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Middleman Minorities and Ethnic Violence: Anti-Jewish Pogroms in the Russian Empire

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Using detailed panel data from the Pale of Settlement area between 1800 and 1927, we document that anti-Jewish pogroms—mob violence against the Jewish minority—broke out when economic shocks coincided with political turmoil. When this happened, pogroms primarily occurred in places where Jews dominated middleman occupations, *i.e.*, moneylending and grain trading. This evidence is inconsistent with the scapegoating hypothesis, according to which Jews were blamed for all misfortunes of the majority. Instead, the evidence is consistent with the politico-economic mechanism, in which Jewish middlemen served as providers of insurance against economic shocks to peasants and urban grain buyers in a relationship based on repeated interactions. When economic shocks occurred in times of political stability, rolling over or forgiving debts was an equilibrium outcome because both sides valued their future relationship. In contrast, during political turmoil, debtors could not commit to paying in the future, and consequently, moneylenders and grain traders had to demand immediate (re)payment. This led to ethnic violence, in which the break in the relationship between the majority and Jewish middlemen was the igniting factor.

Key words: Middleman minorities, Pogroms, Jews, Climate shocks, Ethno-occupational segregation, Conflict, Political uncertainty.

JEL Codes: N33, Z12, J15, N43, Z13

1. INTRODUCTION

Ethnic minorities that dominate middleman occupations, such as traders and financiers, often become targets of persecution and ethnic violence. Examples abound across the globe and

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throughout history: Chinese in the Philippines and Indonesia, Igbos in Nigeria, Lebanese in Sierra Leone, Muslims in India, Greeks and Armenians in the Ottoman Empire, and Jews in Medieval Western and Modern Eastern Europe.

An important body of scholarship in political science (*e.g.* Bonacich, 1973; Chua, 2004; Sowell, 2005) argues that these "middleman minorities" are persecuted because of the very nature of their occupations: the majority views minorities specializing in credit and trade as "unproductive" and considers that these groups earn their living dishonestly through "parasitism" and "exploitation" of the majority. This sentiment, in the view of political scientists, explains why middleman minorities are particularly vulnerable.

Politico-economic literature, in contrast, considers economic competition between different ethnic groups as one of the primary drivers of ethnic conflict (*e.g.* Blattman and Miguel, 2010; Caselli and Coleman, 2013). At least since Bates (1974) and Horowitz (1985), it has been argued that ethnic minorities who directly compete with the majority are more likely to become the target of ethnic violence compared to minorities occupying economic niches the majority does not specialize in. Recent studies have provided systematic evidence in support of this conjecture in different contexts. For example, Muslim traders avoided violence in ports of South Asia because of their economic value to the Hindu majority (Jha, 2013, 2014). Towns where Jews provided moneylending and trading services to the majority were spared during the wave of anti-Jewish violence following the outbreak of the Black Death in Western Europe (Jedwab and *et al.*, 2019). The reformation led to antisemitic violence in the Protestant but not in the Catholic parts of Germany when the Protestant majority entered the credit sector, traditionally dominated by the Jews, after the religious ban on usury was lifted (Becker and Pascali, 2019).

Many episodes of violence against middleman minorities, however, took place without any increase in competition in the middleman sector. In these episodes, violence broke out even though the economic activities of the targeted groups complemented those of the majority. This raises the question of whether a certain politico-economic mechanism, distinct from competition, drives such episodes of violence. In order to address this question, one needs to examine the conditions under which violence against middleman minorities breaks out. In this article, we do this by focusing on the historical events that brought the word *pogrom* into European languages.

We examine the determinants of the outbreaks of anti-Jewish mob violence in the 19th and early 20th century in the Pale of Settlement, a vast area in the Russian Empire where Jews were allowed to live and where they dominated market-intermediary occupations, such as trading and moneylending (*e.g.* Slezkine, 2004; Grosfeld *et al.*, 2013). Figure 1 presents the map of the Pale of Settlement and the locations of pogroms. We combine detailed panel data on anti-Jewish pogroms as well as measures of shocks to agricultural output and to prices of the main agricultural commodity with detailed cross-sectional data on local ethnic composition by occupation and with proxies for preexisting antisemitism to explore the causes of pogroms. We show that pogroms occurred primarily in localities where Jews dominated intermediary occupations related to agriculture (*i.e.* moneylending and grain trading) when economic shocks, caused by severe crop failures and increases in grain prices, coincided with political turmoil threatening the political and social order. Economic crises did not lead to pogroms if they were not concomitant with political turmoil. Economic shocks together with political shocks did not result in pogroms in localities where Jewish community specialized in other occupations, including middleman occupations unrelated to agriculture.

Our main findings are illustrated in Figures 2 and 3, which highlight the variation in pogroms over time and across localities. In Panel A of Figure 2, we present the number of pogroms over time. The vast majority of pogroms came in three waves, which has been well noted by historians (*e.g.* Klier and Lambroza, 1992b). In Panel B, we overlay this time series with economic shocks, *i.e.*, the times of severe crop failures in major grain-producing areas, which led to substantial increases

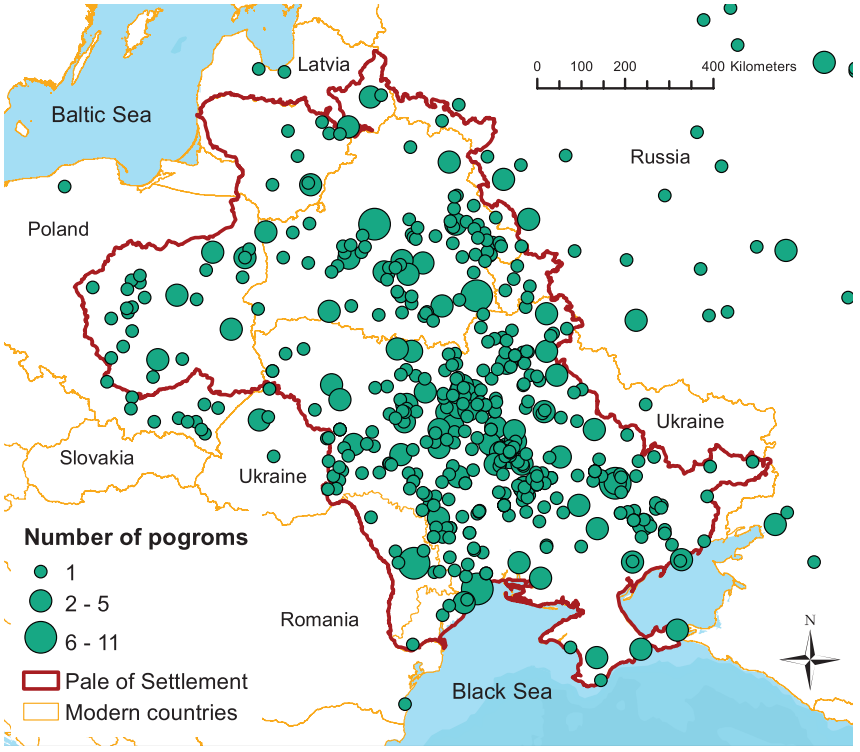

FIGURE 1

The Pale of Settlement and the geographic distribution of pogroms.

Notes: This map presents the geographic distribution of pogroms in Eastern Europe. The darker thicker line represents the border of the Pale of Jewish Settlement. The lighter thiner lines represent modern country borders. The Pale of Settlement measures about 1,400 kilometres in the South-North direction and 1250 kilometres in the East–West direction. The map is produced using equidistant conic projection.

in grain prices.[1] In Panel C, we superimpose the timing of pogroms on episodes of political turmoil, i.e., the periods of extreme political uncertainty about the future, such as following the assassination of Alexander II, the Tsar-Liberator, when peasants thought serfdom would be reinstalled by the new tsar, or during wars that led to occupation of Russia's territory, or the Russian revolutions. In Panel D, we show what happens when the economic shocks coincided with political turmoil. (We describe how we measure economic shocks and political turmoil in detail below.) Figure 2 shows that pogrom waves occurred every time economic shocks coincided with political turmoil.[2] Importantly, economic shocks and political turmoil are distinct: economic shocks that coincided with political turmoil were no more severe on average than those that did

1. We show that the Pale of Settlement was a single market for grain so that grain prices moved together in different areas of the Pale and were determined by shocks to harvest in the major grain-producing areas.

2. Historians also have noticed the extraordinary combination of economic and political crises that led to violence against Jews, in particular violence against Jewish moneylenders. Rogger (1992a) argues that violence against Jewish creditors in Germany, Austria, and French Alsace was brought about by political turmoil arising from the 1848 revolutions in combination with the harvest failures of 1845 and 1846 (p. 314). Aronson (1990) describes the factors that triggered the first wave of pogroms in the Russian Empire: "Exceptional circumstances existed in 1881 ... Unknown tsar had ascended the throne in the wake of the violent assassination of the "Tsar liberator," and the peasants were uncertain [about their future]. The weather was unseasonably hot ... During 1880 and 1881 local crop failures had brought on near famine

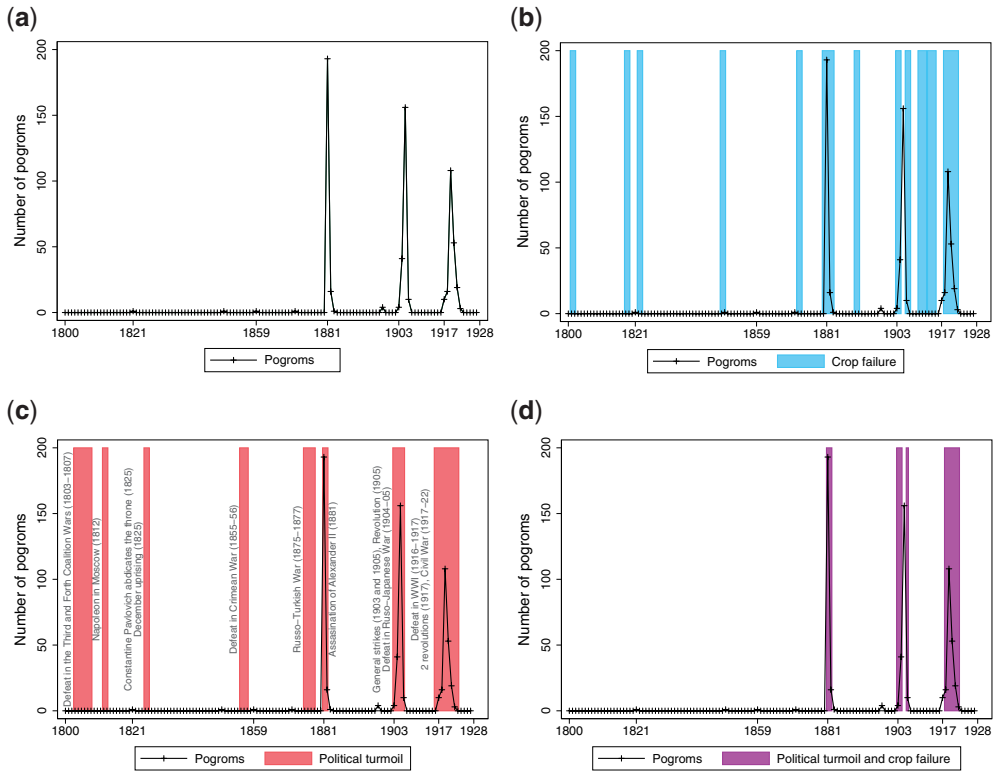

FIGURE 2

Pogrom waves, marketwide economic shocks, and political turmoil. (a) Pogrom occurrence; (b) Pogrom occurrence and marketwide economic shocks; (c) Pogrom occurrence and political turmoi; (d) Pogrom occurrence at the intersection of political turmoil with marketwide economic shocks.

