

Religion, Division of Labor, and Conflict: Anti-Semitism in Germany over 600 Years[†]

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We study the role of economic incentives in shaping the coexistence of Jews, Catholics, and Protestants, using novel data from Germany for 1,000+ cities. The Catholic usury ban and higher literacy rates gave Jews a specific advantage in the moneylending sector. Following the Protestant Reformation (1517), the Jews lost these advantages in regions that became Protestant. We show (i) a change in the geography of anti-Semitism with persecutions of Jews and anti-Jewish publications becoming more common in Protestant areas relative to Catholic areas; (ii) a more pronounced change in cities where Jews had already established themselves as moneylenders. These findings are consistent with the interpretation that, following the Protestant Reformation, Jews living in Protestant regions were exposed to competition with the Christian majority, especially in moneylending, leading to an increase in anti-Semitism. (JEL D74, J15, N33, N43, N93)

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Anti-Semitism¹ continues to be a widespread societal problem² that is deeply rooted in history.³ Although a large recent literature has provided empirical evidence for the cultural⁴ and political⁵ determinants of anti-Semitism, relatively little has been said within quantitative social sciences about its economic roots.⁶ The aim of this article is to assess how economic incentives have contributed to shaping the geography of anti-Semitism.

The economic underpinnings of ethnic/religious hostility have a long pedigree in disciplines as different as history, sociology, economics, and political science. A large part of this literature has underlined the importance of business and labor rivalries in explaining ethnic conflicts and has focused on the role of labor division as a major determinant of the quality of inter-ethnic relations (see Bonacich 1972, 1973 and Horowitz 1985, p. 113). To the extent that the ethnic division of labor reduces competition among ethnicities in the local labor and product markets, it might also shield societies from internal ethnic tensions. Recently, Jha (2013, 2018) has argued that an ethnic division of labor is sufficient to reduce ethnic tensions when the specific advantage of a certain ethnicity cannot be replicated or expropriated by the others.

Can this theoretical framework explain the emergence and persistence of anti-Semitism? More specifically, can the presence or absence of complementarities in the labor market between the Jewish minority and the majority populations explain the variation in anti-Semitic sentiments and violence over time across regions?

To answer these questions, we document a historical episode in which the division of labor between the Jewish minority and the rest of the population played an important role in shaping the geography of anti-Semitic sentiments. We focus on German history between 1300 and 1900. In the first two centuries, Jews had a specific comparative advantage in the moneylending sector, which had two main reasons. First, the Catholic ban on usury prevented Catholics from lending at interest, while (starting from the Catholic Council of the Lateran in 1215) allowing the Jews

¹In this article, we use the word anti-Semitism as indicating “hostility toward or discrimination against Jews as a religious or racial group” (from the entry “Anti-Semitism” in the *Encyclopedia Britannica*).

²On both sides of the Atlantic, major reports by US and EU bodies confirm that anti-Semitism continues to be a concern (see US Department of State 2005 and European Union Agency for Fundamental Rights 2013).

³Although the term anti-Semitism was coined in the nineteenth century, anti-Jewish sentiments and massacres date back to classical times. According to the Roman historian Suetonius, Jews were expelled from Rome in 19 CE. The Jewish philosopher Philo of Alexandria describes an attack on Jews in Alexandria in 38 CE, in which several thousands of Jews were killed. There is evidence of anti-Jewish writings in Alexandria starting from 270 BCE (Feldman 1996).

⁴Voigtländer and Voth (2015) find large effects of the Nazi indoctrination between 1933 and 1945 on the anti-Semitic beliefs of Germans in 1996 and 2006. Voigtländer and Voth (2012) document an exceptional geographic persistence in patterns of anti-Semitism, showing that German cities that experienced anti-Jewish pogroms in 1348 also showed higher levels of anti-Semitism in the interwar period. Menache (1985) analyzes the importance of the blood libels and the stereotypes of Jews in explaining the expulsion of Jews from England and France in the thirteenth and fourteenth centuries.

⁵Scapegoat theories have long been used to explain outbreaks of violence against the Jews. The theory is that in periods of political and economic distress, politicians find it useful to deflect blame to the Jewish minorities. A large body of empirical literature has documented how anti-Semitism in European history responded to adverse climatic shocks (Anderson et al. 2017 and Grosfeld, Sakalli, and Zhuravskaya forthcoming) and major outbreaks of the Black Death between 1348 and 1350 (Cohn 2007, Breuer 1988, and Finley and Koyama 2018).

⁶Kuznets (1960) and Baron and Kahan (1975) provide descriptive evidence on the economic history of Jews and reflect on how discrimination might have affected it, but they provide no econometric evidence. We will discuss the more recent contributions to the literature in more detail later.

to do so.⁷ Second, literacy rates, numeracy, and human capital levels were higher among the Jewish minority compared to the Catholic majority (see Botticini and Eckstein 2005, 2007, 2014). The main implication was that “the combination of circumstances made serving as moneylenders and pawnbrokers the main occupation of Jews in Germany” (from the entry “Germany” in *Encyclopedia Judaica*).⁸ Following the Protestant Reformation in 1517, the German lands split between Catholics and Protestants (see Becker and Woessmann 2009).⁹ Protestant views on usury were less restrictive, and Protestant moneylending was allowed (or at least tolerated) (see Nelson 1969). Moreover, Martin Luther urged his followers to advance education, reducing the human capital gap between the Jews and the majority population (see Becker and Woessmann 2011). Hence, whereas in Catholic areas complementarities between Catholics and Jews persisted (and, in fact, were reinforced following the Catholic Council of Trent, held between 1545 and 1563, which equated usury with murder),¹⁰ in Protestant areas Jews lost their prerogatives in the moneylending sector, which should have resulted in increased competition with the majority population.^{11,12}

Did this change in complementarities between the Jewish minority and the rest of the population affect Jewish history? We show that following the Protestant Reformation, anti-Semitism increased in Protestant Germany relative to Catholic Germany, and this relative increase was more accentuated in trade cities and cities in which Jewish moneylending was established *before* the Reformation.

To document these facts, we collected new data covering Jewish history in German cities and regions over nearly six centuries. First, we assemble a large panel dataset on pogroms and other anti-Semitic behavior with observations available every century from 1300 to 1900 for more than 2,000 German cities. We use these data to document that pogroms, the killings of Jews and expulsions of Jewish

⁷Canon 67 of the Lateran Council states, “Jews may not charge extortionate interest,” but they may charge interest.

⁸From the entry “Germany” in the *Encyclopedia Judaica* (edited by Cecil Roth and Geoffrey Wigoder): “[In the twelfth and thirteenth centuries], the city guilds forced the Jews out of the trades and the regular channels of commerce; this coincided with the stricter appliance of the church ban on usury [...]” Earlier, Israel Abrahams (1896) wrote that “when the medieval Jews devoted themselves largely to commerce and moneylending, they were not obeying a natural taste nor a special instinct but were led to these pursuits by the force of the circumstances, by exclusive laws, and by the express desire of kings and people.”

⁹Readers interested in a broader discussion of the causes of the Reformation are referred to a recent survey (Becker, Pfaff, and Rubin 2016). In our empirical analysis, we employ difference-in-differences estimation allowing us to check for absence of pre-trends with respect to our outcomes of interest.

¹⁰From the Catechism of the Council of Trent: “To this class also belong usurers, the most cruel and relentless of extortioners, who by their exorbitant rates of interest, plunder and destroy the poor. Whatever is received above the capital and principal, be it money, or anything else that may be purchased or estimated by money, is usury; for it is written in Ezechiel: He hath not lent upon usury, nor taken an increase; and in Luke our Lord says: Lend, hoping for nothing thereby. Even among the pagans, usury was always considered a most grievous and odious crime. Hence the question, ‘What is usury?’ was answered: ‘What is murder?’ And, indeed, he who lends at usury sells the same thing twice, or sells that which has no real existence.”

¹¹Baron (1972, p. 451) refers to this competition between Protestants and Jews which he describes as (ultimately) fostering capitalism and explicitly refers to competition between the two groups in banking and trade as a source of tensions: “Protestants and Jews contributed much more than their share to the rise of capitalist institutions and the so-called capitalist spirit [...] These activities by bankers and merchants of both faiths may have stimulated competition and economic rivalries between them which at times created new tensions.”

¹²While we do not have direct measures of increased competition between Protestants and Jews in the immediate aftermath of the Reformation, we present historical narrative, as well as evidence on long-run differences in the Jewish market share in banking.

communities, increased in Protestant Germany relative to Catholic Germany following the Reformation.

Second, we assemble data on all known books printed in German cities between 1450 and 1600. We use these data to construct a panel measure of anti-Semitic attitudes in 10-year intervals, and we provide quantitative evidence of the change in these attitudes in Germany following the Protestant Reformation. We find that the absolute number and the share of books with anti-Semitic titles printed in Protestant Germany increased relative to Catholic Germany following the Protestant Reformation.

The fact that the Reformation had an impact on anti-Jewish attitudes and acts does not necessarily support our theory; that is, the Reformation's impact on anti-Semitism could have stemmed from many channels unrelated to the division of labor between the Jewish minority and the Christian majority. For example, in his final years, Martin Luther himself denounced the Jewish people. Luther mentions lending as a salient activity and urges that they should "earn their bread in the sweat of their brow."¹³ But he does not limit his rage to this one occupation but instead calls for wholesale expulsion ("eject them forever from the country").¹⁴

To test our theory, we collected further city-level data on the sectorial specialization of the city and on the presence of Jewish lending before the Reformation. We use these data to run two different types of difference-in-difference-in-differences exercises.

First, we show reduced-form evidence that the relative increase in anti-Semitism in Protestant compared to Catholic cities after the Reformation was larger in cities where trade was a salient pre-Reformation sector. This evidence supports the view that (Catholic) trade cities had higher returns to banking, making them more attractive for Jewish moneylenders and partially shielding the Jewish minorities from Catholic persecutions. After the Reformation, the increase in competition between the Jews and the majority population resulted in an increase in anti-Semitism that was larger in trade cities than in cities where trade was of lesser importance. A priori it is not clear whether the Protestant Reformation led to an increased competition between the Christian majority and the Jewish minority through a decreasing importance of the original usury ban or through an increasingly educated Protestant population.

The second difference-in-difference-in-differences analysis, however, suggests an important role for moneylending rather than other skill-intensive sectors.

¹³In his book *Von den Juden und ihren lügen (On the Jews and Their Lies)*, Martin Luther proposes the following actions against the Jews: "First, to set fire to their [the Jewish] synagogues or schools [...] Second, I advise that their houses also be razed and destroyed. [...] Third, I advise that all their prayer books and Talmudic writings, in which such idolatry, lies, cursing, and blasphemy are taught, be taken from them. [...] Fourth, I advise that their rabbis be forbidden to teach henceforth on pain of loss of life and limb [...] Fifth, I advise that safe-conduct on the highways be abolished completely for the Jews. For they have no business in the countryside, since they are not lords, officials, tradesmen, or the like. [...] If you great lords and princes will not forbid such usurers the highway legally, some day a troop may gather against them, having learned from this booklet the true nature of the Jews and how one should deal with them [...] Sixth, I advise that usury be prohibited to them, and that all cash and treasure of silver and gold be taken from them [...] Seventh, I recommend putting a flail, an ax, a hoe, a spade, a distaff, or a spindle into the hands of young, strong Jews and Jewesses and letting them earn their bread in the sweat of their brow, as was imposed on the children of Adam. [...] then let us emulate the common sense of other nations such as France, Spain, Bohemia, etc., compute with them how much their usury has extorted from us, divide, divide this amicably, but then eject them forever from the country [...]"

¹⁴We cannot rule out that readers of this pamphlet might have limited their anger to cities where Jews were actively engaged in moneylending, and the impact of this pamphlet is hard to measure anyways, but if anything, the pamphlet in its entirety seems to call for wholesale expulsion of Jews, independent of their actual occupation.

Specifically, we show that the shift in anti-Semitism from Catholic areas to Protestant areas that followed the Reformation is largely explained by cities in which the Jews used to be moneylenders before the Reformation.

We combine these two analyses in an instrumental variables framework where pre-Reformation trade specialization serves as an instrument for pre-Reformation Jewish lending. To the extent that the exclusion restriction holds, this establishes a causal effect of pre-Reformation Jewish moneylending on the post-Reformation shift in anti-Semitism. In this analysis, we are *not* assuming that the trade specialization in 1500 of German cities was random. Cities that specialized in different sectors differed across several dimensions, which are captured by the city fixed effects. Moreover, we add sector-by-century fixed effects in the regressions to control for the fact that the geography of pogroms might have evolved differently for cities that specialized in different sectors.

To address concerns that these effects are the result of anti-Semitic preaching against usury as opposed to economic competition per se, we consider areas with Lutheran and Calvinist rulers. While the late Luther was more outspokenly anti-Semitic, Calvin was less so, but was more supportive of the habit of usury. The fact that the relative shift toward anti-Semitism is more pronounced in Calvinist areas is consistent with the relevance of our proposed economic channel.

To study the long shadow of the Reformation, we use data on a cross section of 452 counties in Prussia, the largest state of the German Empire. We find that local Jewish communities are smaller in 1882 in Protestant areas, and that vote shares for anti-Semitic parties in 1890, 1893, and 1898 are higher. This finding complements our previous findings on the changing geography of anti-Semitism after the Reformation. Furthermore, using the 1882 Prussian occupational census, we find a lower Jewish market share in banking and finance in Protestant areas. Pseudo-difference-in-differences estimates and instrumental variables estimates suggest that these findings are a consequence of the Reformation. These findings confirm that the Reformation reduced the comparative advantage of Jews in these sectors in Protestant areas at the same time as comparative advantages persisted in Catholic areas.

In summary, using a combination of city-level and county-level data as well as historical narrative, we show that the Protestant Reformation induced the following changes. (i) Jewish pogroms, the expulsion of Jews, and anti-Semitic attitudes (captured by anti-Jewish publications and votes for anti-Semitic parties) worsened in regions that became Protestant compared to those that remained Catholic. (ii) This increase in anti-Semitism in Protestant regions was more accentuated in cities that were specialized in trade and in which Jewish moneylending was established *before* the Reformation, serving an important role in the economy. (iii) While before the Reformation, Jewish lenders were quite evenly spread between regions that would become Protestant compared to those that would stay Catholic, at the end of the nineteenth century Jewish involvement in finance and banking was higher in the Catholic regions relative to Protestant regions.

We interpret these findings as evidence that, with the Reformation, the Jews lost their comparative advantage in lending: this exposed them to competition with the Christian majority and led to an increase in ethnic and religious hostility toward the Jews.

The paper proceeds as follows. Section I gives the historical background and discusses related literature. Section II presents our data sources. Section III gives results at the city level documenting the shift in anti-Semitism from Catholic to Protestant cities, as well as on the long-run effects of the Reformation on anti-Semitism. Section IV provides evidence that this shift is explained by the change in the economic complementarity between the Jewish minority and the majority population induced by the Reformation. Concluding remarks close the paper.

