereinslokal: Alzeier 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 Heul 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 Heul'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 Niklas, Cornelius-Heul-Straße 13. Vereinslokal: Zum goldenen Weiber, Promenadenstr. 18.
- *Mandolinen-Klub 1913. Vereinslokal: Zum Nordend. 1. Vorsitzender: Franz Hujenbeth, 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 Ridel, Römerstr. 6. Vereinslokal: Silberne Brille, Römerstraße 6.
- *Männergesangverein „Rheingold E. V.“ gegr. 1884. Jakob Lents, Promenadenstraße 16. Vereinslokal: Haus Moltke, Mainzer Str. 1.

Figure 3— continued