Notes: This figure presents the number of pogroms over time in the Pale of Jewish Settlement. Panel A presents the time series of the pogroms. Panel B adds to this time series the marketwide economic shocks, *i.e.*, the periods when prices of grain went up due to crop failures in the main grain-producing areas. Panel C highlights the periods of political turmoil, *i.e.*, of extreme political uncertainty. Panel D presents the periods when marketwide economic shocks coincided with political turmoil.

not coincide with political turmoil. Several episodes of extremely bad harvests, including the largest famine in the Russian Empire of 1891, took place during periods of political stability and did not cause ethnic violence.

Figure 3 presents the second key driver of pogroms — the Jewish domination over middleman occupations servicing agriculture: moneylending and grain trading. Panel A shows that the frequency of pogroms in localities that suffered from local crop failures in times of political turmoil strongly depended on the share of Jews among local moneylenders: in times of political turmoil, local crop failures were more likely to trigger pogroms in places where most of the

conditions in some areas" (p. 122). Lambroza (1992) writes about the second wave of pogroms in the Russian Empire: "Poor harvest in 1902-1903 caused wide-scale violent unrest in rural areas ... Political conditions were worsened by the disastrous Russo-Japanese War of 1904 and the massacre of innocents at the Winter Palace in January 1905" (p. 195). The occurrence of group violence at the intersection of negative economic and political shocks is not specific to anti-Jewish violence. For example, witch trials in New England in the 17th century also took place when economic and political crises coincided (Boyer and Nissenbaum, 1974).

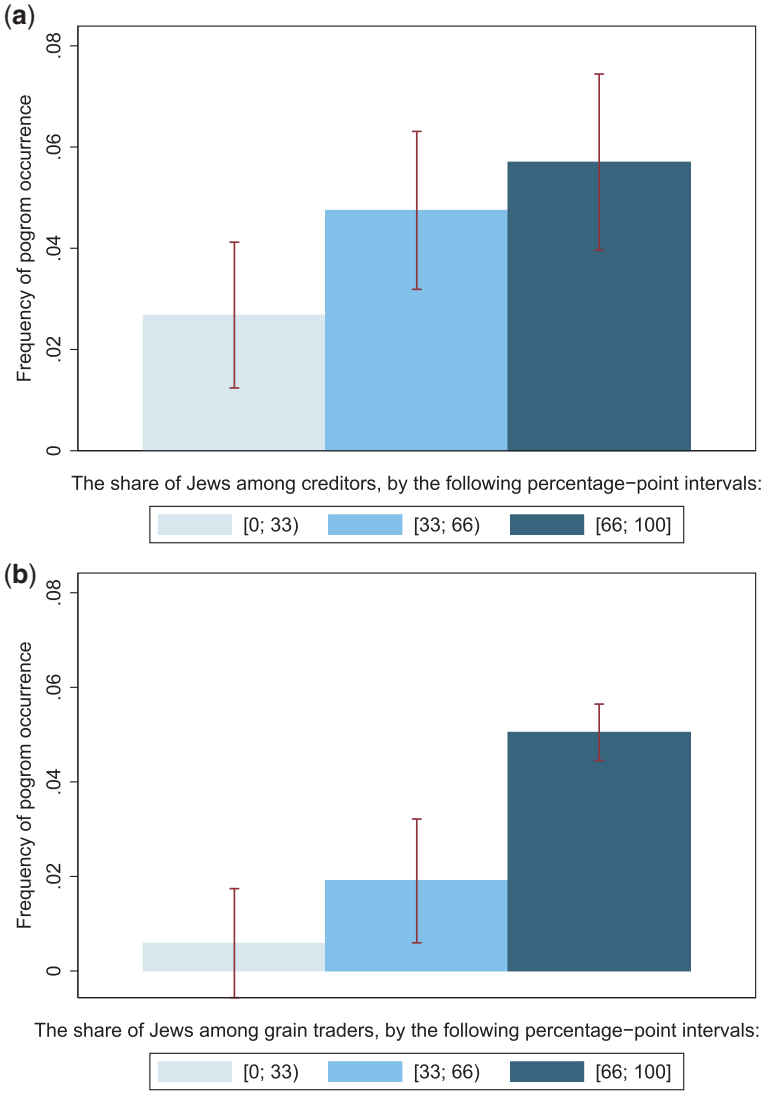

FIGURE 3

Pogrom occurrence and the shares of Jews among creditors and grain traders. (a) Pogroms versus share of Jews among creditors during political turmoil across localities hit by local economic shock. (b) Pogroms versus share of Jews among grain traders during political turmoil and marketwide economic shock across all localities.

Notes: This figure presents the frequency of occurrence of pogroms by percentage-point intervals of the local share of Jews among middlemen during economic and political shocks. Panel A presents the frequency of pogroms as a function of the share of Jews among moneylenders in grid cells that suffered from a negative local economic shock during political turmoil. Panel B presents the frequency of pogroms as a function of the local share of Jews among grain traders in all grid cells during times when political turmoil coincided with marketwide economic shocks.

moneylenders were Jewish. Panel B shows that the share of Jews among local grain traders is a strong predictor of pogroms when political turmoil coincided with periods of high grain prices.

In the empirical analysis, we consider a panel of geographic grid cells measuring 0.5×0.5 degrees (see the map presented in Panel A of Figure A1 in the Online Appendix). The time dimension covers the years between 1800 and 1927, which is the year before the start of Soviet mass collectivization. We estimate a linear probability model with difference-in-differences regressions, in which the probability of pogroms in a grid cell in a year is a function of economic and political shocks and the occupational structure of Jews relative to the majority, controlling for grid-cell and year fixed effects, and adjusting the standard errors for both spatial and over-time correlations. We construct proxies for economic shocks using historical temperature data and government grain-assistance data, showing how they relate to grain yields and prices. We define political turmoil as a time dummy indicating historical events from a comprehensive list of violent successions (with revolt or assassination), defeats in wars, invasions of Russia's territory, general political strikes, and revolutions.

We show that the main driver of pogroms is the interaction of three factors: economic shocks, political turmoil, and the domination of Jews in the middleman sector related to agriculture. The nature of economic shocks that drove pogroms was different in localities where Jews dominated moneylending and where they dominated trade in grain. Local crop failures, which we call local economic shocks, triggered pogroms where Jews dominated the credit sector because credit was extended to peasants locally by moneylenders. In contrast, localities where Jews dominated trade in grain suffered from pogroms when grain prices increased, which happened throughout the Pale because of crop failures in the major grain-producing areas. These marketwide economic shocks were important for grain trade because grain was resold by traders in markets that were often far from the grain-producing areas. We also show that pogroms are not positively associated with engagement of Jews in other occupations, including middlemen unrelated to agriculture.

The mean probability of a pogrom in a grid cell in any given year during the entire 1800–1927 period was 0.5%. Our estimates imply that, in times of political turmoil, local economic shock (*i.e.* local crop failure) increased the probability of a pogrom in an average grid cell by 2.9 percentage points and a marketwide economic shock (*i.e.* a sharp rise in grain prices) increased this probability by 3.8 percentage points (or 41% and 53% of the standard deviation of pogrom occurrence, respectively). Economic shocks did not increase the likelihood of pogroms in times of relative political peace. These average effects of economic shocks on pogroms mask important heterogeneity. The probability of pogroms in a locality depended strongly on the share of Jews among moneylenders and grain traders. Our estimates imply that, in times of political turmoil, if a locality was hit by a local crop failure while grain prices were high, the probability of a pogrom was a precisely estimated zero if, in this locality, there were no Jewish creditors and only 17.6% of grain traders were Jews (*i.e.* the shares of Jews among middlemen were at their minimum); in contrast, the probability of a pogrom increased to 12.5% if all moneylenders and grain traders were Jews (*i.e.* the shares of Jews among middlemen were at their maximum). A one-standard-deviation increase in the shares of Jews among creditors and grain traders increased the probability of pogroms by 3.5 percentage points in times of local and marketwide economic shocks during political turmoil. (The mean value of the share of Jews among creditors was 53.4% with a standard deviation of 28.1 percentage points, and the mean value of the share of Jews among grain traders was 88.2% with a standard deviation of 17.5 percentage points.)

These results are robust to controlling for a large number of potential confounding variables, different levels of aggregation, different assumptions about the variance-covariance matrix, as well as restricting the sample to different subperiods. In addition, the results are robust to using alternative measures of political turmoil and of economic shocks.[3]

There is no viable exogenous source of variation in the shares of Jews among middlemen. Thus, an important concern is whether one can interpret the results as causal. There are several reasons why OLS could produce attenuated estimates that would lead to underestimation of the effects; we discuss them in detail later in the article, and we provide both systematic and anecdotal evidence to support each of these reasons. However, if Jews were more likely to choose middleman occupations related to agriculture in localities where the local majority was more antisemitic, then the results of the OLS regressions would exhibit an upward bias. We present several analyses showing that the latter is unlikely to be the case.