I. Historical Background and Related Literature

In the first three centuries CE, there is no evidence of a systematic Christian ban on usury. In the First Ecumenical Council at Nicaea in 325 CE, the prohibition against usury entered Canon Law. This prohibition was limited to the clergy, and usury was defined as excessive interest.¹⁵ Charlemagne extended the definition of usury to every loan that charged interest (“where more is asked than is given”) and prohibited usury to everyone in his empire. With the Synod of Pavia in 850 CE, this prohibition entered Canon Law.¹⁶ The Second (1139) and the Third (1179) Lateran Councils strongly reaffirmed the interest ban.¹⁷ The immorality of interest was also asserted by St. Thomas Aquinas.¹⁸ This put a tight lock on the practice of usury, “which would put the church in a theoretical bind for centuries because his writings were considered among its highest philosophical and theological teachings” (Geisst 2013, p. 51).

Canon Law applied to the Catholics. In the words of Geisst (2013, p. 23), “as canon law developed, an ‘otherness’ would come to characterize Jews and other money-lenders¹⁹ who did not follow the precepts of the church.” Jewish moneylending was tolerated. In fact, by forbidding the Jews to lend for an immoderate profit,²⁰ the

¹⁵Canon 17 in the First Council of Nicaea: “Forasmuch as many enrolled among the Clergy, following covetousness and lust of gain, have forgotten the divine Scripture, which says, He has not given his money upon usury, and in lending money ask the hundredth of the sum [as monthly interest], the holy and great Synod thinks it just that if after this decree anyone be found to receive usury, whether he accomplish it by secret transaction or otherwise, as by demanding the whole and one half, or by using any other contrivance whatever for filthy lucre’s sake, he shall be deposed from the clergy and his name stricken from the list.”

¹⁶The Synod of Pavia prescribed excommunication of lay usurers and called for restitution of interest to their victims.

¹⁷Canon 13 in the Second Lateran Council: “We condemn that practice accounted despicable and blameworthy by divine and human laws, denounced by Scripture in the old and new Testaments, namely, the ferocious greed of usurers; and we sever them from every comfort of the church.” Canon 25 in the Third Lateran Council: “Nearly everywhere the crime of usury has become so firmly rooted that many, omitting other business, practice usury as if it were permitted, and in no way observe how it is forbidden in both the Old and New Testament. We therefore declare that *notorious* usurers should not be admitted to communion of the altar or receive Christian burial if they die in this sin.”

¹⁸This is the answer given by St. Thomas Aquinas in the Second Part of the *Summa Theologica* (1274) to the question of whether usury is a sin: “To the objection, that a man may take a price for what he is not bound to do; but a man with money is not in every case bound to lend it, it is to be said that he who is not bound to lend may receive compensation for what he has done in lending, but ought not to exact more. But compensation is given him according to the equality of justice, if the exact amount is returned to him that he has lent.”

¹⁹Jewish moneylenders were competing in the Middle Ages with the Lombards and Cahors. Originally, these two groups were Arians and, as such, did not acknowledge the Council of Nicaea. They were considered heretics and therefore fell outside Canon Law (Geisst 2013, p. 23).

²⁰Canon 67 in the Fourth Lateran Council: “Wishing, therefore, in this matter to protect the Christians against cruel oppression by the Jews, we ordain in this decree that if in the future under any pretext Jews extort from Christians oppressive and immoderate interest, the partnership of the Christians shall be denied them till they have made suitable satisfaction for their excesses.”

Fourth Lateran Council (1215) de facto authorized them to lend for a moderate profit. Why did the Catholic Church tolerate Jewish usury? St. Thomas Aquinas gives the answer in the *Summa Theologica*: Jews were permitted to lend money to avoid the even greater danger that Christians would practice usury²¹ (Geisst 2013, p. 51; Poliakov 1977, p. 26).

Starting from the twelfth and thirteenth century, moneylending and pawnbroking became the main occupations of the Jews.²² The main specialization of Jews in German regions continued to be in finance and banking until the nineteenth century²³ (see online Appendix B for a complete set of citations about Jews and moneylending in German history from the *Encyclopedia Judaica*).

The historical literature has emphasized two different motives that explain this phenomenon: (i) the Catholic tolerance toward Jewish lending discussed above; (ii) the high levels of human capital among the Jews (Botticini and Eckstein 2005, 2007, 2014).^{24,25}

The sixteenth century brings the Protestant Reformation. Much has been written about the significance of the Protestant Reformation as a watershed moment in European history, in the general social science literature (Weber 1904), and in economics (Ekelund, Hébert, and Tollison 2002; Becker and Woessmann 2009; Cantoni 2012, 2015; Cantoni, Dittmar, and Yuchtman 2018; Dittmar and Meisenzahl forthcoming; Spenkuch 2017). Work on the exact adoption patterns of the Reformation is comparatively scarce, as shown in a recent survey by Becker, Pfaff, and Rubin (2016). We are not aware of systematic research on Reformation adoption for the whole set of cities covered in our analysis, although one theme stressed consistently in the literature is that the Reformation was an urban phenomenon.

With the Protestant Reformation, the German religious landscape changed dramatically. After a period of turmoil following the start of the Reformation in 1517 in Luther's city of Wittenberg, the Imperial Diet held in 1555 in Augsburg adopted the principle "Cuius regio, eius religio" (Whose rule, his religion). This meant that denominational choices were made by the rulers of the large number of territories that constituted the fragmented German Empire at the time of the Reformation. The Reformation brought about two important changes with respect to moneylending and the relationship between Christians and Jews. First, Jones (2004, p. 87), building on

²¹"As for their taking usury of strangers, that was not granted them as a thing lawful, but permitted for the avoidance of a greater evil" (Aquinas 1274).

²²A similar transition of Jews from their traditional occupations (dyers, silk weavers, and traders) to moneylending occurred in Italy. For a detailed description of the Italian context, see Pascali (2016).

²³We collected data on every profession/economic activity of the Jewish minority in the city-level articles of the encyclopedia *Germania Judaica*, Volume 3 (which covers the years from 1350–1519). In cities with such information, the most cited professions are in the sectors of moneylending and insurance (71 percent of entries), followed by trade (8 percent), medicine (7 percent), food (4 percent), and artisanship (3 percent). So, moneylending is the most salient profession of the Jews in the years before the Reformation.

²⁴The path-breaking research of Botticini and Eckstein covers the time until 1492, before the Protestant Reformation, and takes a worldwide perspective. It stresses that early specialization of Jews in urban occupations was the result of their higher levels of human capital with respect to the Christian population. Starting from the seventh century, whereas the rest of the population was illiterate, all Jewish men were literate because of an education reform of the first century CE. "Why were Jewish farmers (and Jews in general) literate whereas the rest of the rural population was illiterate at the beginning of the seventh century? The Jewish religion made primary education mandatory for boys in the first century when the high priest Joshua ben Gamala issued an ordinance that teachers had to be appointed in each district and every city and that boys of the age of six or seven should be sent."

²⁵Other authors have emphasized the fact that the city guilds forced the Jews out of their traditional occupations in German cities in the twelfth and thirteenth centuries. See Ogilvie (2014).

Nelson (1969), argues that although Luther “was principally opposed to lending money at interest, but made provisions for the practice, Calvin supported and defended the habit of usury, except in a few inherently unloving circumstances.” Protestants were thus allowed (or at least tolerated) to engage in moneylending.²⁶ Second, Protestants, with their emphasis on education, acquired human capital that equipped them with the education necessary to enter highly skilled occupations such as moneylending.²⁷

The combination of these factors in Protestant areas might have disrupted the inter-ethnic complementarities that existed between Jews and Christians (who were all Catholic before the Reformation).²⁸ In a sense, the Reformation made Jews “redundant” in the moneylending business in Protestant areas,²⁹ whereas inter-ethnic complementarities survived in Catholic areas. In fact, in Catholic areas, the interest ban was, if anything, strengthened³⁰ during the Counter-Reformation, and it survived until the eighteenth century.³¹

²⁶Nelson (1969) points out that Luther is more ambiguous about his standpoints on usury than Calvin in the following sense: Luther started out in his early years holding strongly “Catholic views,” which he later weakened, or rather adjusted to practice. In fact, Nelson points out that Luther’s views in pamphlets are quite different from his practical views in his exchanges with Protestant rulers. One might say that he has a “theological” view (in his religious pamphlets) and a “secular” view (when advising secular Protestant rulers). Nelson discusses in detail how Luther’s views are shaped by the realities of the Reformation. His initial hardline stance on usury was interpreted by the poorer parts of the population as an invitation to renege on payment of interest: “In many areas beside Eisenach, extremist elements were to suspend payments of interest charges to creditors” (ibid., p. 43). This particular issue, but also others, raised the question of whether the Reformation was a bottom-up “extremist” movement that was hard to control, or a top-down reform led by secular rulers. Luther clearly favored a controlled reform movement: “If there was to be reform, Luther taught, it would have to come from the princes, and not the people” (ibid., p. 44). Nelson goes on to write: “Whatever Christ may have counselled, said Luther, in obvious reference to the situation at Eisenach and elsewhere, the civil authorities were right to use the sword against recalcitrant debtors. He insists repeatedly that Christians of the sixteenth century were no more bound by the ‘judicial laws’ of Moses than they were by the ceremonial laws, such as the law of circumcision. [...] The Christian man, Luther was to say, was in the truest sense of the Gospel free to lend his money as he chose. Not the Gospels, but the economic situation and the considerations of public utility, were of paramount importance in finding clues for the regulation of loans at interest” (ibid., pp. 44–45). German language sources had been highlighting these same points since at least the middle of the nineteenth century: interestingly, Neumann (1865) has two sections entitled “Luther und seine Anhänger wirken für das kanonische Wuchergesetz, dann praktisch dagegen” [Luther and his followers work to defend the canonical prohibition of usury, but then [are] practically against it] (pp.480–92) and “Calvin tritt gegen das kanon. Wuchergesetz auf” [Calvin stands up against the canonical prohibition of usury] (pp.492–95). See also Hattenhauer (2015). Further references are to be found in the entry “Zins” (=Interest) in the *Theologische Realenzyklopädie* (2004), the definitive reference to key scholarly works in theology and history. We will return to the differences between Calvin and Luther later on.

²⁷See Becker and Woessmann (2009) for historical details and long-term consequences of the Reformation on literacy and economic development from a regional perspective.

²⁸It should be stressed that in Germany, Protestantism is mostly of the Lutheran type. For instance, in Prussia (the largest state by far), Lutherans constitute 94 percent of all Protestants, and only 6 percent of Protestants are Reformed Protestants. However, there are larger numbers of Calvinists in Southern Germany. As mentioned earlier, Luther’s and Calvin’s views on usury differed (see Jones 2004), at least in their emphasis or toleration of everyday practice.

²⁹The recent work by Cantoni, Dittmar, and Yuchtman (2018) highlights the fact that Protestant areas not only turned away from Catholicism but also saw a shift from religious to secular activities. More broadly speaking, the Reformation succeeded because Luther took a practical approach to economic affairs and preferred a rules-based approach to institute the Reformation (see, e.g., Dittmar and Meisenzahl forthcoming on the role of church ordinances in the Reformation). As highlighted above, while the theologian Luther opposed lending at interest, the politician Luther was practically minded, permitting lending at interest.

³⁰De Roover (1948, p. 151) writes, “In the sixteenth century, however, a reaction set in, apparently in an attempt to counteract the spread of the Reformation. The Church reaffirmed its traditional doctrine on the matter of usury and reverted to the uncompromising attitude, which had prevailed prior to the fifteenth century. The secular authorities, however reluctantly, continued to issue licenses, but the Church henceforth refused to grant dispensation to the Lombards. They were, and remained, excommunicated. According to Charles V’s ordinance of January 30, 1546 (n.s.), licensed usurers were forbidden to attend mass or to enter any church under the penalty of forfeiting their licenses. The same prohibition applied to anyone who was in partnership with them, who owned a share in their *tables de prêt*, or who participated in their management.”

³¹In 1745, in the *Encyclica Vix Pervenit*, Pope Benedict XIV writes, “The sin [in usury] rests on the fact that sometimes the creditor desires more than he has given. Therefore he contends some gain is owed him beyond that which he loaned, but any gain which exceeds the amount he gave is illicit and usurious.” In the following years, the

According to our hypothesis, this should have led to a relative increase in anti-Semitic acts in Protestant areas versus Catholic areas following the Protestant Reformation, and this increase should have been more pronounced in areas in which Jewish lending had been more economically relevant to the local population.³² In the empirical sections, we document anti-Semitic acts and attitudes over the centuries and consider whether, following the Protestant Reformation, there was a changing pattern across Protestant and Catholic areas and across areas with different returns to banking, i.e., more or less “need” for Jewish moneylending. Moreover, in online Appendix D, using Prussian census data, we document the impact of the Reformation on the involvement of the Jewish minority in finance and banking over the long run.³³

The only other (recent) working papers we are aware of that empirically study a link between pogroms and moneylending/finance are D’Acunto, Prokopczuk, and Weber (forthcoming) and Grosfeld, Sakalli, and Zhuravskaya (forthcoming).³⁴ D’Acunto, Prokopczuk, and Weber find that present-day financial development is lower in German counties where historical anti-Semitism was higher, compared to otherwise similar counties. Grosfeld, Sakalli, and Zhuravskaya (forthcoming) look at pogroms between 1800 and 1927 in the Pale of Settlement, where Jews were confined to live within the Russian Empire. Their results show that ethnic violence broke out when crop failures coincided with political turmoil. Crop failures without political turmoil did not cause pogroms. At the intersection of economic and political shocks, pogroms occurred in places where Jews dominated moneylending and trade in grain. Also related is the earlier work by Grosfeld, Rodnyansky, and Zhuravskaya (2013). They show that current-day residents of the former Pale of Settlement have lower support for the market compared to those outside the Pale. The mechanism they document is that anti-Semitism generated persistent anti-market culture and trust among non-Jews.

The conceptual framework for our empirical analysis follows the recent paper by Jha (2018), which studies the interactions between a group of “locals” and “non-locals,” where by assumption the nonlocals “enjoy superior information and networks outside a particular location.” Individuals produce goods that impose an externality on others who produce those goods: agents producing the same good always act as substitutes, reducing the payoffs to one another from production, while agents producing different goods may provide either complements or substitutes to each other, respectively raising or lowering one another’s payoffs. Jha’s case of “immigrant minorities” (which he labels as vulnerable outsiders) is the one he

Catholic definition of usury changed. Starting from the work of Scipione Maffei (whose *Dell’impiego dell danaro* [*On the Use of Money*] was widely discussed), usury is defined as “any increment—not beyond the principal—but beyond the moderate rate allowed by law or customs. The new definition represented a radical departure from the basic norms of scholastic economics” (de Roover 1955). Finally, in 1830, the Church too abandoned punishment of usurers, although it did not formally revoke the usury doctrine (see Geisst 2013).

³²Notice that even before the Reformation, Jews were by no means sheltered from attacks. Pogroms against Jews broke out occasionally, such as after the Black Death in 1348–1350, for which Jews were partly blamed.

³³Furthermore, in online Appendix C, we provide a case study on the Margraviate of Brandenburg showing the rise in competition between Protestant and Jewish lenders and the subsequent expulsion of the Jews.

³⁴Finley and Koyama (2018) look at how political rule affected regional variation in the intensity of Black Death pogroms. They consider revenue generated by Jewish moneylending as a primary source of conflict, but do not employ any measure of moneylending.

himself uses to characterize Jews in Europe.³⁵ His model highlights the importance of inter-ethnic complementarities in supporting peaceful coexistence equilibria over long time horizons. A further requirement for the peaceful coexistence equilibrium is that the sources of the inter-ethnic complementarity be costly to replicate or expropriate and that the immigrant minority can threaten to leave. In our case, this complementarity naturally arises as a result of the usury ban imposed by the Catholic Church (which we take as given in this paper), which makes it costly for Christians to replicate the services of Jewish moneylenders. While Christian rulers had the military power to expropriate and/or expel Jews, the historical account shows that this was costly in practice. Expulsions of Jews were often temporary and the same ruler who expelled them invited them back again a few years later. At the same time, Jha's model shows that peaceful coexistence is often supported by transfers from rich nonlocals to a particular set of locals: the "strong" (often incumbent political elites), as these have the lowest costs of engaging in violence. In fact, Jews often paid special taxes to the local ruler in order to buy protection. This provided perverse incentives for rulers to intermittently allow ethnic violence by poorer locals in order to extract greater transfers from non-local minorities. All of this is well in line with what we observe in the German context.

In this stylized account, we abstract from Christian lending. Notice, however, that while the Canon Law placed absolute prohibitions and harsh penalties on Christian usury, it is also true that these measures were frequently ignored in practice (see Nelson 1969 for a complete treatment of the subject). First, from the thirteenth century onward some Italian moneylenders, the Lombards, migrated to German cities, mostly located in the Rhineland, and were granted privileges similar to those granted to the Jews.³⁶ Second, occasionally, so-called Mounts of Piety were created in the fifteenth century under the direct prompt of the Catholic Church, the most well-known being the one in the city of Nuremberg.³⁷ These institutions had normally a philanthropic purpose and were lending money (in advance) to the poor without interest, against a pledge (like pawnshops). Third, the historical literature has documented widespread clandestine interest-taking among non-professional lenders in Medieval German cities (Vornefeld 1989). Finally, some powerful Catholic families had emerged in the fifteenth and sixteenth century from the money exchange and insurance markets and were providing credit to the local rulers masking the interest under different forms.³⁸

Although there is evidence of Christian lending, it is clear, however, that moneylending was mainly practiced by the Jews. Moneylending was the most important occupation for Jews, and Jewish moneylending played an important role for German rulers and citizens. Online Appendix B gives a series of quotes in the *Encyclopedia Judaica* about Jews and moneylending in German history.

³⁵ In fact, Jha (2018) cites our paper as an example consistent with his theory.

³⁶ In the words of Schmieder (2009, p. 31), the privileges of lending that were granted to the Lombards "change the ethnic group of Italian money-merchants in towns on the Rhine into more than just a legal minority: we might finally have found, if not exactly a new type of religious minority, a minority objectionable on religious grounds and in consequence partially excluded from the always religiously defined urban society."

³⁷ For a description of the origin of the Mounts of Piety and their evolution in Italy, see Pascali (2016).

³⁸ The classical example is the Fugger family. This family was closely linked with the Habsburg Emperors of the Holy Roman Empire and financed wars in exchange for the proceeds of silver mines and tax receipts of certain areas ruled by the Habsburgs (Steinmetz 2016).

II. Data and Descriptive Statistics

In the empirical section of this paper, we test the following hypotheses: (i) the Protestant Reformation induced more anti-Semitic acts and anti-Jewish publications in the regions that became Protestant *relative* to the regions that remained Catholic; (ii) if this change was related to the fact that Jews lost their prerogatives in moneylending, it should be more accentuated in cities with larger returns to banking, i.e., in which there was a “need” of Jewish lenders before the Reformation and in which Jewish moneylending was established before the Reformation.³⁹

To test these hypotheses, we need a wealth of data. We discuss various data sources in turn and provide more detailed information on how we coded key variables in online Appendix A.

A. City-Level Data: 1300–1900

Our main sources for data about Jewish communities in Germany are *Germania Judaica* (GJ) (1963–2009) and Alicke (2008). We consult the *Encyclopedia Judaica* (2007) for comparison as it only covers the largest Jewish communities, whereas *Germania Judaica* and Alicke cover all Jewish communities, large or small. *Germania Judaica* covers the period before the Reformation, whereas Alicke covers the entire period and thus constitutes our main source for the post-Reformation period. *Germania Judaica* is richer in breadth and allows us to measure, for instance, Jewish moneylending before the Reformation, which we use in our analysis. Anti-Semitic acts and Jewish presence are the key variables in our basic set of regressions. Other city-level data come from the *Deutsches Städtebuch* (1939–1974), a series of volumes edited by Erich Keyser that provide information on each city in the German Empire incorporated prior to the compilation of the *Städtebuch*. The *Städtebuch* covers 2,344 cities. We follow Cantoni and Yuchtman (2014) and exclude cities outside the Holy Roman Empire (dropping 90 cities in East Prussia). In our main estimation sample, we use those cities that were founded before 1500 (i.e., existed before the Reformation) *and* that have a recorded Jewish presence at least once over the years 1300–1800, making for 1,298 cities in our main analysis.⁴⁰ These cities are depicted in online Appendix Figure A.1. In robustness checks, we use all cities recorded in the *Deutsches Städtebuch*. For symmetry, we use two centuries before the Reformation (1300–1500) and two after the Reformation (1500–1700) in our main analysis, but we show that results are robust when using four post-Reformation centuries (1500–1900). For all of our sources, the online Data Appendix gives examples of how we code variables.

Germania Judaica.—We use Volumes 2 and 3 of *Germania Judaica*, covering the centuries before the Protestant Reformation. These books contain city-level information for all Jewish settlements in the German Empire. Data collection started

³⁹In online Appendix D, we show that, in nineteenth century Prussia, the Jewish market share in banking in Catholic areas is larger than in Protestant areas, consistent with persistent advantages in banking in Catholic areas.

⁴⁰When revising the paper, we communicated directly with Klaus-Dieter Alicke who pointed us to his new website (<http://www.juedische-gemeinden.de/index.php/home>), which covers some additional towns compared to the 2008 book edition. We included those in the updated version of our dataset.

at the beginning of the twentieth century and was initiated by the Society for the Advancement of Jewish Studies (Gesellschaft zur Förderung der Wissenschaft des Judentums). City-specific articles were drafted by a consortium of historians from across the German Empire to facilitate access to local archival sources. Volume 1, covering the years until 1238, was completed before World War II, and work on volumes 2 and 3 resumed after World War II. Voigtländer and Voth (2012) introduced *Germania Judaica* into the economics community. To be precise, they used volume 2. We go beyond their work by using a more extensive list of cities (to link the data with all cities covered in the *Deutsches Städtebuch* described below). We code information on pogroms not only in 1348–1349 but at any point in the fourteenth and fifteenth century and beyond, using *Germania Judaica* 3 as well. We also code information about Jewish lending. Specifically, we define, century by century, the following variables: (i) evidence of Jewish presence; (ii) the persecution of Jews (expulsion of parts of the Jewish community; expulsion of the whole community; killings of parts of the Jewish community; killings of the whole Jewish community); and (iii) Jewish lending activity. Of course, absence of proof of Jewish lending activity is not proof of absence, but to our knowledge, *Germania Judaica* is the best available data. Note that we choose to code data century by century because the sources often do not provide more precise information than that. In some cases, entries might only state that there is “evidence of a Jewish community during the *x*th century.”

Alicke (2008).—Because the *Germania Judaica* project has only covered the period until 1519 (up to the Reformation), we draw on Alicke (2008) for the later centuries. This source was first introduced in the economics literature by Voigtländer and Voth (2012). It is an equally impressive collection of more than 4,600 pages on Jewish history covering thousands of cities. We use it to code Jewish presence and the persecution of Jews in the post-Reformation period in the same way as we do with *Germania Judaica*. Unfortunately, Alicke does not capture Jewish lending activity in a systematic way, which is why we cannot provide direct evidence for a shift in Jewish lending activity in the immediate aftermath of the Reformation.

Deutsches Städtebuch.—The *Deutsches Städtebuch* is our source for variables that enter our regressions either as control variables or as instrumental variables. Anti-Semitism might be “collateral damage” of war activity in which cities are involved. We code information from the *Städtebuch* to capture whether there was a battle near a city; whether the city was besieged, sacked, partially destroyed, completely destroyed, or occupied; or whether the city was involved in a war elsewhere.

To the extent that Protestant Reformers emphasized education (see Becker and Woessmann 2009), education may have increased or decreased anti-Semitism depending on whether more educated Protestants increasingly competed with already well-educated Jews (see Botticini and Eckstein 2005, 2007, 2014) or whether education helped to reduce conflict potential. The *Städtebuch* contains information about the presence of a school, which is the best indicator available to capture schooling in a city. Specifically, for each city/century we record time-varying information whether there is any evidence of the presence of a primary or secondary school. Especially in the early centuries, the great majority of these schools are primary church schools.

To the extent that lending activity was more important in some cities than in others, depending on the sector specialization, we code information about salient industries, as described in the *Städtebuch*. We use a city's specialization in trade before the Reformation as instrumental variable to predict Jewish lending activity before the Reformation.

Other Sources.—Data on the denomination of the ruler in the city in the year 1546 and 1650 are based on two detailed maps from Zeeden (1984) that we digitized. The map from 1546 is reported in online Appendix Figure A.1 and underlies the maps displayed in online Appendix Figures A.3–A.8; the map for 1650 is reported in online Appendix Figure A.2. We would like to stress how detailed and accurate the maps by Zeeden are.⁴¹ Consider, for instance, online Appendix Figure A.1 and take the city of Lindau, in the very South of Germany, at the Eastern edge of the Lake of Constance (Bodensee). It is the one red/Protestant area (Lindau's jurisdiction extended to some neighboring villages) at the Southern border of (modern-day) Germany, surrounded by all-blue (Catholic) territory. Lindau, which adopted the Reformation in 1528, is accurately shown as Protestant. Similarly, the city of Memmingen just a bit to the North-East of Lindau shows as Protestant. In the north of Germany, the Zeeden map shows Parchim as a Protestant city in otherwise Catholic territory, in line with its adoption of the Reformation in the year 1530.⁴²

Pogroms may be more likely to happen in larger cities if larger populations can be equated with a higher probability of inter-religious conflict. The most widely known and established source of population data for the German lands is from Bairoch, Batou, and Chevre (1988) and was kindly shared by Cantoni (2015). They cover all cities that, at some point until 1800, reached a threshold of 5,000 inhabitants. We use this data to define intervals for population size starting at 5,000 inhabitants, and we group city-century observations with less than 5,000 inhabitants in the [0, 5,000] interval.

Panel A of online Appendix Table A.1 shows descriptive statistics for the sample of 1,298 cities over 4 centuries (i.e., for 5,192 observations). On average, 69 percent of city-century observations have some evidence of Jewish presence. Moreover, 13 percent of city-century observations have evidence of pogroms (i.e., killings or expulsions of Jews). Online Appendix Table A.2 shows those indicators century by century and separately for Protestant and Catholic cities (defined on the basis of whether a city was Protestant or Catholic in 1546; we explain the origin of this definition further below). This table documents two interesting facts. First, Jewish presence over the centuries is practically the same between Catholic and Protestant cities. Second, there was a crossover in the incidence of pogroms between the two pre-Reformation centuries (1300–1400 and 1400–1500) to the two post-Reformation centuries (1500–1600 and 1600–1700). While pogroms are more prevalent in Catholic cities before the Reformation, this changes during the sixteenth century. Online Appendix Figures A.3 to A.6 show the geographic distribution of cities

⁴¹ A scan of the original multi-color maps is available from us on request.

⁴² Still, to further show that our results do not rely exclusively on the coding of cities as Protestant or Catholic based on one particular map, we re-ran our main analysis on the smaller set of cities used in Cantoni (2012) who hand-coded cities as Protestant or Catholic based on various sources. Our results are unchanged. We are grateful to Davide Cantoni for sharing his data with us.

with Jewish communities and pogroms over four centuries (while online Appendix Figures A.7 and A.8 extend the data to the nineteenth century). Going back to online Appendix Table A.1 and turning to military conflicts, the most common type are sackings, which affect 12 percent of city-by-century observations. There is evidence for the presence of a school for 41 percent of city-by-century observations for which there is information on schools in the *Städtebuch* (4,920 out of 5,096 observations).

Panel B of online Appendix Table A.1 presents a cross-sectional view of the same set of 1,298 cities for variables for which we use no variation over time. Just over half of the 1,298 cities had a Protestant ruler in 1546 and about 60 percent in 1650. Jewish lending activity before 1500 is documented for one-quarter of the cities. (These cities are displayed in online Appendix Figure A.9). As for the sector structure of cities, there is an average of 1.4 entries on salient sectors per city from the *Städtebuch* and 0.2 entries for trade and trade-related services.

One might worry that there is a systematic difference in record-keeping of city histories between Protestant and Catholic cities. Online Appendix Table A.3 compares the length of entries in the *Städtebuch* between Protestant and Catholic cities. The average length of entries is three pages for all cities, with Catholic cities, if anything, having marginally longer entries. While we did not perform a similar exercise for length of entries in the *Germania Judaica*, it seems unlikely that differences in pogrom intensity between Catholic and Protestant cities is driven by differences in reporting.

B. Anti-Jewish Sentiment in Books: 1450–1600

The Universal Short Title Catalogue (USTC) produced at the University of St. Andrews (2012) is the primary source of data on book editions that were published around the Protestant Reformation. The USTC is designed as a universal catalogue of all known books printed in Europe in 1450–1600 and provides information for each book on the city in which it was published, the language, and the year of publication.

Data on the number of anti-Jewish books published in each city/decade were constructed as follows. First, we downloaded the USTC catalogue for all books in German and Latin that were published in the cities in our sample. We identified a total of 88,457 books with complete information about the city and year of publication (40,738 in German and 47,719 in Latin). Second, we identified 5,000 books in the USTC that were clearly not anti-Jewish. Third, we used Christian Wolf's (1715) *Bibliotheca Hebraea*, a comprehensive catalogue of books printed before 1715 that has a dedicated section listing anti-Jewish content, to identify 201 anti-Jewish books published in either Latin or German. Fourth, we used the books identified as either anti-Jewish or not (the "training sample") to measure the distribution of words across the two categories of books, following the same logic as the seminal work by Dittmar and Seabold (2015). Generally, the titles of these books provide extended descriptions of the contents.⁴³ This allowed us to determine which features of language are important in identifying anti-Jewish books. Fifth, we used the naïve Bayesian text algorithm to construct a ranking of books based on their probability

⁴³The median title in our data has 22 words (mean = 23.77) and 160 characters (mean = 176.77).

of being anti-Jewish. Finally, we computed the total number of books in each city/decade that were classified among the top 2 percent⁴⁴ in terms of probability of being anti-Jewish.