First, we assembled historical data that allowed us to create two alternative proxies for preexisting antisemitism. In particular, we collected and georeferenced data on the locations of violence against Jews that took place before 1800. All the episodes of violence against Jews that took place on the territory which became the Pale of Settlement at the end of the 18th century, when the Russian Empire annexed them from the Polish-Lithuanian Commonwealth, were different from pogroms that we study because Jews as an ethnic and religious group were "neither the initial nor the principal targets" in these episodes (Klier, 2011, p. 59); instead, they were victims of their economic ties to the primary target, Polish-Catholic nobility.[4] Assuming that Jews were more likely to become victims of violence directed against another group in places with higher anti-Jewish sentiment, we can use the locations of these events as a proxy for preexisting antisemitism. We also collected data on the publication place of all antisemitic and non-antisemitic books published in Eastern Europe in the main European and Eastern European languages throughout the 18th century. We use the geographical proximity to places of publication of antisemitic books conditional on the number of literate non-Jews as another proxy for preexisting antisemitism. The two measures of preexisting antisemitism are positively correlated with each other. They also positively correlate with the index of the occupational segregation between Jews and non-Jews across all occupations, suggesting that in more antisemitic places Jews and Gentiles tend to engage in different occupations. However, this correlation is mostly driven by the variation in whether Jews were allowed to work in agriculture. In particular, we show that the measures of preexisting antisemitism are uncorrelated with the shares of Jews in credit and grain trading, conditional on the overall index of occupational segregation or on shares of Jews in other main occupations. We also show that our results are robust to controlling for the interactions of the economic shocks and political turmoil with preexisting antisemitism, overall occupational segregation across ethnic groups, and the shares of Jews in different occupations, suggesting that the results are not driven by the variation in the preexisting antisemitism.

3. Our baseline measure of political turmoil based on historical events is strongly correlated with the increases in the Russia's sovereign bond yield spread relative to the British sovereign bond, which was shown to reflect the country's political uncertainty (Mauro *et al.*, 2002). We replicate our main results using an alternative definition of political turmoil defined entirely on the basis of sovereign yield spread series. We also show that our results are robust to using grain prices directly to measure marketwide economic shocks as an alternative to the baseline measure derived from the climate data. As these regressions require restricting the sample to subperiods for which the yield spread and price data are available, we use proxies based on historical events and climate as the baseline.

4. The two main episodes of violence against Jews before 1800 in the area we study were the Cossack uprising led by Bogdan Khmelnytsky in 1648 and the Koliyivshchyna violence in 1768. Both were uprisings by the Orthodox majority against the Polish elites. Another episode of anti-Jewish violence took place several centuries before, during the Kiev riots in 1113; historians argue that this episode also should not be qualified as a pogrom (Klier, 2011).

Second, we use the methodology developed by Altonji *et al.* (2005) and Oster (forthcoming) to show that our results are unlikely to be driven by unobserved variation. The estimates imply that for our results to be spuriously generated, the effect of the unobservables on pogroms needs to be as least twice as important as the combined effect of the interactions of economic and political shocks with past violence against Jews, the share of antisemitic books, the overall occupational segregation across ethnic groups, and the interactions of the shocks with the shares of Jews in the local population and in middleman occupations. This is extremely unlikely, as we do directly control for the main drivers of ethnic violence.

Finally, we present the results of the spatial matching analysis. In particular, we replicate our results in a subsample of grid cells which are geographic neighbours. We define two treatments: for the domination of Jews over the credit sector and for the domination of Jews over the grain-trading sector. For each treatment, we identify all pairs of contiguous grid cells that differ in that treatment, but that have the same value of economic and political shocks and the same treatment status based on the other middleman occupation. In these subsamples of matched contiguous grid cells, treatment cells are slightly (but statistically significantly) more rural. However, conditional on urbanization, all observables are balanced between treatment and control grid cells, suggesting that unobservables are also likely to be balanced. We show that our results are robust to using the matched subsamples and that controlling for urbanization and for the treatment-control-pair fixed effects does not affect the results. The point estimates of the main effect in the full sample and in the matched subsamples are similar, also suggesting that our results are not driven by omitted variables. Overall, all these pieces of evidence suggest that our results are extremely unlikely to have an upward bias. Thus, we interpret them as causal.

What is the mechanism behind these results? The evidence we present is inconsistent with the traditional "scapegoat" theory, according to which Jews were blamed for all economic misfortunes of the majority (*e.g.* Girard, 1986; Glick, 2008). There are several reasons for this. First, basic scapegoating theory would imply that Jews are targeted as a group, irrespective of their occupations. Second, political turmoil could matter for scapegoating because it is associated with lower probability of punishment of potential perpetrators. However, in the first two waves of pogroms, political turmoil was not associated with a decrease in the probability of punishment; instead, it meant the increase in uncertainty about the future, affecting the relationship between creditors and debtors, which we discuss below in detail. Historically, during the first two waves of pogroms, the state had full capacity to punish pogrom perpetrators and actually did it. Both *ex ante* and in most cases *ex post*, the probability of punishment of pogrom perpetrators was rather high (Klier, 2011, p. 49). In the last wave of pogroms (which took place after the 1917 revolution), political turmoil meant both greater uncertainty about the future and the collapse of state institutions, particularly law enforcement. During that wave, ethnic violence was exacerbated by the absence of law enforcement in the midst of the Civil War. We show that our results hold both before and after 1917. We also use data on theft, homicide, and arson per capita as placebo outcomes before 1917 to show that our results are not driven by an increase in general crime, which would have been the case if political turmoil meant simply a decrease in law enforcement (Bignon *et al.*, 2017).

Furthermore, in turbulent times, violence may be directed against ethnic groups that constitute economic elites, either because of an attempt to appropriate their resources or for reasons of scapegoating. The Jews in the Pale of Settlement, however, were neither an economically thriving minority nor the elite—generally, they became particularly poor in the second half of the 19th century due to severe discrimination, double taxation, restrictions on mobility, and the ban on doing agriculture in most areas within the Pale (with the exception of the agricultural colonies of Novorossiya). There were, of course, differences in income and wealth within the Jewish

communities; in particular, Jewish middlemen were better off than Jewish cobblers and tailors. However, there is no evidence that Jewish creditors and grain traders were richer than other Jewish middlemen engaged in activities unrelated to agriculture, such as traders of non-agricultural goods, whereas only Jewish domination over the middleman sector *related to agriculture* was associated with pogroms.

Importantly, shocks to relative rather than absolute incomes could exacerbate ethnic conflict as was shown by Mitra and Ray (2014). We consider what happens to the relative income of the peasant majority during agricultural economic shocks compared to the minority involved in grain trade, trade in non-agricultural goods, and credit and argue that the income of the majority was affected more relative to traders of non-agricultural goods than relative to creditors. This means that the relative income cannot be the mechanism as there is no evidence that in communities where Jews dominated non-agricultural trade, economic and political shocks led to pogroms. To sum up, the fact that neither severe economic shocks in the absence of political turmoil before 1917 nor the domination of Jews in trade in non-agricultural goods caused pogroms is inconsistent with the traditional scapegoat theory and with violence aiming at appropriation of economic resources in times of economic shocks.

The following mechanism is consistent with our findings. The economic relationship between Jewish middlemen and the majority was based on repeated interactions: on a regular basis, creditors lent to peasants and grain traders extended credit both to peasants in rural areas and to urban buyers of grain.[5] As we discuss below, historians point out that in periods of bad economic shocks, it was common for creditors to forgive debt completely or to roll it over for the next period (Antonov, 2016). The reason why creditors were ready to forgive debt was the value of continuation of their relationship with debtors. Political turmoil made this continuation uncertain as debtors were not able to credibly commit to continue business in the future. Political shocks such as the threat of bringing back serfdom, displacement or conscription of peasants and urban dwellers, collectivization of peasants' farms, or nationalization of private property in the future, led to the situation in which creditors required immediate repayments of debt and grain traders demanded immediate payments for grain even in the case of severe economic shock. Middlemen were unable to forgive debts and extend new credit because they were rationally anticipating that debtors may not be in the position in the future to continue economic relationship with creditors because of political uncertainty. The debtors, in turn, could not repay due to economic shocks and reverted to violence in expectation that Jewish middlemen would require repayments.[6] This mechanism is similar to the causes of violence against moneylenders in rural India during the Deccan Riots at the end of the 19th century as described and modelled by Kranton and Swamy (1999).[7]

This politico-economic explanation of pogroms does not mean that we question the prevalence of antisemitism among non-Jews in the Pale of Settlement. Anti-Jewish attitudes were transferred from one generation to the next through family upbringing in the Pale (*e.g.* Grosfeld *et al.*, 2013) as much as in other parts of Europe (*e.g.* Voigtländer and Voth, 2012). Many of the pogroms were justified by the perpetrators' beliefs in *blood libel* (*e.g.* Klier and Lambroza, 1992a). Our results

5. Trade credit, *i.e.*, delayed payments for goods, was very common. In the Russian Empire, trade and credit were interlinked, as is the case in many developing countries (Ray, 1998, pp. 561–582).

6. Note that credit contracts were relatively well enforced in the Russian Empire before the collapse of the state in 1917 (Antonov, 2016).

7. The difference between Deccan Riots and pogroms in the Russian Empire is the reason why repeated interaction, on which the relationship between creditors and debtors was based, was no longer credible. In the case of Deccan Riots—it was the colonizer's legal reforms, whereas in the case of pogroms—it was political turmoil.

highlight the economic and political factors that led to outbursts of violence under the conditions of inherent religious and ethnic animosity.