The naïve Bayesian classifier is a probabilistic classifier that applies Bayes' theorem to compute the probability that a certain text pertains to a certain category under the assumption that words are conditionally independent of each other.⁴⁵ For instance, assume that a title is made of n words X_1, \dots, X_n ; then,

$$(1) \quad \frac{\Pr(\text{antiSemitic} | \text{title} = X_1, \dots, X_n)}{\Pr(\text{book antiSemitic})} = \frac{\prod_{i=1}^n \Pr(X_i | \text{antiSemitic})}{\prod_{i=1}^n \Pr(X_i)}.$$

Notice that $\Pr(X_i | \text{antiSemitic})$ and $\Pr(X_i)$ can be computed for every i once we have defined an initial set of books that are anti-Jewish and an initial set of books that are not. We then use the estimated $\frac{\Pr(\text{antiSemitic} | \text{title} = X_1, \dots, X_n)}{\Pr(\text{book antiSemitic})}$ to construct a ranking of books from those with a higher probability of being anti-Jewish to those with the lowest probability.

To understand the type of books that are categorized among the top 2 percent in terms of probability of being anti-Jewish by the algorithm, here are some examples:⁴⁶

- *De veritate fidei christianae libri quinque in quibus de religionis nostrae fundamentis contra Ethicos Judaeos Agarenos sive Mahumetanos et perverse christianos plurima subtilissime simul atque exactissime disputantur.*⁴⁷
- *Epistola contra Judaeorum errores.*⁴⁸
- *Ratschlag ob Christlicher Obrigkeit gebueren muege das sie die Jueden unter den Christen zu wonen gedulden und wo sie zu gedulden welcher gestalt und mass.*⁴⁹
- *Frage. Ob ein rechtgleubiger Christ mit Unchristen als mit Juden Tuercken heiden oder mit offentlichen uberfuerten ketzern muege Buergerliche gemeinschaft haben mit inen essen und trincken.*⁵⁰

⁴⁴We choose this value because it minimizes the probability of classifying a book as either being anti-Jewish when it is not or not being anti-Jewish when it is within the training sample. In all the regressions, the choice of this cutoff affects the estimated constant but not the other coefficients.

⁴⁵Many empirical comparisons between naïve Bayes and more complicated decision tree algorithms showed that the naïve classifier is one of the most efficient and effective classifiers for machine learning and data mining, even if the conditional independence assumption is rarely true in real-world applications (see Kononenko 1990; Langley, Iba, and Thomas 1992; and Pazzani 1996). Recent articles have shown that there are sound theoretical reasons for the apparently implausible efficiency of naïve Bayes classifiers (see Roth 1999, Hand and Yu 2001, and Zhang 2004).

⁴⁶Online Appendix Figure A.10 reports the word cloud for anti-Jewish Latin books. Not surprisingly, the most common words are “Contra” and “Iudei” (“Against” and “Jews”). Among the most frequent words, we also see “Errores” (Mistakes), “Adversus” (Enemy), “Perfidia” (Perfidy), “Foenore” (Usury), and “Infidels” (Infidels).

⁴⁷*Five books on the truth of the Christian faith, which discuss the fundamentals of our religion against Pagans, Jews, Muslims and on the side of the Christians in a keen and accurate way.*

⁴⁸*Letter against the Jewish mistakes.*

⁴⁹*Advice as to whether Christian rulers should permit Jews to live among Christians and where and to what extent they might be tolerated.*

⁵⁰*Question. Whether a righteous Christian should be allowed to have community or eat and drink with non-Christians such as Jews, Turks and heathens or with convicted heretics.*

There is a printing of at least 1 edition of a German or Latin language book in 149 German cities over the decades 1451–1600, and in 108 cities at least 10 editions are printed over this period (see online Appendix Figure A.11). As shown in panel C of online Appendix Table A.1, the average number of books printed per city-by-decade observation is 40.20, ranging from 0 in decades without printing to 1,433 editions in just 1 decade in the most print-active city. The average number of editions with predicted anti-Jewish content is 0.11, ranging between 0 and 9 editions. Our main estimation sample for the analysis of book titles is the set of cities that have (i) at least 10 book editions over the decades 1451–1600; (ii) the same religion as the surrounding territory. The rationale for the first criterion is that we want to exclude cities in which there is little evidence of printing in the years of analysis. We run robustness regressions on the set of cities with at least 1, at least 5, at least 25, at least 50, and at least 100 print editions in 1451–1600. The rationale for the second criterion is that we expect readership to come both from the printing cities themselves, but also from the surrounding countryside. In robustness regressions, we also include cities with a different denomination than the surrounding countryside.

C. Prussian County Data at the End of the Nineteenth Century

For the post-Reformation period, we also draw on Prussian census data (Becker et al. 2014) to study in more detail, for 452 counties, the link between the Protestant Reformation and anti-Semitism. In the main text, our outcome of interest is vote shares for anti-Semitic parties in the 1890, 1893, and 1898 elections to the *Reichstag*.⁵¹ Vote shares for anti-Semitic parties constitute an alternative measure of anti-Semitism in the late nineteenth century, in a century which saw few outright pogroms. We look at additional outcomes (Jewish market share in banking and Jewish residential patterns) in online Appendix D.

III. Results Part 1: Changes in the Geography of Anti-Semitism

Our discussion of city-level data is presented in two parts. First, we show how pogroms against Jews changed over the course of six centuries in a basic difference-in-differences setup, probing the robustness of these results with respect to three factors: (i) education; (ii) military conflict; and (iii) city size. We also present results based on various subsamples, different definitions of Protestantism and distinguish between the Lutheran and Calvinist types of Protestantism.

Then, we consider the decades before and after the Reformation by examining anti-Jewish sentiment based on the titles of books printed in Protestant and Catholic cities across Germany to document the geographic shift in anti-Jewish sentiment before and after 1517 in towns with printing presses.

⁵¹In 1890, for the first time, the anti-Semitic party stood for election to the Reichstag and was listed in the sources as *Anti-Semiten*. After being renamed, it was listed in 1893 as *Deutsche Reformpartei (Anti-Semiten)*. In 1898, three anti-Semitic parties ran in the German Empire, but sources report their total vote count as *Anti-Semiten (Deutsche Reformpartei, Anti-Semiten, Christlich-soziale Partei)*.

A. City-Level Data: 1300–1700

Before presenting regression results, Figure 1 displays the pogrom intensity, separately for Catholic and Protestant cities. Panel A uses our main estimation sample, the set of all cities founded before 1500 that had a Jewish community at least once during 1300–1800. Panel B restricts the sample to locations in *Germania Judaica* 2 and 3, i.e., the subset of cities that had a Jewish community at least once before 1519 (the final year covered by *Germania Judaica*, Volume 3). One can note the overall decline, over the centuries, in the pogrom intensity. Note that our theory of changes in complementarities between the Jewish minority and the Christian majority does not explain per se the fact that the number of pogroms is decreasing over time in both cities. We conjecture that this pattern could be explained by two factors: the secular decline in violent crime discussed in Eisner (2003),⁵² and the increasing returns to banking.⁵³

Note that the important exception from this overall downward trend in both samples occurs in Protestant cities during the Reformation century from 1500–1600, where the pogrom intensity goes up and overtakes that in Catholic cities.⁵⁴ The level of pogrom intensity is higher in the lower panel, not surprisingly, as places with a Jewish community before 1519 are more likely to experience pogroms than places where the first Jewish community was founded later. The main sample is larger and gives us more power in our estimations, but we also report results for the restricted GJ2/GJ3 sample and many more samples when presenting regression results.

A similar pattern is described by online Appendix Figures A.3 to A.6, which again describe the location of pogroms every century from 1300–1700. In the centuries 1300–1400, there is not a clear association between pogroms and the religion that the city would adopt in 1546. Instead, in the centuries 1500–1700, pogroms are more concentrated in areas that were Protestant in 1546. This shift in anti-Semitism toward Protestant areas can be clearly see in the data also in the 1700s (see online Appendix Figure A.7) and in the 1800s (see online Appendix Figure A.8), although pogroms are rarer in these last two centuries.

After having taken a first glance at the raw data, we start our regression analysis with a basic difference-in-differences setup:

$$(2) \quad Y_{it} = \alpha \text{Protestant}_i \text{After}_t + X_{it} \beta + \gamma_i + \gamma_t + \varepsilon_{it}.$$

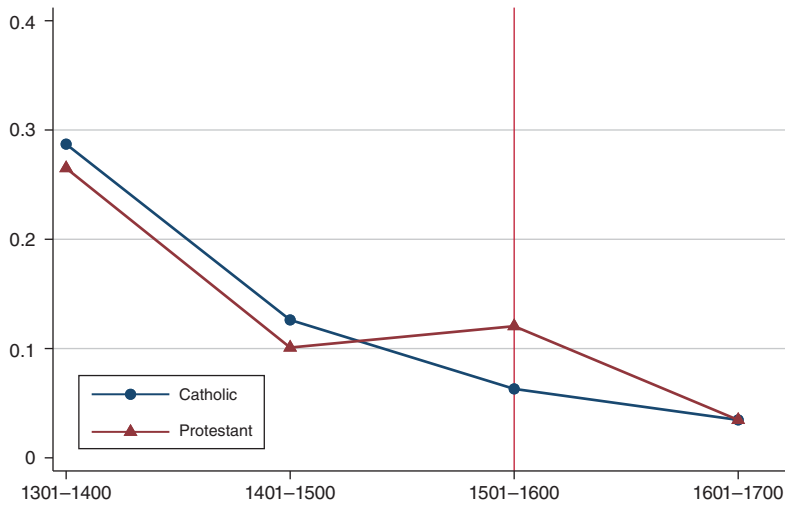
Here, After_t is a dummy variable for the centuries 1500–1600 and later; Protestant_i denotes whether a city was Protestant a few decades after the Reformation in 1546; X_{it} denotes a vector of control variables. Importantly, all regressions use city and century fixed effects. To the extent that there are any unexplained time-constant

⁵²Figure 7 in Eisner (2003) illustrates a strong downward trend in homicide rates in Germany and Switzerland between 1200 and 1900. Loosely speaking, over the centuries, humanity seems to increasingly settle conflicts by less violent means.

⁵³We thank an anonymous referee for suggesting this point.

⁵⁴The originally higher pogrom intensity in Catholic cities is consistent with the idea that larger cities are more likely to experience pogroms and with the fact that, as Cantoni (2015) writes: “cities of the Protestant camp start off smaller in 1300.”

Panel A. 1,298 cities in the Deutsches Städtebuch used in the main regressions



Panel B. 791 cities with a Jewish community before the Reformation

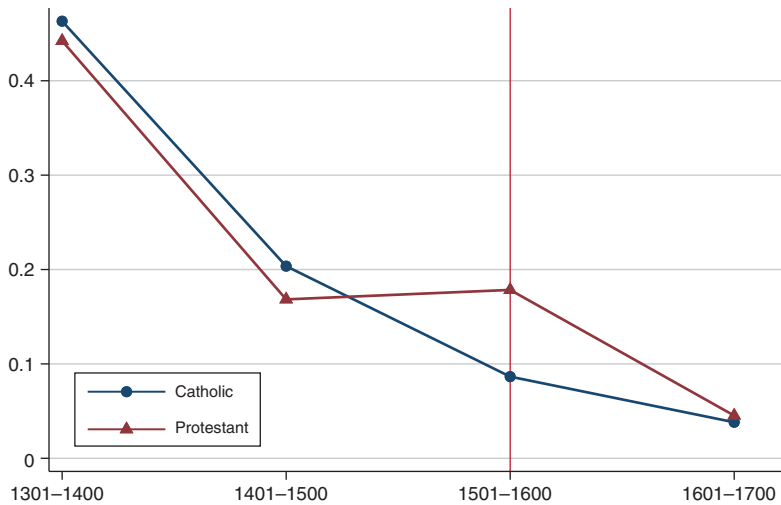


FIGURE 1. SHARE OF CITIES WITH KILLINGS OR EXPULSIONS OF JEWS BY CENTURY

Notes: Data on killings and expulsions of Jews come from *Germania Judaica* (1963–2009) and Aliche (2008). Protestant and Catholic are defined by the denomination of the local ruler in 1546, according to Zeeden (1984).

differences between cities in their likelihood of persecuting Jews, they are addressed by city fixed effects. We cluster standard errors at the city level.

We also estimate, in robustness checks, a more flexible specification that allow us to test for pre-trends in anti-Semitism and that takes the following form:

$$(3) \quad Y_{it} = \sum_{t=1400}^{1600} \alpha_t Protestant_i + X_{it}\beta + \gamma_i + \gamma_t + \varepsilon_{it},$$

where we let the coefficient on the interaction term vary over the centuries. It is important to note that in this specification, the estimated coefficient α_t must be measured relative to a baseline time period, which we take to be the fourteenth century.

Table 1 displays the results of regressions based on equation (2). Column 1 shows the *Protestant_tAfter_t* coefficient when no control variables are included. Here, the centuries 1300–1400 and 1400–1500 are combined as pre-Reformation, and the centuries 1500–1600 and 1600–1700 are combined into a post-Reformation period. The coefficient of 0.052 implies that Protestant cities, compared with Catholic cities, are 5.2 percentage points more likely to experience a pogrom after the Reformation relative to before. The difference of 5 percentage points is quite substantial considering that the average frequency of city-by-century observations with anti-Semitic acts is 13 percent. In column 2, we control for a time-varying indicator for Jewish presence in the city as well as its interaction with a post-Reformation dummy, which barely changes the coefficient on the main effect of interest.⁵⁵ In columns 3 and 4 we show results in the sample of cities in GJ2/GJ3 (i.e., cities in which there is evidence of Jewish presence before the Reformation). Estimated coefficients are, if anything, larger, but we note also the larger standard errors in the smaller sample. It is important to note that in both the main sample, and the GJ2/GJ3 sample, we check for pre-trends to see whether the adoption of the Reformation might be endogenous to differential trends in anti-Semitism before the Reformation. Our results indicate that this is not the case.

Having noticed that results in the main sample are, if anything, more conservative than in the GJ2/GJ3 sample, we present further results in the main sample, where we probe robustness to confounding factors. Column 5 examines the importance of education, which *ex ante* could go both ways. To the extent that increased human capital investment allows Protestants to enter high-skilled occupations such as moneylending, these human capital investments alone might explain the shift in anti-Semitism toward Protestant areas. Controlling for the presence of a school and its interaction with a post-Reformation dummy, the main difference-in-differences coefficient remains unaffected. This finding does not contradict Botticini and Eckstein, whose focus is on the role of human capital investments in driving Jewish occupational specialization. Our results merely show that schooling *alone* does not explain the change in geographic patterns of anti-Semitism that followed the Reformation.

Anti-Semitism might also differ according to the degree of involvement of cities in war activities. In column 6, we control for different indicators of war activity in which a city is involved, and their interaction with a post-Reformation dummy. Although some indicators of war activities (and their interaction) affect the pogrom intensity, the main difference-in-differences coefficient remains unaffected.

In column 7, we control for 11 intervals of population size, starting at 5,000 inhabitants, and we group city-century observations with less than 5,000 inhabitants in the [0, 5,000] reference interval.⁵⁶ As in previous columns, we also use interaction

⁵⁵The presence of a Jewish community is no prerequisite for a pogrom because Jews may be persecuted while traveling through a city. This case is, however, a rare exception.

⁵⁶We lose a few observations for Bairoch cities with missing data in some years. Furthermore, the *Städtebuch* has population data for an unbalanced panel making up less than one-half of all city-by-century observations. We experimented with the population data in the *Städtebuch* and results are robust to the inclusion of population variables from this source (see Becker and Pascali 2016).

TABLE 1—ANTI-SEMITISM BEFORE AND AFTER PROTESTANT REFORMATION: MAIN RESULTS

Dependent variable:	Pogrom happened in the century						
	Main sample		Cities with Jews < 1500		Main sample		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Protestant × After	0.052 (0.018)	0.050 (0.017)	0.078 (0.027)	0.071 (0.026)	0.049 (0.018)	0.050 (0.018)	0.051 (0.018)
Jewish presence		0.331 (0.015)		0.403 (0.021)			
After × Jewish presence		−0.132 (0.017)		−0.038 (0.035)			
School					0.141 (0.025)		
Post × school					−0.186 (0.025)		
Battle near the city						0.075 (0.053)	
City besieged						0.214 (0.128)	
City sacked						0.034 (0.053)	
City partially destroyed						0.004 (0.045)	
City completely destroyed						0.158 (0.088)	
City occupied						0.093 (0.050)	
City involved in war						−0.041 (0.052)	
After × battle near the city						−0.072 (0.085)	
After × city besieged						−0.132 (0.135)	
After × city sacked						−0.065 (0.068)	
After × city partially destroyed						0.004 (0.049)	
After × city completely destroyed						−0.157 (0.096)	
After × city occupied						−0.107 (0.058)	
After × city involved in war						−0.061 (0.059)	
Century dummies	✓	✓	✓	✓	✓	✓	✓
Population brackets (and interaction with After)							✓
Mean dep. var.	0.129	0.129	0.203	0.203	0.129	0.129	0.114
R ² (within)	0.104	0.190	0.194	0.267	0.122	0.114	0.128
Observations	5,192	5,192	3,164	3,164	5,192	5,192	4,588
Number of cities	1,298	1,298	791	791	1,298	1,298	1,178

Notes: The table reports estimates from panel regressions with city fixed effects. The sample period is 1300–1700. The unit of observation is city by century. *After* (*Reformation*) is a dummy variable for the centuries 1500–1600 and later. *Protestant* is a dummy variable that identifies cities that had a Protestant ruler in 1546. Standard errors (reported in parentheses) are clustered at the city level.

terms of the population controls with a post-Reformation dummy. The point estimate in the first row remains the same.^{57,58}

While these specifications serve as stress tests for our difference-in-differences specifications, they also carry the risk of introducing “bad controls.”⁵⁹ We even experimented with the inclusion of all control variables used in columns 5 through 7 at once and results still hold. Still, a purist reader would concentrate on regressions without such controls. Other readers will find results from these exercises reassuring. The main difference-in-difference coefficient in column 5 through 7 is positive and significant throughout with magnitude very similar to columns 1 and 2.

Further Robustness Checks.—The online Appendix tables show various robustness checks.

Testing for Pre-Trends: Online Appendix Table A.6 uses equation (3) to test for the absence of pre-trends, using 3 different estimation samples: columns 1 and 2 use the main sample; columns 3 and 4 use the sample of cities covered in GJ2/GJ3; columns 5 and 6 use the complete sample of all 2,344 cities covered in the *Städtebuch*, i.e., with no sample restriction whatsoever. Odd-numbered columns only use the three century-specific Protestantism coefficients, whereas even-numbered columns control for a time-varying indicator for the presence of a Jewish community and its interaction with a post-Reformation dummy. In all three samples, there is no evidence of pre-trends, as measured by the coefficient for the fifteenth century (with the fourteenth century being the reference century). Post-Reformation effects are stronger in the sixteenth century than in the seventeenth century, which is not surprising given the secular decline in pogroms. Remember that below, nineteenth century data give a complementary perspective using voting for anti-Semitic parties as a way to measure anti-Semitic attitudes.

Different Subsamples: First, online Appendix Table A.7 shows results using the set of all cities in Keyser’s *Städtebuch*, including those in East Prussia and those in which there was no evidence of Jewish presence, for a total of 2,344 cities. Results are similar. Online Appendix Table A.8 uses the main sample of 1,298 cities but, one by one, excludes each of the 18 regions in the *Städtebuch* and modern-day Poland to probe robustness to outlier regions.

Different Definitions of Protestantism: Our main analysis uses an indicator for Protestant *ruler* in 1546, based on Zeeden (1984). Alternatively, online Appendix Table A.9 shows results when we use an indicator for Protestant *ruler* in 1650, also based on Zeeden. This indicator accounts both for late adoption of Protestantism as well as for Protestant cities returning to Catholicism during the Counter-Reformation. Results are very similar to those in the main analysis, suggesting that they are not driven by a potentially arbitrary coding of Protestantism. An alternative use of the

⁵⁷ Online Appendix Table A.4 shows the equivalent of columns 5 through 7 for the set of GJ2/GJ3 cities.

⁵⁸ Online Appendix Table A.5 applies Conley standard errors that are also robust to spatial correlations. Since they barely differ from standard errors clustered at the city level, we proceed with city-level clustering in the paper.

⁵⁹ “Bad controls” may affect coefficient stability, in particular in IV estimation.

information on Protestantism in 1546 and 1650 is to exclude all regions returning to Catholicism between 1546 and 1650 and to focus exclusively on ultimately-Protestant and always-Catholic regions. We would expect results in this sample to be more pronounced because cities that return to Catholicism might never have been as committed to Protestant doctrine as those that remain Protestant after adoption. In fact, online Appendix Table A.10 shows that coefficient estimates here are somewhat larger.

Influential Pogrom Waves?: We also show that results are not driven by a particular wave of pogroms: In online Appendix Table A.12, we exclude five different pogrom waves before the Reformation, most prominently the Black Death pogroms in 1347/1350 (column 2). We also exclude one-by-one the centuries before the Reformation. Results are robust also here.

In Summary.—We take the results in Table 1 and the numerous robustness checks in the online Appendix as evidence that the Reformation brought about a change in the geography of anti-Semitism, namely, a shift toward Protestant cities. This shift is not explained by pre-trends or different location patterns of Jews before or after the Reformation or by education, war activities, or population size.

We argue that this shift is likely related to economic factors, and we document this in Section IV. However, before doing so, we want to further examine the Reformation period to determine whether the shift in anti-Jewish sentiments developed slowly over time or could be seen quite soon after 1517. To this end, we use book titles from which we infer anti-Jewish content.

B. *Anti-Jewish Content in Book Titles: 1451–1600*

The decades before and after the Reformation are particularly interesting. Although our main analysis covers several centuries, we also pay particular attention to the decades around the Reformation. In particular, for the decades from 1451 to 1600, we capture city-level anti-Jewish attitudes using new data on the anti-Jewish books that were published in the city.

The underlying assumption is that the books printed in a city measure not only the willingness of a printer to print them, but also the supply of and demand for content in that city. Previous studies have highlighted large shipping costs on books in German regions in the Reformation years.⁶⁰ To be on the conservative side, however, our benchmark sample excludes cities with a different religion compared to their surrounding territories.

The use of anti-Jewish publications as a measure of anti-Semitism has several closely related advantages with respect to using the incidence of pogroms. First, pogroms, though terrible, do not occur at very high frequency.⁶¹ Anti-Jewish

⁶⁰Dittmar (2011) cites Edwards (1995), who observes, “If, for example, there was an interest in Strasbourg for a work first published in Wittenberg, it was more common for a printer in Strasbourg to reprint the work than it was for the printer in Wittenberg to ship a large number of copies [500 kilometers] to Strasbourg.” Dittmar provides additional evidence that in a first approximation, equating the place of print with local demand for these books is an acceptable assumption.

⁶¹Only one-quarter of the cities in the full sample documented at least one episode of killings or expulsions of Jews over the years 1300–1900.

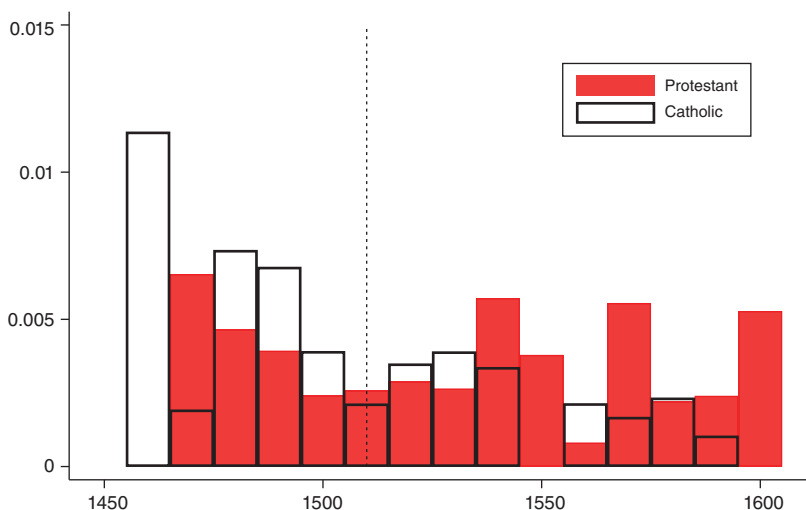


FIGURE 2. SHARE OF BOOKS WITH ANTI-JEWISH TITLES

Notes: The figure displays the share of books with anti-Jewish titles. The data source is the Universal Short Title Catalogue (USTC). Book titles are classified as anti-Jewish using a naïve Bayesian classifier (see main text for details). Sample is set of cities with at least ten printed editions in the period 1450–1600. Religion of ruler in 1546 following Zee den (1984) defines cities as Protestant or Catholic.

sentiment is likely to be around even when no pogrom occurs, and a higher data frequency allows us to zoom in closer to the Reformation and to capture underlying sentiment. Second, if a city is heavily anti-Semitic this might result in the absence of a Jewish community, which makes Jewish persecution less likely in the city. Third, Jewish persecution reflects both the intensity of anti-Semitism but also the balance of power between Jews and Christians, so it's not a pure measure of "sentiments."

The measure also comes with disadvantages. First, book titles might not capture anti-Jewish sentiments of the population at large, but rather that of some literate elite. Second, books are published in a smaller number of cities: instead of more than 1,000 cities, we have just above 100 cities with at least 1 print edition. Despite these caveats, we consider the results on book titles as relevant additional evidence on anti-Semitism at higher frequency.

Before turning to regression analysis of the data described in Section IIB, we give a graphical presentation of the data. Figure 2 shows the share of books with anti-Jewish titles published in Protestant versus Catholic cities.⁶² Two interesting patterns are captured by this figure. First, in the decades preceding the Protestant Reformation, there is a reduction in the share of anti-Jewish books, which mirrors the reduction in anti-Jewish violence that we have already documented in Figure 1. Moreover, exactly as in Figure 1, before the Reformation, we observe a more anti-Jewish culture in cities that stayed Catholic after the Reformation compared to cities that turned Protestant. Once the Reformation starts, this general decline in the share of anti-Jewish publications continues in Catholic cities but not in Protestant

⁶² Online Appendix Figures A.12 and A.13 show the number of cities publishing (any) anti-Jewish book editions and the number of anti-Jewish book editions published in each decade, separately for Catholic and Protestant cities.

TABLE 2—ATTITUDES TOWARD JEWS BEFORE AND AFTER PROTESTANT REFORMATION: USTC DATA

Estimation method: Dependent variable:	Poisson model with FE		Panel FE		Panel FE	
	Number of anti-Jewish titles		Share anti-Jewish titles		Median prob. anti-Jewish title	
	(1)	(2)	(3)	(4)	(5)	(6)
Protestant × after Reformation	1.390 (0.722)	1.072 (0.546)	0.003 (0.002)	0.003 (0.002)	0.036 (0.015)	0.031 (0.017)
Number of books		0.003 (0.000)		0.000 (0.000)		0.000 (0.000)
Decade dummies	✓	✓	✓	✓	✓	✓
Mean dep. var.	0.314	0.314	0.003	0.003	0.023	0.023
R ² (within)			0.037	0.056	0.454	0.483
Observations	528	528	523	523	1,456	1,456

Notes: The table reports estimates from panel regressions with city fixed effects, as indicated in column header. The sample period is 1450–1600. The unit of observation is city by decade. Estimates in columns 3 through 6 are weighted by the total number of books printed in a city during the sample period. *After (Reformation)* is a dummy variable equal to 1 for decades starting with the decade 1510–1519. Cities which have a different religion with respect to the surrounding territory are excluded. Standard errors (reported in parentheses) are clustered at the city level.

cities: as a result, starting from the 1540s, the share of anti-Jewish publications is generally higher in Protestant cities compared to Catholic cities.

Using the equivalent of equation (2) at the decade level and restricting the sample to printing cities with books documented in the USTC, we can estimate whether there was a geographic pattern in anti-Jewish printing before and after the Reformation, whereby Protestant cities developed differently from Catholic cities. Note that in this exercise, we define “post-Reformation” as 1510 onward. Although the Reformation took place in 1517, the decade from 1510 to 1519 is the decade of the Reformation, and the printing of Luther’s works started within this decade. The results are robust to defining the post-Reformation period as 1520 onward.

Table 2 displays the results of this analysis. In the first column, the dependent variable is the number of books. We run Poisson regressions to take into account the fact that it is a count variable. As expected, the estimated α is positive and statistically significant; its estimated value reflects the fact that, following the Reformation, the number of anti-Jewish books published in Protestant cities is four times higher ($\exp(1.390) = 4.103$) relative to what happened in Catholic cities.⁶³ In the second column, we take the more conservative approach of controlling for the overall printing activity of a city.⁶⁴ The estimated α is still positive and statistically significant, although one-third smaller. The following two columns reports weighted least square regressions, in which the dependent variable is the share of books with

⁶³ Although the results in column 1 consider the set of cities that had at least 10 book editions until the year 1600, online Appendix Table A.13 considers different subsamples. It shows that results are robust when restricting the analysis to cities with at least 1, 5, 25, 50, and 100 printed books.