We contribute in several ways to the vast literature on economic, political, institutional, and climatic determinants of ethnic conflict, reviewed by Blattman and Miguel (2010), Jackson and Morelli (2011), and Burke *et al.* (2015); see also Caselli and Coleman (2013), Mitra and Ray (2014), and Esteban *et al.* (2015). First, our results highlight the importance of political uncertainty as a factor that links economic shocks to ethnic violence against middleman minorities. We do replicate the result of the previous literature in our context by showing that economic shocks were on average associated with pogroms, as was shown by Anderson *et al.* (2017) in the context of anti-Jewish violence in Western European cities in the Middle Ages and, *e.g.*, Miguel (2005), Burke *et al.* (2009), and Harari and La Ferrara (2018) in other contexts. However, we document that this relationship averages out the important effects of political turmoil and the domination of Jews over middleman occupations. Anderson *et al.* (2017) also showed that the correlation between agro-climatic shocks and anti-Jewish violence in the European cities disappeared by the 16th century and that it was lower in states with higher State Antiquity Index (Bockstette *et al.*, 2002). Anderson *et al.* (2017) argue that the emergence of strong nation states in Europe allowed the rulers to protect the Jewish minority from persecution, as the minority was a useful provider of credit to the rulers. This mechanism is different from the one in the context of anti-Jewish pogroms in the Russian Empire: Jews in the Pale of Settlement were not a small wealthy minority, serving the rulers, as in Europe in the Middle Ages; instead, they were a sizeable poor and discriminated minority without any relationship to the crown, often providing middleman services to peasants and townsfolk.

Second, we document that segregation of minorities into middleman occupations is distinct from other cases of ethno-occupational segregation as the former may catalyze violence under unfavourable economic and political conditions, while the latter helps avoiding intergroup conflict by decreasing economic competition. Our results help to reconcile two seemingly contradictory literatures: on the one hand, economists (*e.g.* Jha, 2007, 2013, 2019; Jedwab and *et al.*, 2019) stress the positive role of economic complementarity of ethnic groups for peaceful coexistence; on the other hand, other social sciences (*e.g.* Bonacich, 1973; Chua, 2004; Sowell, 2005; Dubnow, 1920; Slezkine, 2004) document the episodes of violence against ethnic minorities segregated along the occupational lines.

Finally, our work is related to the literature on the economic role played by Jews historically (*e.g.* Botticini and Eckstein, 2012; Johnson and Koyama, 2017; Spitzer, 2015a; Botticini *et al.*, 2017; Becker and Pascali, 2019) and in the long run through the persistence of cultural values (*e.g.* Voigtländer and Voth, 2012; Pascali, 2016; Grosfeld *et al.*, 2013). We also contribute to the literature on the economic and political origins of the persecution of Jews (*e.g.* Allport, 1954; Arendt, 1973; Becker and Pascali, 2019) as well as on its economic and social consequences (*e.g.* Acemoglu *et al.*, 2011; Akbulut-Yuksel and Yuksel, 2015; D'Acunto *et al.*, 2019; Spitzer, 2015b).[8]

The rest of the article is organized as follows. Section 2 provides a brief overview of the Pale of Jewish Settlement and the waves of anti-Jewish violence in the Russian Empire. We describe the data and develop the measures of economic and political shocks in Section 3. Section 4 discusses the estimation strategy. Section 5 presents the results. We discuss the mechanism in Section 6. In Section 7, we conclude.

8. Sakalli (2017) and Arbatli and Gokmen (2016) study the consequences of persecution of other middleman minorities.

2. HISTORICAL BACKGROUND

2.1. *Jews in the Russian Empire*

The Russian Empire acquired the largest Jewish community in the world by annexing the territories of the Polish-Lithuanian Commonwealth during the Partitions of Poland (1772–1795); the borders were redrawn and finalized by the Congress of Vienna in 1815. Jews faced restrictions both in spatial mobility and occupational choices since their incorporation into the Russian Empire. They were confined to an area known as the Pale of Jewish Settlement and had the legal status of merchants, which prohibited them from getting involved in agriculture and owning arable land in most areas within the Pale.[9] These restrictions lasted until the 1917 February revolution.

The Pale of Settlement covered a vast area in Eastern Europe, including parts of contemporary Latvia, Lithuania, Poland, Russia, and Ukraine, and the whole of contemporary Belarus and Moldova (as presented in Figure 1). According to the 1897 census, 5.2 million Jews lived in the Russian Empire, out of whom 4.8 million resided in the Pale of Settlement. Jews were a minority constituting 11.3% of the total Pale population and dominated market intermediary professions. In particular, Jews constituted 84% of all traders of agricultural and non-agricultural goods, 92% of all grain traders, and 37% of all moneylenders. In addition, Jews were overrepresented in crafts (45% of all employed in this sector were Jews with tailor and cobbler being the two most popular Jewish occupations within this sector) and in transport (30% of people in transport services were Jews, mostly transporting people and small quantities of goods on horse-driven carts). These professions together absorbed 11% of total Pale's employment. An agricultural worker (*i.e.* peasant) was the most popular occupation in the Pale. 70% of all economically active residents of the Pale were peasants. Only 0.6% of agricultural workers were Jews. Jews were present in every district—the second-tier administrative division of the Russian Empire, known as *uezd*—inside the Pale of Settlement. Yet, there was a great extent of heterogeneity across localities in the Pale both in the presence of Jews and in their occupations. Figure A2 in the Online Appendix presents the spatial distribution of the share of Jews in the local population and among moneylenders, grain traders, traders of other goods, craftsmen, and employed in transportation sector across grid cells in the Pale. Panel A of Table 1 presents the summary statistics on the Jewish presence in the local population and in different local occupations across grid cells in 1897. The average grid cell had 10.3% of Jews among all local residents (with standard deviation of 5 percentage points), 54.7% of Jews among local moneylenders (with standard deviation of 28 percentage points), and 88% of Jews among grain traders (with standard deviation of 18 percentage points).

2.2. *Pogroms*

The Jews of Russia periodically became victims of ethnic violence, *i.e.*, pogroms. A pogrom was a violent mob attack directed specifically at Jews as an ethnic and religious group; it involved physical assaults (including rape and murder) and caused significant damage to Jewish property. The severity of pogroms varied greatly. For example, the pogrom in Balta in March 1882 left

9. The Pale was first instituted by several decrees starting with 1791 and subsequently by law of 1835 (see Pipes (1975) and Klier (1986) for the details of the formation of the Pale of Settlement). There were several exceptions to the Pale restrictions; "native Jews" were allowed to stay in Courland province despite it being outside the Pale. Also, in the 1820s, Jews were evicted from several cities inside the Pale, such as Kiev, Sevastopol, and Yalta. There were exceptions to the occupational restrictions as well. The "enactment concerning the Jews" of 9 December 1804, granted the Jews the right to buy and rent land in Novorossiya, the South-Western provinces of the Pale of Settlement, which led to the formation of the Jewish Agricultural Colonies. *May Laws* of 1882, however, barred Jews from settling anew in the rural areas and from owning and renting any real estate or land outside of towns and boroughs. The only exception to *May Laws* was the Jewish agricultural colonies of Kherson province.

TABLE 1
Summary statistics: pogroms and Jewish presence in different occupations

Variable	Mean	Std. Dev.	Min.	Max.
Panel A: The share of Jews in local population and in occupations across grid cells				
		Number of observations = 576		
Share of Jews in population	0.103	0.051	0.009	0.249
Share of Jews among creditors	0.534	0.281	0	1
Share of Jews among traders	0.800	0.209	0.064	0.988
Share of Jews among agricultural traders	0.812	0.207	0.046	0.995
Share of Jews among grain traders	0.882	0.175	0.176	1
Share of Jews among non-agricultural traders	0.820	0.198	0.087	0.993
Share of Jews among general traders	0.791	0.226	0.068	0.995
Share of Jews among craftsmen	0.481	0.208	0.035	0.841
Share of Jews among transporters	0.350	0.209	0.003	0.913
Share of Jews among peasants	0.007	0.006	0	0.041
Panel B: Pogroms across grid cell × year observations				
		Number of observations = 73,728		
Pogrom occurrence	0.0052	0.0721	0	1
Pogrom occurrence in agricultural season	0.0035	0.0591	0	1
Pogrom occurrence in harvest period	0.0017	0.0408	0	1
Pogrom occurrence in non-agricultural season	0.0015	0.0386	0	1
Pogrom occurrence in unknown season	0.0006	0.0241	0	1
Number of pogroms	0.0087	0.1681	0	20
Number of pogroms in agricultural season	0.0059	0.1455	0	19
Number of pogroms in harvest period	0.0024	0.0689	0	7
Number of pogroms in non-agricultural season	0.0020	0.0555	0	4
Number of pogroms in unknown season	0.0007	0.0317	0	3

two people dead and about 1,200 houses and shops pillaged; the pogrom in Odessa in October 1905 left—according to different sources—between 300 and 1,000 dead and about 5,000 injured; the pogrom in Proskurov in February 1919 left as many as 1,700 dead (Klier and Lambroza, 1992b). Information about historical pogroms has been put together by several Jewish historians, primarily from archival records of police reports and testimonies. The number of victims and the property damage was, however, not well recorded in many instances.

The first major pogrom took place in Odessa in 1821. As we illustrate in Figure 2, the vast majority of pogroms took place in three waves: 1881–1882, 1903–1906, and 1917–1921. Historians have recognized that each of these waves took place during exceptional political and meteorological circumstances (Rogger, 1992b). The first wave of pogroms occurred after the assassination of Tsar Alexander II, who liberated Russia's serfs in 1861. He was killed by the members of a revolutionary organization called the People's Will on March 13, 1881. His death agitated peasants, who feared that the new tsar would reinstitute serfdom. Antisemitic circles spread a rumour that the tsar had been assassinated by Jews (Aronson, 1992, p. 44). Most of the first-wave pogroms were carried out by peasants. The second wave coincided with Russia's abysmal performance and ultimate defeat in the Russo-Japanese War and the revolutionary movement culminating in the enactment of the Russia's first constitution and the formation of the Russia's first parliament (*Duma*). Some of the second wave's large-scale pogroms were organized and carried out by radical monarchist groups known as the "Black Hundreds", who blamed the Jews for the breakdown of social order and for the revolutionary movement. The third wave of pogroms occurred in the midst of the revolutionary agitation of the two 1917 revolutions and the subsequent Civil War. Many third-wave pogroms occurred in localities close to the war front and were carried out in part by peasants, in part by the militia (Encyclopedia Judaica, 2007). Every

pogrom wave took place following severe crop failure (Kenez, 1992; Lambroza, 1992; Aronson, 1992; Slezkine, 2004).

The geography of the pogroms by wave is presented in Figure A1 in the Online Appendix. Localities that were affected by pogroms are scattered throughout the Pale of Settlement. A number of pogroms also took place east of the Pale border, both before and after Jews were allowed to migrate eastward in 1917.[10] In our analysis, we restrict the sample to the grid cells within the Pale because the Jews constituted a much bigger share of the population in the Pale compared to outside the Pale and, as a consequence, pogroms affected a much larger part of the population.