⁶⁴ We would like to stress, however, that one could well argue for not controlling for overall printing activity, i.e., that what is displayed in online Appendix Figures A.12 and A.13 is the relevant exercise. The argument against controlling for the total number of books is that the mere existence of a larger number of books with anti-Jewish titles means that the ideas expressed therein are diffusing in a city, independently of how much other material on various topics is around. The counter-argument is that with more books around overall, anti-Semitic titles are not as salient.

anti-Jewish titles.⁶⁵ Observations are weighted by the number of books published in the city because the share of anti-Jewish books is a more precise measure of anti-Jewish attitudes in cities that published a larger number of book editions.⁶⁶ Again, the estimated α is still positive and statistically significant independently of whether we control for the number of published books in the city. Following the Protestant Reformation, the share of anti-Jewish books published in Protestant cities is higher by approximately 0.3 percentage points relative to what happened in Catholic cities. This is a large effect when considering that the average share of anti-Jewish books in the benchmark sample is 0.3 percent.

Since Protestantism was a new religious creed, it is plausible that much more was written by theologians to debate the foundations of this new religion compared to what would be written in Catholic cities. Moreover, Dittmar and Meisenzahl (2018) show that the Reformation came with new city ordinances establishing the Protestant religion in local laws. To ensure that our results are not simply picking up a temporary surge in either the religious or the legal literature, in online Appendix Table A.15 we perform an analysis similar to Table 2, but additionally controlling for the number of books published with religious or legal contents. Results are robust.

Until now, we worked with books that are classified as anti-Jewish using an arbitrary cutoff on the probability of being anti-Jewish attached by the Bayesian classifier. Alternatively, we could use the probability attached to *each* book of being anti-Jewish. More specifically, in the last two columns of Table 2, the dependent variable is the median probability of books being anti-Jewish among all books published in a city. Again, following the Reformation, there is a relative increase in books that have a higher probability of being anti-Jewish in cities that became Protestant compared to cities that remained Catholic.

Remember that in all regressions of Table 2, we drop cities that have a different religion with respect to the surrounding territory. In online Appendix Table A.16, we repeat the analysis including these cities, which are almost exclusively Protestant cities in Catholic territories. It turns out that these cities do not behave in a way that is statistically different than Catholic cities, once the Reformation starts. This is in line with the idea that in the additional cities entering the sample, printers provide for a split market, reducing differences between Protestant and Catholic cities.

Finally, in online Appendix Table A.17 we show that our main results are robust when we control for a time-varying indicator for the presence of a school and its interaction with a post-Reformation dummy. This is once more suggestive that our regressions are not just capturing higher levels of literacy among the Protestants.

To conclude, the difference-in-differences regressions presented in this subsection indicate a shift in anti-Jewish sentiment, following the Reformation, toward

⁶⁵Notice that the number of observations decreases from 1,456 to 523. For 933 city-by-decade observations, there are no books published and, therefore, shares of anti-Jewish books are not defined.

⁶⁶One-quarter of the cities in the benchmark sample published less than 40 book editions in the 150 years of data and none of these cities published any book classified as anti-Jewish. This is arguably not an indicator of a higher level of tolerance toward the Jews, but rather the result of having so few books published. More generally, the share of anti-Jewish books published in a city is going to be a very noisy indicator of anti-Jewish attitudes in cities with limited printing activity. The weighted least squares estimates will correct for the fact that the variance of the error term is higher in these cities. Unweighted regressions are reported in online Appendix Table A.14. As can be seen, the sign of the estimated α stays the same as in Table 2 and the magnitude of the estimated coefficient is similar. However, standard errors are larger and the estimated α is not statistically significant.

Protestant cities. Figure 2 and online Appendix Figures A.12 and A.13 support the hypothesis that this simple pre-post Reformation difference-in-differences setup does not identify a secular trend but rather a genuine shift in anti-Semitism after the Reformation.

C. Persistence: The Long Shadow of the Reformation

City-Level Data over Six Centuries: 1300–1900.—We pursue several ways to show that effects of the Protestant Reformation on anti-Semitism are persistent. While our previous analysis on the city-level dataset uses two centuries before and after the Reformation (1300–1700), we can extend the sample period to go until 1900. Table 3 confirms that our results are still valid when we do so.

The Emergence of Anti-Semitic Parties in the Late Nineteenth Century: County-Level Data.—A second way to show persistence is to turn to county-level Prussian census data from the late nineteenth century. While pogroms against Jews are rare in the nineteenth century, Prussian county-level data allow us to construct an alternative measure of anti-Semitism in the form of votes cast for anti-Semitic parties. We use election data from elections to the Lower House of the German Empire parliament (the *Reichstag*) where, starting in 1890, anti-Semitic parties ran for parliament. Online Appendix Figures P.1, P.2, and P.3 display the regional distribution of votes for anti-Semitic parties in 1890, 1893, and 1898.

We note here that in the 1880s and 1890s, usury laws on Catholics had already been revoked and that, formally, Jews had equal rights to the majority population.⁶⁷ This makes it likely that what we measure with the Prussian data is a reflection of a change in anti-Jewish sentiments, that followed the Reformation and persisted to the end of the nineteenth century.

We run cross-sectional regressions as follows:

$$(4) \quad Y_i = \alpha + \beta \text{ShareProtestant}_i + X_i \gamma + \varepsilon_i.$$

In Table 4, we look at election results for anti-Semitic parties. Every coefficient displayed in panels A and B stems from separate ordinary least squares (OLS) regressions for elections to the Reichstag in 1890, 1893, and 1898. Because anti-Semitic candidates did not stand in all electoral precincts, we run regressions on the vote share of anti-Semitic candidates as well as on binary indicators of whether anti-Semitic candidates stand in a precinct. Note that in all regressions, we cluster by electoral precinct because electoral precincts are typically composed of two or three counties. All regressions show positive coefficient estimates that are statistically significant in nearly all specifications. For instance, in the 1898 elections to the Reichstag, anti-Semitic parties are 24.7 percentage points more likely to stand in all-Protestant counties compared to all-Catholic counties. The cross-sectional nature of the data

⁶⁷ As noted in footnote 31, around 1830, the Church had given up on persecuting usurers, although it did not formally revoke the usury doctrine. Furthermore, during the late eighteenth and early nineteenth century, calls for Jewish emancipation were followed by edicts declaring Jews to be citizens with “equal rights.” For example, on March 11, 1812, the Prussian King Frederick William III issued an edict that declared all legally resident Jews of Prussia to be citizens.

TABLE 3—LONGER PANEL FOR THE YEARS 1300–1900

Dependent variable:	Pogrom happened in the century						
	Main sample		Cities with Jews < 1500		Main sample		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Protestant × After	0.037 (0.017)	0.036 (0.015)	0.053 (0.025)	0.051 (0.023)	0.031 (0.016)	0.038 (0.017)	0.045 (0.016)
Century dummies	✓	✓	✓	✓	✓	✓	✓
Jewish presence (and interaction with After)		✓		✓			
Presence of school (and interaction with After)					✓		
Military conflicts (and interaction with After)						✓	
Population brackets (and interaction with After)							✓
Mean dep. var.	0.089	0.089	0.138	0.138	0.089	0.089	0.078
R ² (within)	0.133	0.227	0.241	0.298	0.154	0.142	0.153
Observations	7,788	7,788	4,746	4,746	7,788	7,788	7,106
Number of cities	1,298	1,298	791	791	1,298	1,298	1,260

Notes: Main sample of cities in the Deutsches Städtebuch. The sample period is 1300–1900. The table reports estimates from panel regressions with city fixed effects. The unit of observation is city by century. *After* (*Reformation*) is a dummy variable for the centuries 1500–1600 and later. *Protestant* is a dummy variable that identifies cities that had a Protestant ruler in 1546. Standard errors (reported in parentheses) are clustered at the city level.

makes a causal interpretation of the link between Protestantism and the various outcomes less obvious. Becker and Woessmann (2009) propose distance to Wittenberg as an instrument for the share of Protestants in nineteenth century Prussia, exploiting the concentric spread of the Reformation from Wittenberg, the birthplace of the Lutheran variant of Protestantism.⁶⁸ The first-stage equation, complementing equation (4) is the following:

$$(5) \quad \text{ShareProtestant}_i = \delta + \theta \cdot \text{distWittenberg}_i + X_i\eta + \vartheta_i.$$

Using the exogenous variation in the share of Protestants generated by distance to Wittenberg,⁶⁹ we confirm the positive effect of the share of Protestants on anti-Semitic vote shares (see panel C).

The Very Long Shadow: The Reformation and the Persecution of Jews under the Nazis.—Voigtländer and Voth (2012) document an exceptional geographic persistence in patterns of anti-Semitism, showing that German cities that experienced anti-Jewish pogroms in 1349 were more likely to experience higher anti-Semitism during the Weimar Republic. Specifically, anti-Semitism in the interwar period is

⁶⁸Online Appendix D elaborates further on commonalities and differences in identification strategies in the city-level panel and the cross-sectional data at the county level for nineteenth century Prussia.

⁶⁹Column 1 of online Appendix Table P.5 shows the first-stage results without controls. The share of Protestants falls rapidly with distance to Wittenberg: every 100 km of distance to Wittenberg is associated with a drop of 9.2 percentage points in the share of Protestants. See online Appendix Table P.5, column 3, for first-stage results with controls.

TABLE 4—PROTESTANTISM AND ANTI-SEMITIC PARTIES

	Share anti-Semitic parties			Running in elections		
	1890 (1)	1893 (2)	1898 (3)	1890 (4)	1893 (5)	1898 (6)
<i>Panel A. OLS (No controls)</i>						
Share of Protestants (1882)	0.0212 (0.011)	0.0406 (0.013)	0.0720 (0.017)	0.0788 (0.051)	0.136 (0.072)	0.247 (0.080)
Mean dep. var.	0.013	0.028	0.041	0.097	0.305	0.303
R ²	0.011	0.029	0.054	0.010	0.012	0.041
Observations	452	452	452	452	452	452
	(7)	(8)	(9)	(10)	(11)	(12)
<i>Panel B. OLS with controls</i>						
Share of Protestants (1882)	0.0383 (0.018)	0.0638 (0.018)	0.109 (0.025)	0.0802 (0.069)	0.233 (0.092)	0.311 (0.089)
Controls [‡]	✓	✓	✓	✓	✓	✓
Mean dep. var.	0.013	0.028	0.041	0.097	0.305	0.303
R ²	0.109	0.120	0.196	0.046	0.085	0.157
Observations	452	452	452	452	452	452
	(13)	(14)	(15)	(16)	(17)	(18)
<i>Panel C. IV with controls</i>						
Share of Protestants (1882)	0.0282 (0.017)	0.0994 (0.032)	0.131 (0.038)	0.208 (0.112)	0.453 (0.182)	0.598 (0.192)
Controls	✓	✓	✓	✓	✓	✓
Mean dep. var.	0.013	0.028	0.041	0.097	0.305	0.303
Cragg-Donald Wald <i>F</i> -statistic	111.315	111.315	111.315	111.315	111.315	111.315
Kleibergen-Paap rk Wald <i>F</i> -statistic	54.894	54.894	54.894	54.894	54.894	54.894
Observations	452	452	452	452	452	452

Notes: The table reports OLS estimates in panels A and B. Panel C reports IV estimates, where the share of Protestants in 1882 is instrumented with distance to Wittenberg. The unit of observation is the Prussian county in 1882. Standard errors (reported in parentheses) are clustered at the precinct level.

Controls: share age below 10, share females, share born in municipality, share of Prussian origin, average household size, ln(population size), Posen dummy, share of county population in urban area.

measured as (i) pogroms in the 1920s, (ii) attacks on synagogues in 1938, (iii) deportations of Jews after 1933, (iv) anti-Jewish letters to the editor of the Nazi newspaper *Der Stürmer*, (v) votes for the DVFP in 1925 (a radical anti-Semitic party), (vi) votes for the NSDAP in 1928, (vii) a principal component of the previous measures.

In online Appendix Table A.19, we use their same data to show the persistence of the change in the geography of anti-Semitism caused by the Reformation all the way to the twentieth century.⁷⁰ We run a two-period difference-in-differences regression. Each column corresponds to a different measure of anti-Semitism in the 1920s–1930s. The main regressor is the interaction between the indicator for whether the city was Protestant in 1546 and the After Reformation dummy.

We find similar evidence as in our larger panel: the Reformation caused an increase in various alternative measures of anti-Semitism, including Nazi vote shares. However, systematic differences in anti-Semitism across Protestant and Catholic cities in the later phase of the Weimar Republic also reflect strong opposition of Catholic

⁷⁰We are grateful to Joachim Voth and Nico Voigtländer for sharing their data with us. Online Appendix Table A.18 shows descriptive statistics for this dataset.

dignitaries against the Nazis (Deuerlein 1963, Spenkuch and Tillmann 2018), while Protestant regions were strongholds of the NSDAP (Fandel 2002).

In Summary.—We interpret the results in this subsection as evidence that the Protestant Reformation still casts its shadow on anti-Semitism until the onset of the twentieth century.

IV. Results Part 2: Evidence for the Economics of Anti-Semitism

The estimates reported so far show that pogroms against Jews and anti-Jewish publications increased in Protestant areas relative to Catholic areas following the Protestant Reformation. How is this result related to the change in the complementarities between the Jewish minority and the majority population induced by the Reformation?

A. City-Level Data: 1300–1700

To answer this question, we first return to the sample of cities from the *Städtebuch* used in Section IIIA. Before presenting the regression results, we illustrate the main result in Figure 3. This figure repeats the same analysis of Figure 1 (showing the evolution of pogrom intensity in Catholic versus Protestant cities from 1300–1700), for the main sample (panel A), and for cities with a Jewish community before the Reformation (panel B). It splits the sample depending on whether cities were specialized in trade and trade-related services before the Reformation (left-hand side) or not (right-hand side). For the binary sample split in this figure, we code a city to be a trade city if at least half of the industries, reported in the *Städtebuch* to define its main economic activities for the pre-Reformation period, can be classified as trade-related.

If we look at Catholic cities, we observe a similar decline in pogroms between trade and non-trade cities. For cities that became Protestant, as highlighted already in Figure 1, pogroms increased in the century of the Reformation. The novelty that comes from Figure 3 is that this increase was four times larger in trade cities compared to non-trade cities. Among trade cities, cities that became Protestant used to be considerably less anti-Semitic toward the Jews before the Reformation compared to cities that stayed Catholic and became consistently more anti-Semitic in the century of the Reformation and in the following ones. Among non-trade cities, cities that became Protestant experienced a much smaller increase in anti-Semitism in the century of the Reformation, while anti-Semitism came back to the mean level in the following centuries. The figure also illustrates that there are no pre-trends explaining this result and that there is, instead, a genuine break in the exact century of the Reformation for Protestant cities (that is more pronounced in pre-Reformation trade cities).