Historians argue that Jews in the Russian Empire were often blamed for "economic exploitation" because of their middleman role in the largely agrarian society (Klier, 2011, pp. 131–132). For example, Aronson (1992, p. 49) stated that before the first pogrom wave, "the peasants suspected that the prices the Jews paid for agricultural produce were exceptionally low and that the interest they took on loans were exceptionally high". Rogger (1992b) also argued that food shortages and high prices for grain in times of crop failure directed the anger of peasants and burghers against the Jews because of their occupation as traders and creditors.

The first historical analyses of pogroms suggested that Russia's government officials helped organize and cover up the pogroms. "The government [...] sought to redirect discontent away from itself toward a more acceptable and defenceless scapegoat" (Lambroza, 1987). However, this view was revised after police records and actions of the authorities to prevent pogroms and to punish the perpetrators were researched. A new historical consensus emerged that pogroms were organized and perpetrated by the mobs, without involvement of the authorities. A survey by Dekel-Chen et al. (2011) summarizes this change: "Later, ... researchers revise[d] this first impression: ... They ... argued that it was impossible to confirm the participation of high officials in the planning of the riots and that, in fact, a great number of perpetrators were punished."

3. DATA AND THE MEASUREMENT OF ECONOMIC AND POLITICAL SHOCKS

To investigate the conditions under which pogroms broke out, we compile data on pogroms and the ethnic composition of the local population by occupation and construct measures of the negative economic shocks that hit the majority, of the episodes of political turmoil that undermined long-term economic relationships, and of prior antisemitism. In this section, we describe the data and present several checks on the measures we have constructed. Summary statistics of all variables used in the analysis are presented in Table 1 in the main text and Table A1 in the Online Appendix.

3.1. *Data sources*

3.1.1. Pogroms. To start, we use data on pogroms compiled by Grosfeld *et al.* (2013). We extend these data by adding a time dimension, *i.e.*, by identifying the exact time of each pogrom using the historical sources. The full list of sources of data on pogroms is provided in the Online Appendix. Because we are interested in the determinants of mob violence, following Grosfeld *et al.* (2013), we exclude from the definition of pogroms few cases of violence against Jews known to be perpetrated solely by the police and the military without the participation of local civilians.

10. Some Jews crossed Pale border despite the restrictions; they were often evicted back to the Pale. In addition, Jews holding the status of the first-guild merchant or possessing a university degree were legally able to live east of the Pale border legally. However, very few attained this social position. According to the 1897 census, only 128,000 Jews lived in the European provinces of the Russian Empire to the east of the Pale of Settlement.

The resulting data set includes information on the location and the date of each pogrom (with few precise dates missing). We georeferenced the locations of all pogroms and built a panel data set at a grid-cell level with 0.5 × 0.5 degrees resolution that covers the period from 1800 to 1927. Because we aim to study politico-economic determinants of ethnic violence, we stop in 1927, because this is the last year before the start of Soviet collectivization, which put an end to individual farming in the vast majority of our sample geography.

We measure violence against the Jews with a dummy variable that takes the value of 1 if a pogrom took place in a given year and grid cell, and 0 otherwise. Systematic data on the number of casualties do not exist: historians give widely varying estimates of the number of casualties and of the extent of property damage for many of the pogroms. There were a total of 638 pogroms in 385 different grid cell × year observations. Panel B of Table 1 summarizes the data on pogroms. The probability that a pogrom occurred in a given grid cell and year is 0.52%, and the average number of pogroms at the grid-cell-year level is 0.0087.

3.1.2. Ethnicity, occupational composition across ethnicities, and other population statistics. To measure ethnic composition by occupation across localities, we digitized the detailed statistical volumes summarizing the 1897 census of the Russian Empire (Troynitsky, 1899–1905). These volumes provide information for 236 districts (*uezds*) inside the Pale of Settlement. We assign district-level census data to grid cells using the following procedure: if a grid cell overlaps with only one district, we assign to this grid cell the value of the corresponding district. When several districts overlap with a given grid cell, we assign to this grid cell the average value of the census data across these districts weighted by the relative size of the areas of each district overlapping with that particular grid cell.[11]

The census volumes report employment by occupation separately for each ethnic group in the Russian Empire; we use these data to measure the share of Jews in each occupation. In the Online Appendix, we present the list of the main ethnic groups that lived in the Pale of Settlement in 1897 with their relative sizes (Online Appendix Table A2), the list of the main occupations with their relative sizes in the total population and among Jews (Online Appendix Table A3), and ethnic composition of middleman occupations (Online Appendix Table A4). Table 1 provides summary statistics for the 1897 census variables at the grid-cell level. Figures A4 and A5 in the Online Appendix present the histograms of the shares of Jews in the local population and in various occupations, as well as the correlation between them.

The 1897 census also provides information on literacy levels in Russian language and in the native language separately for each native language. We use data on the literacy rate among non-Jews to proxy for how exposed they could be to antisemitic books. In the Pale of Settlement, the literacy rate among non-Jews was 20%; among Jews, it was 37%.

We also use data from the 1926 Soviet Census and the 1931 Polish Census to measure the change in the share of Jews and in the share of Jews among traders and creditors from 1897. These two censuses are less detailed than the 1897 census: the 1931 Polish Census does not provide information about occupations by ethnicity, while the 1926 Soviet Census provides information about moneylenders and all traders as a single occupational category.

3.1.3. Grain yields, grain prices, and crop failures. The data on historical grain yields come from Markevich and Zhuravskaya (2018) and Dower and Markevich (2018). The two

11. Figure A3 in the Online Appendix shows the boundaries of grid cells and districts.

sources differ in terms of time coverage and aggregation level. Markevich and Zhuravskaya (2018) have collected information on grain yields at the province level (*gubernia*), *i.e.*, the first-tier subnational administrative division of the Russian Empire, for the 19th century. In this article, we focus on the second half of the 19th century, starting with 1864, because there was a sharp uptick in grain yields and productivity right after the abolition of serfdom in 1861 (Markevich and Zhuravskaya, 2018) and there are no data for 1862 and 1863.[12] Dower and Markevich (2018) provide data on yields separately for winter and spring crops at the district level (*uezd*) for two years: 1913 and 1914. We use these data to construct a proxy for agricultural commercialization at the district level as the share of wheat in total rye and wheat production. The rationale behind this proxy is that rye was used mostly by peasants for their own consumption while wheat was the most important marketable crop. We also use data for the price of rye by province and year in the European provinces of the Russian Empire between 1860 and 1915 from Mironov (1985, pp. 244–252). We deflate these prices to the 1860 level using an aggregate price index for the consumer basket in European Russia from Strumilin (1954, pp. 514–515). Kahan (1989, pp. 108–144) gives a list of crop failures between 1800 and 1914 in the European provinces of the Russian Empire and describes the historical accounts of the climatic shocks that led to these crop failures. In addition, he provides panel data across years and provinces on government assistance in the form of grain provided from the imperial grain reserves to provinces that experienced severe crop failure resulting in famines or near-famine conditions.

3.1.4. Seasonal temperature. To measure economic shocks, we combine data on grain yields, prices, and historical accounts of crop failures with data from climatologists. In particular, we use a data set that provides information on reconstructed historical seasonal temperature. These data were built by Luterbacher *et al.* (2004) and Xoplaki *et al.* (2005) and previously were used by Ashraf and Michalopoulos (2015) and Buggle and Durante (2017). These temperature data were derived from indirect proxies such as tree rings, ice cores, corals, and ocean and lake sediments, as well as archival documents. Luterbacher *et al.* (2004) and Xoplaki *et al.* (2005) reconstructed historical temperature by calibrating the indirect proxies to Climatic Research Unit (CRU) gridded data based on weather station observations by Mitchell and Jones (2005) for the 20th century and extending time series backward for earlier years.

Using this data set, we construct measures of seasonal temperature shocks. For each grid cell in each season, we calculate the deviation of temperature from the historical mean by taking the difference between the temperature in a grid cell in a season in a particular year and the grid-cell-specific 75-year rolling mean temperature. The rolling mean temperature is used to account for the long-term climate change. We then normalize this variable by the grid-cell-specific standard deviation of the season temperature in the corresponding 75-year period to account for variability of climate. (All our results are robust to using the mean season temperature for each grid cell over the entire observation period instead of the 75-year rolling mean.) We then use dummies for the extremely hot and extremely cold seasons for each grid cell in each year. We set these dummies equal to one if the deviation of temperature from the historical mean in the grid cell, season, and year falls above the 95th percentile of its distribution for the extremely hot and below the 5th percentile of its distribution for the extremely cold season temperature.

3.1.5. Political turmoil. To measure an increase in political instability during our observation period, we compile a comprehensive list of historical events that includes all violent

12. Data on grain yield at the province level are not available for the most-western provinces of the Russian Empire, called the Kingdom of Poland. Also, a few cross-sections are missing between 1864 and 1914.

political successions (with a revolt or an assassination), all defeats in wars, all invasions of Russia, all general political strikes, and all revolutions. This list consists of the following historical episodes: defeat in the War of the Third Coalition (1803–1806); defeat in the War of the Fourth Coalition (1806–1807); Napoleon's invasion of Russia (1812), abdication of the throne by Constantine Pavlovich (1825); defeat in the Crimean War (1855–1856), the assassination of Alexander II (1881), conflict in Manchuria (1903); defeat in the Russo-Japanese War (1904–1905), general strikes and the Russia's first revolution (1905), a series of defeats in the First World War (1916–1918), the February and the October revolutions and abdication of throne by Nicholas II (1917), and the Civil War (1918–1922).

These events all shared a common trait: an increase in uncertainty about the future. Some of these events, but not all, also shared another feature, namely, they were associated with weakening of the state, in particular weakening of the state capacity to enforce law and order. For example, the Napoleonic invasion of Russia and the Civil War completely eliminated law enforcement from many areas inside the Pale of Settlement. In contrast, the assassination of Alexander II in 1881 was not associated with a change in the ability of the state to enforce law and order, or with a weakening of any other aspect of state capacity. The change in the identity of the monarch was, however, politically very important. It was associated with a sharp increase in uncertainty about the future for the former serfs (*e.g.* Aronson, 1990), who constituted 43% of all rural Russia's residents (Bushen, 1863) and who feared that they would be forced back into serfdom. Even though serfdom was not reinstated, the assassination of Alexander II led to a substantial reduction in civil liberties and to an abandonment of the Alexander's liberalization reforms.