We interpret this evidence as supporting the view that trade cities had larger returns to banking and a higher “need” for Jews compared to the other cities in pre-Reformation Germany. There can be potentially two reasons: first, the Jews were the only group formally allowed to lend money for a profit; second, as argued by Botticini and Eckstein, the Jews were traditionally exceptionally literate and

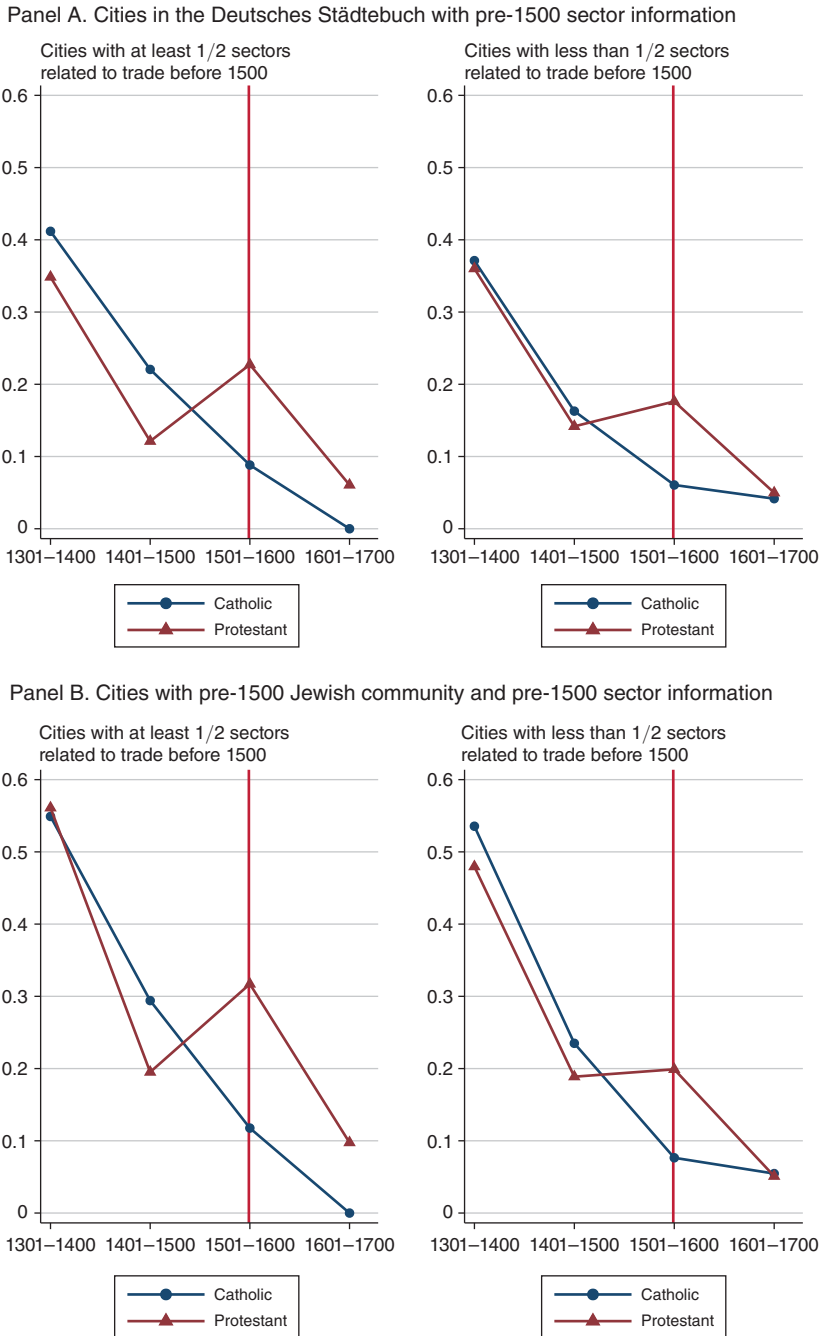


FIGURE 3. SHARE OF CITIES WITH KILLINGS OR EXPULSIONS OF JEWS BY CENTURY: TRADE CITIES VERSUS NON-TRADE CITIES

Notes: Sample of cities with pre-1500 sector information in the Deutsches Städtebuch. Data on killings and expulsions of Jews come from *Germania Judaica* (1963–2009) and Alicke (2008). Protestant and Catholic are defined by the denomination of the local ruler in 1546, according to Zeeden (1984). Information about salient sectors is drawn from the Deutsches Städtebuch.

TABLE 5—ANTI-SEMITISM BEFORE AND AFTER PROTESTANT REFORMATION: TRADE VERSUS NON-TRADE CITIES

Dependent variable:	Pogrom happened in the century							
	Cities with pre-1500 sector information				Main sample			
	And <50% trade	And ≥50% trade	Cities with pre-1500 sector information		Without evidence of trade	With evidence of trade	Main sample	
Sample:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Protestant × After	0.078 (0.033)	0.181 (0.061)	0.078 (0.033)	0.075 (0.033)	0.022 (0.019)	0.149 (0.046)	0.022 (0.019)	0.025 (0.019)
Protestant × (pre-1500 trade ≥ 50%) × After			0.103 (0.069)	0.138 (0.067)				
Protestant × (any pre-1500 trade) × After							0.127 (0.050)	0.137 (0.050)
Century dummies	✓	✓	✓	✓	✓	✓	✓	✓
Population brackets (and interaction with After)				✓				✓
Mean dep. var.	0.170	0.185	0.173	0.154	0.109	0.206	0.129	0.114
R ² (within)	0.145	0.172	0.150	0.188	0.083	0.196	0.114	0.131
Observations	2,100	536	2,636	2,214	4,116	1,076	5,192	4,588
Number of cities	525	134	659	578	1,029	269	1,298	1,178

Notes: The table reports estimates from panel regressions with city fixed effects. The sample period is 1300–1700. The unit of observation is city by century. *After (Reformation)* is a dummy variable for the centuries 1500–1600 and later. *Protestant* is a dummy variable that identifies cities which had a Protestant ruler in 1546. Columns 1 through 4 are for the set of cities with pre-1500 sector information in the *Deutsches Städtebuch*. Columns 5 through 8 are for the main sample. Standard errors (reported in parentheses) are clustered at the city level.

skilled compared to the majority population. The Protestant Reformation could have reduced the “need” for Jews especially in trade cities by softening the ban on Christian usury and by promoting a good public education system.

After having taken a first glance at the raw data, we start our regression analysis. In Table 5, we first divide the sample depending on whether cities were specialized in trade and re-estimate equation (2).

Here we use two different ways to define a pre-Reformation trade city. In columns 1–4, trade cities are defined as in Figure 3, as the cities with at least half of industry entries in trade and trade-related sectors. In columns 5–8, trade cities are defined as cities with at least one industry entry in trade or trade-related sectors. Each definition has pros and cons. The first one is likely to be a better measure of the specialization of the city before the Reformation, as it is designed to capture the relative size of the trade sector in the economy compared to the remaining sectors (mainly agriculture and manufacturing). The limitation of this measure, however, is that it cannot be computed for those cities that do not have detailed industry data before the Reformation (approximately one-half of our benchmark sample).

As can be seen, there is an increase in anti-Semitism in Protestant cities relative to the Catholic ones, but the shift is stronger in trade cities (columns 2 and 6), whereas the shift is smaller in the rest of the sample (columns 1 and 5). Specifically, the estimated difference-in-differences coefficient is more than twice as large in cities with more than half of the industry entries in trade compared to all other cities with at least 1 industry entry (columns 1 and 2) and is more than 6 times larger in cities with at least 1 entry in trade compared to cities with no entry in trade (columns 5 and 6).

A more rigorous way to illustrate the differential impact of the Protestant Reformation on anti-Semitism in places in trade versus non-trade cities is to estimate the difference-in-difference-in-differences specification reported in the following equation:

$$(6) \quad Y_{it} = \alpha_0(\text{Protestant}_i \cdot \text{After}_t \cdot \text{TradeCity}_i) + \alpha_1(\text{After}_t \cdot \text{TradeCity}_i) \\ + \sum_{t=1400}^{1600} \alpha_t^P \text{Protestant}_i + X_{it}A + \alpha_i + \alpha_t + \varepsilon_{it},$$

where TradeCity_i is a dummy variable that identifies trade cities before the Reformation. The second term on the right-hand side controls for differences in the evolution of anti-Semitism between Protestant and Catholic places, whereas the third one controls for differences in the evolution of anti-Semitism between trade and non-trade cities. Finally, we control for city- and century-fixed effects, and X is a vector of additional control variables.

We would like to emphasize that we do not assume that the cities that were specialized in trade before the Reformation are randomly distributed. These cities are different across several dimensions (e.g., they are larger and host larger Jewish communities), which might have an impact on anti-Jewish sentiments and acts. We account for this by controlling for city-fixed effects and for the interaction between the TradeCity_i dummy and century-fixed effects. Columns 3, 4, 7, and 8 report the estimated coefficients on the triple interaction: they are positive in all columns and range from 0.10 to 0.14 (statistically significant in 3 out of 4 cases). In column 3 (4), the estimated coefficient is 0.10 (0.13) and it reflects the difference between the diff-in-diff coefficients reported in columns 1 and 2 (resp., columns 5 and 6). Estimated coefficients become slightly larger when we add the population controls (columns 4 and 8), indicating that city size is not driving our estimates.

We do not take a stand on whether the Protestant Reformation led to increased competition between the Christian majority and Jewish minority through a decreasing importance in the original usury ban or through an increasingly educated Protestant population. Still, it seems that the increasing competition between the Protestants and the Jews was mainly manifested in moneylending rather than in other skill-intensive sectors. We illustrate the intuition behind this conclusion in online Appendix Figure A.14. The figure shows the evolution of pogrom intensity in Catholic versus Protestant cities, and it splits the sample between cities with and without evidence of Jewish lending before the Reformation. As can be seen, there is an increase in pogroms intensity in Protestant cities in the aftermath of the Reformation relative to Catholic cities, but only in cities in which the Jews used to be moneylenders. There is virtually no change in anti-Semitism between Catholic and Protestant cities in which the Jews were not moneylenders. This result, again, is not explained by pre-trends.⁷¹

⁷¹The level of pogroms in the fourteenth and fifteenth century is higher in cities in which there is evidence of Jewish lending before 1500. This result, which might seem at odds with our theory, is explained by two facts. First, our sample comprises a number of cities that were not hosting a Jewish community before 1500. Clearly, in the great majority of these cities there isn't any Jewish lending or pogroms until 1500, which creates a spurious positive correlation between these two variables until that year. Second, cities characterized by Jewish lending before 1500 were also larger before the Reformation and hosted larger Jewish communities, which increases the probability of pogroms and the probability that records of anti-Semitism survived until today.

TABLE 6—ANTI-SEMITISM BEFORE AND AFTER PROTESTANT REFORMATION: THE ROLE OF JEWISH LENDING

Dependent variable:	Pogrom happened in the century									
	OLS diff-in-diff-in-diff			IV difference-in-difference-in-differences estimates						
Estimation method:										
	Without pre-1500 lending	With pre-1500 lending	Main sample	Main sample		Cities with Jews < 1500		Main sample		
Sample:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Protestant × After	0.017 (0.017)	0.147 (0.044)	0.017 (0.017)	-0.089 (0.062)	-0.093 (0.058)	-0.186 (0.151)	-0.208 (0.136)	-0.091 (0.063)	-0.084 (0.062)	-0.155 (0.081)
Protestant × (pre-1500 Jewish lending) × After			0.130 (0.048)	0.534 (0.230)	0.544 (0.216)	0.629 (0.349)	0.673 (0.319)	0.535 (0.235)	0.513 (0.230)	0.947 (0.402)
Century dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Pre-1500 Jewish lending × After			✓	✓	✓	✓	✓	✓	✓	✓
Number of sectors pre-1500 × After				✓	✓	✓	✓	✓	✓	✓
Jewish presence (and interaction with After)					✓		✓			
Presence of school (and interaction with After)								✓		
Military conflicts (and interaction with After)									✓	
Population brackets (and interaction with After)										✓
Mean dep. var.	0.080	0.275	0.129	0.129	0.129	0.203	0.203	0.129	0.129	0.114
R ² (within)	0.055	0.272	0.150							
Cragg-Donald Wald F-statistic				42.781	35.473	24.258	20.286	21.900	33.786	1.602
Kleibergen-Paap rk Wald F-statistic				12.325	10.408	8.315	7.778	6.972	9.818	0.497
Observations	3,880	1,312	5,192	5,192	5,192	3,164	3,164	5,192	5,192	4,588
Number of cities	970	328	1,298	1,298	1,298	791	791	1,298	1,298	1,178

Notes: The table reports estimates from panel regressions with city fixed effects. The sample period is 1300–1700. The unit of observation is city by century. *After (Reformation)* is a dummy variable for the centuries 1500–1600 and later. *Protestant* is a dummy variable that identifies cities which had a Protestant ruler in 1546. Columns 1 through 3 apply OLS to panel regressions with city fixed effects. Columns 4 through 10 show instrumental variables regressions with city fixed effects, where the endogenous variables are (*Protestant × pre-1500 Jewish lending × After*) and (*pre-1500 Jewish lending × After*), and the excluded instruments are (*Number of pre-1500 trade-(related) sectors × After*) and (*Protestant × number of pre-1500 trade-(related) sectors × After*). Standard errors (reported in parentheses) are clustered at the city level.

After analyzing the raw data in online Appendix Figure A.14, we move to the regression analysis in Table 6. Here, we first divide the sample depending on whether there was evidence of Jewish lending in the city or not and re-estimate equation (2). The estimates reported in columns 1 and 2 confirm that the relative increase in pogrom intensity in Protestant cities relative to Catholic cities in the century of the Reformation and the following one is found only for cities in which Jews used to be moneylenders. Also here, we repeat the analysis estimating the following difference-in-difference-in-differences specification:

$$(7) \quad Y_{it} = \beta_0(\text{Protestant}_i \cdot \text{After}_t \cdot \text{JewLending}_i) + \beta_1(\text{After}_t \cdot \text{JewLending}_i) + \sum_{t=1400}^{1600} \beta_t^P \text{Protestant}_i + X_{it}B + \beta_i + \beta_t + \eta_{it},$$

where *JewLending_i* is a dummy variable that identifies those cities for which there is evidence of Jewish lending before 1500. Again, we emphasize that we do not

assume that the cities in which Jews were moneylenders in 1500 are randomly distributed: these cities are different across several dimensions (e.g., they are larger and more involved in trade and manufacturing) which might have an impact on anti-Jewish sentiments and acts. We account for this by controlling for city-fixed effects and for the interaction between the pre-1500 Jewish lending and the post-Reformation dummy. Column 3 of Table 6 reports the estimated coefficient on the triple interaction term, which is positive and statistically significant (the estimated coefficient, 0.13, reflects the difference between the difference-in-difference coefficients estimated in columns 1 and 2).⁷²

Despite controlling for various fixed effects, we might still capture a lower bound of the impact of the Protestant Reformation in cities with Jewish lending for two reasons. The first reason is related with the sources we used on Jewish history until 1517: information on Jewish communities are likely to be more complete at the times of pogroms, which creates a spurious positive correlation between pre-Reformation pogroms and pre-Reformation Jewish lending, especially if the disputes over moneylending are the source of expulsions. The second reason relates to the fact that in cities with pre-Reformation Jewish lending, the presence of a powerful Jewish bourgeoisie might have partially shielded the Jewish minority.

To address the potential bias of the OLS analysis, we move to two-stage least squares (2SLS) estimates: we instrument the dummy that identifies evidence of pre-Reformation Jewish lending using the number of industries related to trade and trade-related services in which the city was specialized before the Reformation. This IV addresses the two concerns above because it captures the larger returns to banking and thus the “need” of Jewish lending before the Reformation rather than actual evidence of Jewish lending and it’s not collected from sources on Jewish history but from the *Städtebuch*. Specifically, the first-stage equations are

$$(8) \text{ Protestant}_i \cdot \text{After}_t \cdot \text{JewLending}_i \\ = \lambda_0(\text{Protestant}_i \cdot \text{After}_t \cdot \text{NrEntriesTrade}_i) + \lambda_1(\text{After}_t \cdot \text{NrEntriesTrade}_i) \\ + \sum_{t=1400}^{1600} \lambda_t^P \text{Protestant}_i + X_{it} \Gamma + \lambda_i + \lambda_t + \zeta_{it}^1,$$

$$(9) \text{ After}_t \cdot \text{JewLending}_i \\ = \pi_0(\text{Protestant}_i \cdot \text{After}_t \cdot \text{TradeCity}_i) + \pi_1(\text{After}_t \cdot \text{NrEntriesTrade}_i) \\ + \sum_{t=1400}^{1600} \pi_t^P \text{Protestant}_i + X_{it} \Pi + \pi_i + \pi_t + \zeta_{it}^2,$$

where NrEntriesTrade_i is the number of industries related to trade or trade-related services that are reported in the *Städtebuch* as characterizing the economic activity of city i for the pre-Reformation years.

⁷²Online Appendix Table A.20 presents OLS difference-in-difference-in-differences results with various additional controls.

The exclusion restriction amounts to assuming that, conditional on a long list of fixed effects, the pre-Reformation trade specialization of a city affected the relative change in pogrom intensity of Protestant versus Catholic cities that followed the Reformation only through influencing the probability of pre-Reformation Jewish moneylending.

A potential problem with the 2SLS estimates is that larger cities have more information regarding the industries that operated there before the Reformation. For this reason, we always control for the number of industries that are mentioned in the *Städtebuch* as operating in the city before the Reformation interacted with the post-Reformation dummy.

The second-stage estimates are reported in columns 4–10 of Table 6. (Online Appendix Tables A.21 and A.22 report the first-stage and reduced-form estimates, respectively.) As expected, the estimated difference-in-difference-in-differences coefficients are generally larger in the 2SLS estimates compared to the OLS estimates. Our 2SLS estimates can be thought of as LATE-type estimates.⁷³ They capture the difference-in-differences shift in anti-Semitism (Protestant versus Catholic cities and after versus before, i.e., the standard difference-in-differences estimates) in cities with pre-Reformation Jewish lending relative to cities without Jewish pre-Reformation lending (i.e., the triple difference), where the Jewish lending is caused by the city being (more) active in trade (compliers).

The magnitude of the coefficient in column 4 of Table 6 (0.53) might seem large, but we would like to put it in context. To do so, we go back to the pogrom intensity in different subsets of cities. On one extreme, in cities with pre-Reformation Jewish lending and with the majority of economic activity in trade, the pogrom intensity increased from 0.3 to 0.4 in Protestant cities, while it decreased from 0.4 to 0.1 in Catholic cities: thus, the relative increase in pogrom intensity in Protestant places relative to Catholic places was here close to 0.5. At the other extreme, in cities without Jewish lending and with little trading activity, the pogrom intensity remained substantially unchanged both in Protestant and Catholic places. This large difference between these two subsets of cities in their respective trajectories of pogrom intensity between Protestant and Catholic cities suggests that variation in trading activity selects a group of compliers where the DD effects are quite large.⁷⁴

In column 5, we control for a time-varying indicator for the presence of a Jewish community as well as its interaction with a post-Reformation dummy: the estimated triple difference coefficient is practically unchanged. In column 6 and 7, we restrict the analysis to cities with confirmed Jewish presence before 1500 and find our main results confirmed.⁷⁵

In column 8, we control for a time-varying indicator for the presence of a school and its interaction with a post-Reformation dummy: the estimated difference-in-difference-in-differences coefficient remains unaffected.

⁷³Local average treatment effects (LATE) apply in the case of binary treatments and binary instruments. Our setting with two endogenous variables, namely the triple interaction term involving Jewish lending which is our focus here, and the double interaction term, is slightly more complicated. Also note that our instrument is based on the number of subsectors involved in trade.

⁷⁴Remember that the group of “compliers” is a subset of the treated cities (i.e., those with pre-Reformation Jewish lending), and, as is well known, an unobserved subset (as we are not able to identify which treated cities are compliers), which is why the LATE cannot be directly computed based on the raw data.

⁷⁵For this sample, online Appendix Table A.23 presents additional results with additional controls.

Complementary evidence is shown in online Appendix Table A.24, where we run regressions similar to the first three columns of Table 6, i.e., we substitute the pre-1500 Jewish lending indicator with an indicator for the presence of a school before the Reformation (i.e., the coefficient of interest is now associated with the triple interaction: Protestant \times After \times Pre-1500 School). We do not find evidence for the hypothesis that the Protestant Reformation led to a differential increase in anti-Semitism in cities that had a school before the Reformation. Taken together, the results in column 8 of Table 6 and in Table A.25 suggest that schooling had a rather limited impact on the geography of anti-Semitism in German cities, following the Reformation. Admittedly, we cannot rule out the human capital channel, as the presence of a school is a rather crude measure for literacy and numeracy rates in German cities, and it might not capture the diffusion of the more refined skills that are needed to work in the lending sector.⁷⁶

Another potential explanation for our results is that the Protestant Reformation might have affected the level of conflicts in a city, in a differential way between trade and non-trade cities. In column 9, we control for different indicators of war activity in which a city is involved, and their interaction with a post-Reformation dummy. Again, the main coefficient remains unaffected. Another potential confounding factor comes from the fact that the sectoral specialization before 1500 might be a measure of the level of economic success of the city before 1500. More specifically, our results might capture the fact that the Protestant Reformation could have induced a differential impact on anti-Semitism depending on the level of development of the city. Population figures for these cities are our best estimates of their economic success. The estimates reported in column 10 control for population size and its interaction with the post-Reformation dummy. The estimated coefficient on the triple difference is positive, statistically significant and larger than in the previous columns; the low *F*-statistic is explained by the fact that the excluded instruments do not have much power in predicting the interaction between pre-Reformation lending and the post-Reformation variable and implies that the results should be taken with a grain of salt.⁷⁷

Further Robustness Checks.—Online Appendix Tables A.25 through A.30 present various robustness checks. Online Appendix Table A.25 uses all 2,344 cities in the *Städtebuch*. Online Appendix Table A.26 drops 1 of 18 regions in the *Städtebuch* at a time. Online Appendix Table A.27 uses six centuries of data, from 1300–1900. All of the robustness checks broadly confirm our findings in the main sample. Online Appendix Table A.28 defines a city to be Protestant if it had a Protestant ruler in 1650. Online Appendix Table A.29 excludes cities that returned to Catholicism after being Protestant in 1546. Online Appendix Table A.30 excludes the five most prominent pogrom waves one by one.

⁷⁶One referee made the point that “[o]ne does not need a push toward broad, mass education (as in Protestant territories) to put gentiles in competition with Jews in the market for moneylending. Making everybody able to read and write is not necessary. For Christians to engage in moneylending, you only need an elite with high education and numeracy, not peasants and working classes who can barely decipher the Bible. And such an elite with literacy and numeracy skills, and endowed with sufficient capital to start moneylending, existed already in cities, Protestant and Catholic alike, before and after the Reformation: the merchants. The crucial constraint preventing Christians to enter the moneymaking business was not the lack of education.”

⁷⁷Notice that a weak instrument problem would bias the 2SLS estimates toward the OLS estimates, and therefore against estimating large triple interaction coefficients.

Calvin versus Luther.—As we have detailed in the introduction, Luther himself denounced the Jewish people, and in particular the Jewish lenders. We would tend to see this as part of the process through which violence against the Jews was coordinated rather than an independent (and thus confounding) factor.⁷⁸ To make this point, we exploit the fact that there were two different Protestant denominations in the German lands after the Reformation: Lutherans and Calvinists. Zeeden (1984) provides detailed data on the exact Protestant denomination of the rulers of German cities in 1650.⁷⁹ Luther and Calvin were different with respect to their views on both usury and the Jews. Specifically, Calvin was more tolerant on usury,⁸⁰ but less (openly) anti-Semitic compared to Luther.⁸¹ If preaching against Jewish lenders is an independent and confounding factor explaining the difference-in-difference-in-differences results, one would expect Lutheran areas to see a stronger shift toward anti-Semitism than Calvinist areas. The opposite seems to be the case. Online Appendix Table A.11 shows that the shift in anti-Semitism is generally more pronounced in areas with Calvinist rulers compared to those with Lutheran rulers, although this difference is rarely statistically significant.

B. Book Titles: 1451–1600

We can even document a differential shift in anti-Jewish sentiments in our analysis of book titles, despite the small sample size. In online Appendix Table A.31 we use the equivalent of equations (3), but now using the city-decade level data on the anti-Jewish publications and restricting the sample to printing cities.⁸² This table mirrors Table 2, but now reporting the difference-in-difference-in-differences estimates. Columns 1–2 reports estimates from Poisson regressions and the dependent variable is the number of anti-Jewish books published in the city-decade; columns 3–4 reports weighted least squares regressions when the dependent variable is the share of anti-Jewish books; columns 5–6 reports weighted regressions when the dependent variable is the median probability that the book published is anti-Jewish.⁸³

In all columns, the coefficient on the triple difference is positive and is statistically significant in 5 out of 6 columns. The triple difference estimates imply that the

⁷⁸We thank one referee for making this point.

⁷⁹Note that when rulers converted from Lutheranism to Calvinism (e.g., Margrave Johann Sigismund of Brandenburg-Prussia in 1613), the population often remained Lutheran. The implicit assumption underlying this analysis is thus that the denomination of the ruler mattered, not the one of the citizens.

⁸⁰Baron (1972) quotes from Calvin's comment on Luke, chapter 6, verse 35 ("But love your enemies, do good to them, and lend to them without expecting to get anything back."): "No Scriptural testimony exists which would totally condemn usury. [...] [Luke chapter 6, verse 35] has been gravely distorted." Several ordinances in Calvin's city of Geneva officially set the allowable maximum interest rate at 5 percent. "Certainly no legislation adopted by the city during Calvin's regime could have been promulgated without at least his tacit approval."

⁸¹Baron (1972, p. 338) writes: "Calvin had few, if any, contacts with contemporary Jews." Pak (2009) writes: "Calvin seems to have very little to say about contemporary Jews and Judaism, and he did not feel inclined to write separate treatises on the topic, unlike a number of his contemporaries." This is of course not to say that Calvin wrote or said literally nothing about Jews. Baron (p. 339) notes that "Occasionally he indulged in gibes at contemporary Jewish greed and repudiated the Jews' privileged position with respect to charging interest to non-Jewish borrowers." But Baron (p. 348) concludes that "Calvin showed himself, on the whole, somewhat more merciful toward Jews and Muslims than toward Christian heretics."

⁸²More specifically, we use the same benchmark sample as the one in the Section IIIB, which comprises the set of cities that printed at least ten different books from 1451–1600 and had the same religion of the surrounding territories.

⁸³Unweighted OLS estimates are reported in online Appendix Table A.32: as can be seen, weighting observations does not substantially change the results.

great majority of the shift in anti-Jewish attitudes (now captured by anti-Jewish publications) from Catholic to Protestant places in the aftermath of the Reformation is explained by cities with evidence of Jewish lending before the Reformation. These results should be taken with a grain of salt as the sample is now restricted to 91 cities,⁸⁴ admittedly a rather small sample to estimate a triple difference coefficient.⁸⁵

C. Summary of Evidence for the Economics of Anti-Semitism

In conclusion, these results provide evidence that the increase in anti-Semitism following the Protestant Reformation was more pronounced in Protestant areas with a greater pre-Reformation “need” for local moneylenders. This finding is consistent with our hypothesis that the Catholic ethics of usury and the high levels of human capital among the Jews partially shielded the Jewish minority in cities in which Jewish moneylenders were particularly needed. The observed shift of anti-Semitism toward these areas is in line with the historical narrative that Jews lost their prerogatives in these cities and were exposed to a tougher competition with the Christian majority.

While not strictly necessary to support our argument, online Appendix D documents a lower Jewish market share in banking and finance in Protestant areas of late 19th century Prussia.

V. Conclusion

In the recent debate on the determinants of anti-Semitism, economic factors have received comparatively little attention. Although there is no doubt that cultural and political factors are at play, we show that economic factors also play a role. Using data on German cities and regions over six centuries, we show that the geography of anti-Semitism is related to the geography of economic interactions between the Jewish minority and the Christian majority. The Catholic ethics of usury and higher levels of human capital with respect to the majority population gave to the Jewish minority a comparative advantage in moneylending in Catholic regions. This produced a complementarity between Jews and Christians that was broken up in Protestant areas after the Reformation. Jews were by no means sheltered from pogroms even in Catholic areas, as evidenced by the well-known pogroms after the Black Death in 1349, well before the Reformation. However, our results document that anti-Semitic acts and attitudes became relatively more frequent in Protestant areas relative to Catholic areas after the Reformation. We show that this differential effect of the Reformation is largely driven by the set of cities with documented Jewish lending activity before the Reformation. It is consistent with the idea that, after the Reformation, Jews lost their prerogatives in banking and finance in Protestant Germany but not in Catholic Germany. We document, for late nineteenth century Prussia, a lower Jewish market share in banking in Protestant areas.

⁸⁴Observations are distributed in the following way: 23 Protestant cities and 31 Catholic cities with pre-Reformation evidence of Jewish lending; 13 Protestant cities and 24 Catholic cities without any evidence of pre-Reformation Jewish lending.

⁸⁵The size of the sample also makes it too demanding to run 2SLS estimates of the kind we run in this section using the pogrom data.

Our findings are important for both researchers and policymakers. For researchers, they provide empirical evidence on the link between the division of labor and anti-Semitism. Moreover, the dataset that underlies the research provides researchers with a new and extensive source of information covering anti-Semitism in German cities and counties over six centuries. For policymakers who are willing to learn from history, our results suggest that anti-Semitism, and inter-ethnic conflict more generally, does respond to economic incentives. This is an important finding in light of ongoing contemporary ethnic conflicts worldwide.

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