In the empirical analysis, we define a dummy variable for political turmoil that varies only over time and equals one during years that coincided with the historical events that brought political uncertainty (from the list described above) and one year after these episodes. We treat one year following the episodes of political uncertainty as political turmoil in order to account for the fact that these episodes may have lasting implications. (Below, we establish robustness of our results to different alternative definitions of political turmoil, including a measure based on Russia's sovereign bond yield spread.) The baseline definition of political turmoil is presented on a timeline in Panel C of Figure 2.

3.1.6. Violence against Jews before 1800. We have geocoded locations of all major incidents of violence against Jews that took place before the start of the 19th century on the territory that became the Pale of Settlement near the end of the 18th century. Historians (*e.g.* Klier, 2011, p. 59) name three episodes of violence, in which Jews were among the victims before 1800. The two main episodes were uprisings of the Orthodox majority against the Polish elites: Bogdan Khmelnytsky uprising of 1648 and the Koliyivshchyna rebellion of 1768. Jews became victims of these riots because they often were managers (leaseholders) of Polish estates. Another episode of anti-Jewish violence took place several centuries before, during the Kiev riot of 1113; historians argue that this episode also should not be qualified as a pogrom (Klier, 2011) because, as in the other two episodes, Jews were not the primary target. Figure A6 in the Online Appendix presents spatial distribution of locations of past violence.[13]

3.1.7. Antisemitic books in the 18th century. To construct a measure of antisemitic publications in the 18th century, we digitized Polish national bibliography, *Bibliografia polska*,

13. An important determinant of the locations of violence during the Khmelnytsky's uprising is the distance to Zaporozhian Sich, the origin location of Khmelnytsky's 1648 campaign.

compiled by Karol Estreicher in 1870, which contains all books in the main European and Eastern-European languages published in the Polish-Lithuanian Commonwealth or elsewhere about Poles or Polish culture or by Polish authors; and we use digital bibliography of all nonreligious books published in Russian language in the Russian Empire throughout the 18th century collected by the National Library of Russia.[14] Both Polish and Russian bibliographies contain the place and year of publication and the titles of all books; in addition, the Polish bibliography contains short summaries of the content. We searched for keywords in the titles and summaries of these books suggesting that a book is about Jews or that it contains some antisemitic content (because its title or summary contains derogatory terms used in antisemitic texts). Among the list of books containing these keywords, we have manually coded all antisemitic titles. For each publication town in and around the Pale of Settlement, we count the number of all books and the number of all antisemitic books published in that location. As the final step, we spatially interpolate the number and the share of antisemitic books between the publication towns to get a value of these measures for each grid cell. Figure A7 in the Online Appendix presents spatial distribution of publication towns and the interpolated share of antisemitic books.

3.1.8. Additional variables. To verify that our results are not driven by the effect of shocks on general crime rather than ethnic violence, we use data on the number of thefts, homicides, and arsons per capita. Data on the number of thefts and homicides exist for 1900–1912 and for the number of arsons for 1900–1910. (Data for other years are not available.) The number of arsons comes from MIA (1912). Homicides and thefts come from the annual volumes of the statistics of district courts and the chambers of justice published by the Ministry of Justice in St. Petersburg between 1904 and 1915. We use data from 1905 Land Census assembled by Dower and Markevich (2017) to measure land inequality with a Gini index and tax rates from Nikolay (1890). We also digitized historical railway maps to measure proximity to railways.

3.2. *Economic and political shocks*

3.2.1. Measuring local economic shock: temperature, government grain assistance, and agricultural yields. Throughout the 19th century and in the early 20h century, the Russian Empire had a predominantly agricultural economy: 85% of the working-age population was employed in agriculture in 1885, and by 1913, this figure had declined to 82%. Agriculture contributed the most to the Russia's GDP: about 54% of total value added was produced by agriculture in 1885 and about 47% in 1913 (Cheremukhin *et al.*, forthcoming). Food made up about 55% of the total exports of the Russian Empire, and the empire was the world's greatest grain exporter (Gayle and Moskoff, 2004). Due to the use of primitive technologies, climate shocks had a large effect on grain yield. Historians have pointed out that crop failures due to natural calamities were frequent and had a significant effect on wellbeing (often leading to famines or near-famine conditions), because agriculture was the main sector of the economy, output-to-seed ratio was low, and grain storage was extremely costly (Kahan, 1989, pp. 108–144).

It is a challenging task to measure shocks to agricultural incomes in the 19th century. We use several datasets to build a proxy for local economic shocks. First, we ask which climate shocks, measured by the reconstructed historical seasonal temperature, mattered

14. The sources of Polish bibliography are: *https://www.estreicher.uj.edu.pl/home/* and *https://catalog.hathitrust.org/api/volumes/oclc/3963468.html* (accessed on 15 January 2018). The source of the Russian bibliography is: *http://nlr.ru/rlin/ruslbr_v2.php?database=RLINXVIII* (accessed on 24 January 2018). The bibliography of religious texts published in the Russian Empire is unavailable.

for grain production. Agricultural scientists (*e.g.* Hall, 2001) argue that extremely hot temperatures in the early growing season, often referred to as heat stress, causes grain yield to collapse.[15] In the Pale of Settlement, both winter and spring grains were cultivated, with winter grains constituting the majority. Winter grains in that area are planted in September, sprout in May and June, and are harvested in July and August; spring grains are planted in April and May, sprout in June and July, and are harvested in August and September (Joint Agricultural Weather Facility, and U.S. Department of Agriculture, 1992, p. 139). Given this agricultural calendar, for both winter and spring grains, spring in the reconstructed historical temperature dataset (March, April, and May) represents the early growing season, whereas summer and autumn (*i.e.* June–November) represent the harvesting season.

Using the available historical data on grain yields, we investigate whether and how temperature shocks in each of the seasons, including the growing and the harvesting seasons, affected agricultural output. We regress log yield in a province and year on the temperature shocks in each season in the province and year controlling for year and province fixed effects (to single out the variation that is relevant for the subsequent analysis) and correcting standard errors for spatial correlation within a 250-kilometer radius and one spatial lag.[16] We find that the only seasonal temperature shock that has a significant and robust negative effect on yields across different specifications is the extremely hot spring.[17] The magnitude of this effect is substantial: conditional on province and year fixed effects, an extremely hot spring reduced grain yield in a province in the same year by about 20%.

To sum up, consistent with the climatology and agricultural literature, we find that extremely hot temperatures during the early growing season were detrimental to the main output of the agricultural production in the Pale of Settlement.[18]

15. This literature defines heat stress as the rise in temperature in the growing season beyond a threshold level for a period of time sufficient to cause irreversible damage to plant growth and development. According to agricultural scientists, high temperatures in the early growing season reduce grain yield, and in particular, wheat yield, through interrelated mechanisms: the acceleration of phasic development, accelerated senescence, reduction in photosynthesis, increase in respiration, and the inhibition of starch synthesis in the growing kernel (Shpiler and Blum, 1990). Asseng *et al.* (2015) show that for each additional Celsius degree in global mean temperature, global wheat production drops about 6%.

16. We use 250-kilometer radius for spatial correlation in all regressions where province is the unit of analysis to ensure that error terms from neighbouring provinces are correlated. In all regressions with grid cell as the unit of analysis, the baseline Conley radius is assumed to be 100 kilometers. Below, we show robustness of our results to alternative assumptions about variance–covariance matrix.

17. Table A5 in the Online Appendix presents regression results using dummies for temperature shocks. The only significant coefficient is on the dummy for hot spring. To illustrate the relationship between temperature shocks and yields, Figure A8 in the Online Appendix presents the semiparametric relationship between the deviation of temperature from its grid-specific mean for each season and province grain yield (conditional on province and year fixed effects). The figure confirms a strong relationship between extremely hot spring and crop failure as well as no relationship between other seasonal temperature shocks and yields. In addition, data on yields for spring and winter grains separately are available at the district level for 1913 and 1914. These years, however, were exceptionally hot. Therefore, using these data we cannot study how different weather shocks affected yields. We can, however, verify that yields were lower in districts where spring season was particularly hot during these years. Figure A9 in the Online Appendix presents the unconditional non-parametric relationship between grain yield in 1913 and 1914 at the district level and the deviation of spring temperature from the historical mean separately for spring and winter grains. As above, we find that grain yield for both types of crops collapsed when spring temperature reached extremely high levels in these two years.

18. Agricultural scholars point out that cold winters also could damage the seeds of winter crops and reduce their yields (Braun and Sãulescu, 2002). In addition, extremely cold weather during the later stages of the growing season may also negatively affect yields of both winter and spring crops (Acevedo *et al.*, 2002). Consistent with these mechanisms, Anderson *et al.* (2017) find that colder growing seasons increased the likelihood of Jewish persecutions in the European cities between 1100 and 1600, but not between 1600 and 1800. The time coverage of their study overlaps with the Little Ice Age (LIA). During the LIA, mean annual temperatures declined by 0.6°C relative to the average temperature

As shown above, the incidence of extremely hot spring temperatures has substantial power in predicting crop failures. This prediction is not perfect, however. This is partly because natural calamities other than shocks to seasonal temperature also affected agricultural yields and partly because reconstruction of temperature from indirect sources is prone to errors. In particular, there is no detectable climatic anomaly in the reconstructed climate dataset during the 1880–1882 period. However, the consensus among historians is that agricultural output was exceptionally low, particularly in central Ukraine preceding the first pogrom wave. Kahan (1989, p. 108 and pp. 141–142) indicated the yield and the yield-to-seed ratio were extremely low and that it was caused by a drought. Aronson (1990, p. 112) also indicated that the first pogrom wave was caused by "near-famine conditions" due to crop failures that occurred because of extremely hot weather. Grain prices, which, as we show in the next subsection, were strongly significantly negatively associated with grain output in the most suitable areas of the Pale (*i.e.* Ukraine), also were very high between 1880 and 1882.

Thus, to correct this imperfection in the historical reconstructed weather shocks data, we supplement them with data on government grain assistance from imperial grain reserves, which was given to provinces affected by extreme grain shortages. Kahan (1989) provides the list of provinces and years that got such assistance throughout the 19th century. In the first two columns of Table A6 in the Online Appendix, we show that grain assistance was indeed given to provinces that experienced crop failure and the timing of these grain subsidies coincides with periods of high grain prices. Consistent with historical accounts of crop failures right before and during the first pogrom waves, three provinces in the Southern part of the Pale of Settlement got government grain assistance in 1880 and 1881 and one province in 1882. We deem that these provinces had a local crop failure in these years despite not observing shocks in reconstructed temperature data. Thus, we define a dummy variable for a local economic shock that equals one in all grid cells and years such that spring temperature was extremely high or in grid cells located in provinces that got government assistance in 1880–1882.[19]

The first two columns of Table 2 present the relationship between grain yields and local economic shocks at the province level. We aggregate local economic shocks at the province level by taking a simple average across all grid cells in the same province and year. As above, we correct standard errors for spatial correlation within a 250-kilometer radius and one temporal lag and control for province and year fixed effects. The first column of Table 2 shows that local economic shocks were associated with a significant and large fall in agricultural output. The second column shows that this relationship was contemporaneous, as grain yield was significantly related to a local economic shock in the same year and was unrelated to last year's local economic shock.

3.2.2. Measuring marketwide economic shocks: crop failure in grain-producing regions and grain prices. The Pale of Settlement was a large area, in which different regions

between 1000 and 2000 CE across the Northern Hemisphere. It is documented that during the LIA frequent cold winters and summers led to crop failures and famines in northern and central Europe (Encyclopædia Britannica, 2015). The temperature levels in Europe have increased between the observation periods in Anderson *et al.* (2017) and in our data. The change in climate is the likely reason why crop failures after 1800 occurred following hot growing seasons, whereas they have occurred following cold growing seasons during the LIA.

19. As the baseline, we use hot spring from the reconstructed climate data in all years but 1880–1882 and the fact of getting government assistance in 1880–1882 as the local economic shock. We do not use the government grain-assistance data throughout because the time span of these data is substantially smaller than that of the reconstructed seasonal temperature data. In addition, the government assistance data do not cover a few grid cells in the most western part of the Pale of Settlement. Below, we show the robustness of our results to using only the hot spring from the reconstructed climate data for all years.

TABLE 2
Grain yield, grain price, and the local and marketwide economic shocks

	Log grain harvest: 1864–1914			Log price of rye: 1860–1915				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Local econ shock	−0.209***	−0.213***	−0.196**	0.012	−0.009			
	(0.075)	(0.070)	(0.080)	(0.017)	(0.021)			
Local econ shock lag		0.028		0.016	−0.015			
		(0.114)		(0.019)	(0.020)			
Local econ shock × Political turmoil			−0.033					
			(0.170)					
Local econ shock × High grain suitability					0.035*			
					(0.021)			
Local econ shock lag × High grain suitability					0.061*			
					(0.031)			
Marketwide econ shock						0.053**	0.063***	0.077***
						(0.023)	(0.023)	(0.027)
Political turmoil							−0.055**	−0.026
							(0.022)	(0.027)
Marketwide econ shock × Political turmoil								−0.066
								(0.044)
High grain suitability					−0.039***			
					(0.008)			
Log grain price lag				0.745***	0.695***			
				(0.029)	(0.032)			
R^2	0.593	0.593	0.593	0.804	0.810	0.013	0.022	0.026
Year FE	Yes	Yes	Yes	Yes	Yes	No	No	No
Province FE	Yes	Yes	Yes	No	No	No	No	No
Observations	535	535	535	1,294	1,294	1,312	1,312	1,312
Mean of dependent var.	15.47	15.47	15.47	3.865	3.865	3.866	3.866	3.866
SD of dependent var.	0.724	0.724	0.724	0.216	0.216	0.216	0.216	0.216

Notes: The unit of analysis is province × year. This table presents the impact of local economic shocks, marketwide economic shocks, and political turmoil on log grain harvest between 1864 and 1914 and log of price of rye between 1860 and 1915 at the province level. There are twenty four provinces in the Pale of Settlement, fifteen of which are outside the Kingdom of Poland. Grain yield data are unavailable for provinces inside the Kingdom of Poland. Standard errors are corrected for both spatial and temporal correlations following Hsiang (2010) in a radius of 250 km and 1 temporal lag. ***$P < 0.01$, **$P < 0.05$, *$P < 0.1$.

had varying levels of grain productivity and grain production. Panel A of Figure A10 in the Online Appendix illustrates this. We divide all Pale provinces into two groups: those with above- and below-median suitability for grain cultivation and present the aggregate rye output for the two groups of provinces over time. The figure shows that most of grain was produced in the areas suitable for grain cultivation (*i.e.* Ukraine and Southern Poland). Yet, the Pale of Settlement was small enough to be an integrated market for grain. Provincial prices of grain were highly correlated between provinces within the Pale. Panel B of Online Appendix Figure A10 shows that prices of rye moved together in provinces with above- and below-median grain suitability. In

fact, as shown in Panel A of Online Appendix Figure A11, the Pale of Settlement was integrated with the European grain market, as prices for wheat in Warsaw (one of the major grain markets inside the Pale) and Amsterdam (one of the major European commodity exchanges) were highly correlated.[20]

We define a marketwide economic shock as a dummy that equals one when any of the areas with above-median suitability for grain cultivation inside the Pale had a local economic shock in this or the previous year. This variable is a proxy for periods of high grain prices.[21] Panel B of Online Appendix Figure A10 illustrates that prices, indeed, rose with marketwide economic shocks presented on the figure as shaded areas. It is also clear from the figure that prices were sticky as crop failures in grain-producing regions led to an increase in prices for two consecutive years, which is unsurprising given that a shock to yield affected both the level of consumption in the current year and the stock of seeds available for the next year's planting season (Kahan, 1989).

Columns 4 to 7 of Table 2 present the relationship between grain prices and local and marketwide economic shocks more formally. As prices are strongly correlated across provinces, we do not control for province fixed effects in these regressions. In columns 4 and 5, we control for year fixed effects and show that prices rose for two years following a local economic shock, but only when this local economic shock occurred in areas particularly suitable for grain cultivation. In columns 6 and 7, we omit year effects, as the marketwide economic shocks variable varies only over time, and we verify that the marketwide economic shocks were associated with significantly higher prices.[22]

Overall, the analysis presented in the two subsections above motivates the use of local economic shocks as a measure of local crop failure and the use of a marketwide economic shock as a measure of high grain prices. For a subset of years, for which the grain price data are available, we also use the price of grain directly to measure the market conditions for grain trading.

3.2.3. Checks on political turmoil: sovereign bond yield spread and the severity of economic shocks. In this subsection, we check whether the dummy for political turmoil is a good proxy for political uncertainty. First, we compare political turmoil to quantitative data on the investor confidence in the Russian state. Mauro *et al.* (2002) compiled data on historical yields of sovereign bonds of several states, including the Russian Empire, that were traded on the London Stock Exchange. They reported the series of sovereign bond yield spreads relative to the British consol and showed that these spreads increase with an increase in domestic political uncertainty.[23] Figure A12 in the Online Appendix illustrates that the sovereign bond yield rose sharply at every episode of political turmoil. We construct a dummy for rising sovereign bond yield spread in a particular year equal to one if there was a rise in the (smoothed) series of the sovereign bond yield

20. Province-level prices of wheat are unavailable for all provinces in the Pale of Settlement; thus, we rely on prices of rye in our analysis. We verify, however, that prices of wheat and rye were highly correlated over time using series from the Warsaw grain market; this correlation is presented in in Panel B of Online Appendix Figure A11.

21. By "marketwide economic" shock we mean the price shock that affects price of grain in the entire Pale of Settlement.

22. Table A7 in the Online Appendix shows that the second lag of the local economic shock did not affect prices and that marketwide economic shock affected prices irrespective of whether we control for the level of prices before the occurrence of the marketwide economic shock, *i.e.* two periods ago. This table also documents that grain prices and grain yields are strongly negatively correlated.

23. They discuss specifically several episodes in the Russia's history that are included in our definition of political turmoil.

spread between January of this year and January of the following year. In column 3 of Table A6 in the Online Appendix, we regress this measure on the political turmoil dummy and show that they are strongly positively correlated. Below, in Section 5.4, we show robustness of our results to using the dummy for rising sovereign bond yield spread instead of political turmoil.

Second, it is important to understand whether political turmoil itself was driven by economic shocks and whether those economic shocks that coincided with political turmoil were just more severe than economic shocks that did not trigger political turmoil. Historians do agree that some of the episodes of political turmoil were directly related to economic shocks. For instance, the 1917 revolution in Russia started with bread shortages in the imperial capital, St. Petersburg. Yet, other events, such as uncertainty about the fate of peasants after the assassination of Alexander II or military defeats, were not directly related to economic shocks. We use several measures of severity of economic shocks to check whether those economic shocks that coincided with political turmoil were more severe. In columns 3 and 8 of Table 2, we test whether local economic shocks occurring during political turmoil were associated with a deeper fall in agricultural output and whether marketwide economic shocks in times of political turmoil led to higher grain prices than the economic shocks without political turmoil. In particular, we estimate the coefficients on the interactions of the two types of economic shocks with political turmoil and find that there is no significant difference in severity of economic shocks measured by either grain output or price during and outside political turmoil.

3.3. *Checks on the measures of prior antisemitism*

Both the historical violence against Jews (Voigtländer and Voth, 2012) and the share of antisemitic books (Becker and Pascali, 2019) were used in the previous literature to measure antisemitic attitudes. We follow this literature and use violence against Jews before 1800 and the number of antisemitic books controlling for the total number of published books in the 18th century as measures of preexisting antisemitism. The two measures are correlated with each other as presented in Table A8 and illustrated in Panel A of Online Appendix Figure A13.[24] Consistent with Voigtländer and Voth (2012), we also find evidence of persistence: pogroms between 1800 and 1927 were significantly more likely in places that witnessed violence against Jews in the past, as illustrated in Panel B of Online Appendix Figure A13.

4. THE MAIN ECONOMETRIC SPECIFICATION

We study the determinants of pogrom occurrence by estimating a linear probability model in a panel setting controlling for all time-invariant unobserved characteristics of the localities with grid-cell fixed effects and for over-time variation with year fixed effects. We estimate different versions of the following basic equation:

$$V_{it} = \alpha + \beta E_{it} P_t M_i + \gamma E_{it} + \sigma E_{it} P_t + \delta E_{it} M_i + \theta P_t M_i + \mathbf{X}_{it}^{'} \boldsymbol{\phi} + \mu_t + \eta_i + \varepsilon_{it}, \quad (1)$$

where i indexes grid cells and t indexes years. V stands for violence and V_{it} denotes a dummy for the occurrence of pogroms in a grid cell i in year t. E is a measure of occurrence of a negative economic shock, which could be a local shock, *i.e.* a proxy for local crop failure, or a marketwide economic shock, *i.e.* a proxy for a sharp increase in grain price. Local economic shocks vary both

24. The table presents these correlations using the actual measures of antisemitic books in the sample of publication towns and using the interpolated measures for the full sample of grid cells inside the Pale of Settlement.

across grid cells and over time, whereas marketwide economic shocks vary only over time (in which case there is only one subscript: E_t and the direct effect of E as well as the effect of its double interaction with P, *i.e.* γ and σ, are not identified as E_t and E_tP_t are collinear with year effects). P_t denotes episodes of political turmoil, i.e., the time of extreme political uncertainty about the future; it also varies only over time. M stands for middlemen and M_i denotes the share of Jews among local moneylenders or grain traders. This variable varies only across grid cells, as it comes from 1897 census. μ_t is the year fixed effect and η_i is the grid-cell fixed effect.

$\mathbf{X}_{it}$ denotes the set of control variables. In order to separate the effect of the presence of Jews from their specialization in middleman occupations, we control for interactions of the share of Jews in the local population with economic and political shocks. To make sure that the estimated effects are not confounded with the level of development of the locality or the composition of local economy, we also control for the interactions of both types of shocks with the size of the credit and grain-trade sectors and with a dummy indicating the absence of the credit sector in the locality.[25]

As both ethnic violence and climate shocks are spatially correlated and correlated over time, we correct standard errors for both spatial and temporal correlations following Conley (1999) and Hsiang (2010). In the baseline specification, we assume that error term of each observation is correlated with error terms of all observations within a 100-kilometer radius and one temporal lag of this observation. Online Appendix Figure A3 illustrates the circles with a 100-kilometer radius overlaid with grid cells and district borders: the 100-kilometer Conley radius implies that error terms are correlated for each set of 12 neighbouring grid cells. These correlated neighbouring observations match to at least three and at most 14 neighbouring districts (*uezds*), depending on the district size. We also establish robustness of our results to various alternative assumptions about the variance–covariance matrix.

We estimate this equation with OLS. After presenting the main results, we discuss identification assumptions, consider the most important potential confounds, and present the evidence suggesting that the results are not driven by the endogeneity of the local share of Jews in middleman occupations.[26]

5. RESULTS

5.1. *Pogroms, economic shocks, and political turmoil*

First, we test the relationship between pogroms and economic and political shocks. As a starting point, we verify that, on average, both local and marketwide economic shocks increased the probability of ethnic violence against Jews in the Pale of Settlement. Columns 1 and 2 of Table 3 present the relationship between the probability of pogrom occurrence in a grid cell and the local and marketwide economic shocks controlling only for grid-cell fixed effects. We find a positive

25. In order to keep the same sample across specifications, we define the share of Jews among moneylenders to be equal to zero when there are no moneylenders in the locality, which happens in 1.22% of the sample. Employment in all other considered occupations is above zero in all localities. The results are robust to excluding observations with zero employment in the credit sector.

26. As the baseline, we use dummy variables for economic shocks. Below we also document robustness to using the continuous measure of local economic shocks, namely, the deviation of the local spring temperature from its grid-specific mean and the continuous measure of marketwide economic shocks, namely, grain price. In specifications with continuous measures of economic shocks, we subtract the sample means from all continuous variables before taking interaction terms with other continuous variables. For example, in this case, the main explanatory variable becomes $(E_{it}-\bar{E})P_t(M_i-\bar{M})$ instead of the $E_{it}P_tM_i$. We subtract sample means from continuous variables before calculating interaction terms also when we explore heterogeneity of the main effect with respect to other continuous variables.

and significant correlation. In column 5, we show that the relationship between local economic shocks and pogroms is robust to partialing out all of the over-time variation (presented in Figure 2) with year fixed effects.[27] This relationship was documented in other settings in previous literature (*e.g.* Miguel, 2005; Anderson *et al.*, 2017). However, as we show in Figure 2, not all negative economic shocks led to ethnic violence. Columns 3 and 4 of Table 3 present the effect of the negative economic shocks separately during and outside times of political turmoil. We find that local economic shocks have no effect on pogroms during times of relative political stability: the coefficients on proxies for local and marketwide economic shocks (not interacted with a dummy for political turmoil) are precisely estimated zeros. In contrast, the coefficients on the interactions between the proxies for local and marketwide economic shocks, on the one hand, and political turmoil, on the other hand, are positive, large, and statistically significant. Column 6 shows that the relationship between the local economic shocks, political turmoil, and pogroms is robust to controlling for year fixed effects.[28]

In all tables with occurrence of pogroms as a dependent variable, for convenience of interpretation, we multiply all coefficients by 100, so that the estimated effects are measured in percentage points. The estimates imply that a negative local economic shock during times of political turmoil increased the probability of a pogrom in a grid cell by 2.9 percentage points or 41% of a standard deviation of pogrom occurrence (column 6 of Table 3) and a negative marketwide economic shock increased the probability of pogrom occurrence by 3.8 percentage points (column 4 of Table 3). These effects are large compared to the mean probability of a pogrom in a grid cell in any given year from 1800 to 1927, which is 0.52%.[29]

The effect of spring temperature on pogroms is illustrated in Figure 4, which shows a sharp increase in the likelihood of a pogrom in the grid cells and years that experienced an extremely hot spring. The figure presents the unconditional, non-parametric relationship between the occurrence of pogroms and the deviation of spring temperature in a grid cell and year from its historical grid-specific mean. Two lines represent this relationship in a sample of all years and in a sample of years during political turmoil. Comparing the two lines, one can see that the likelihood of pogroms is generally much higher during times of political turmoil, particularly in localities that were affected by an extremely hot spring.

The last column of Table 3 shows that pogroms during political turmoil and local economic shocks were more likely in localities with a larger Jewish community relative to the size of the

27. The direct effect of the marketwide economic shock cannot be estimated with year fixed effects because it varies only over time.

28. As shown in Figure 2, there is a gap during the second wave of pogroms in 1905 between the timing of two consecutive economic shocks of 1903–1904 and 1906–1907. Yet, 1905 coincided with political turmoil (defeat in the Russo-Japanese War and the first Russian Revolution). Pogroms of 1905 manifest themselves in a positive and significant coefficient on the political turmoil dummy. However, one could argue that in 1905, people still felt the consequences of the economic shock of the preceding years. As we show below, our results are robust to adding more lags to the definition of marketwide economic shock. In the specification of column 4 of Table 3, adding one more lag to marketwide economic shock makes the coefficient on the dummy for political turmoil a precisely estimated zero (instead of 0.86 with an SE of 0.16, the coefficient on political turmoil becomes equal to 0.04 with an SE of 0.03), while the coefficient on the interaction term between marketwide economic shock and political turmoil remains large and statistically significant.

29. Theoretically, hot weather *per se* might lead to more anti-Jewish violence during political turmoil by making people too hot and, as a result, agitated and violent. Experimental studies in psychology showed that higher ambient temperatures may increase interpersonal hostility (Kenrick and MacFarlane, 1986; Vrij *et al.*, 1994). In Table A9 in the Online Appendix, we show that pogroms were affected by local economic shocks through their effect on agricultural incomes rather than directly: we regress the probability of pogrom occurrence during the summer time, (*i.e.* between June and August) on the incidence of extremely hot spring, *i.e.* growing season (March to May). This temperature shock affected the subsequent yields. We find a significant positive relationship between hot spring interacted with political turmoil and pogroms in the summer, irrespective of whether we directly control for the temperature shocks during the summer.

TABLE 3
Pogrom occurrence at the intersection of economic shocks and political turmoil

	Pogrom occurrence						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Local econ shock	1.757***		0.003		1.088*	0.047	0.058
	(0.474)		(0.038)		(0.584)	(0.061)	(0.062)
Marketwide econ shock		1.374***		0.002			
		(0.205)		(0.022)			
Political turmoil			1.799***	0.861***			
			(0.214)	(0.162)			
Local econ shock × Political turmoil			2.752***			2.937*	3.274**
			(0.971)			(1.620)	(1.602)
Marketwide econ shock × Political turmoil				3.810***			
				(0.591)			
Local econ shock × Political turmoil × Share of Jews							33.637*
							(19.367)
Political turmoil × Share of Jews							0.335
							(1.610)
Local econ shock × Share of Jews							−0.105
							(0.859)
Marketwide econ shock × Political turmoil × Share of Jews							−0.856
							(9.646)
Marketwide econ shock × Share of Jews							−0.049
							(0.592)
R^2	0.016	0.019	0.032	0.043	0.113	0.114	0.115
Grid FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	No	No	No	Yes	Yes	Yes
Observations	73,728	73,728	73,728	73,728	73,728	73,728	73,728
Mean of dependent var.	0.522	0.522	0.522	0.522	0.522	0.522	0.522
SD of dependent var.	7.207	7.207	7.207	7.207	7.207	7.207	7.207

Notes: The unit of analysis is grid cell × year. The dependent variable is a dummy variable that takes the value of 1 if a pogrom occurred in a given year and grid cell, and 0 otherwise. We multiply the dependent variable by 100 to measure all coefficients in percentage points of probability of pogrom occurrence. This table presents the results of regressions in which the probability of a pogrom in a grid cell and year is related to local economic shocks, marketwide economic shocks, and political turmoil. Standard errors are corrected for both spatial and temporal correlations following Hsiang (2010) in a radius of 100 km and 1 temporal lag. ***$P < 0.01$, **$P < 0.05$, *$P < 0.1$.

population. One could expect this, given that Jews were a minority everywhere in the Pale. (The share of Jews across grid cells varied from 0.9% to 24.9%.) A one-standard-deviation increase in the share of Jews in the local population, on average, was associated with 1.7 percentage point increase in the probability of pogroms in localities hit by local economic shock during political turmoil. The share of Jews in the local population interacted with the marketwide economic shock did not significantly affect the probability of pogrom occurrence.

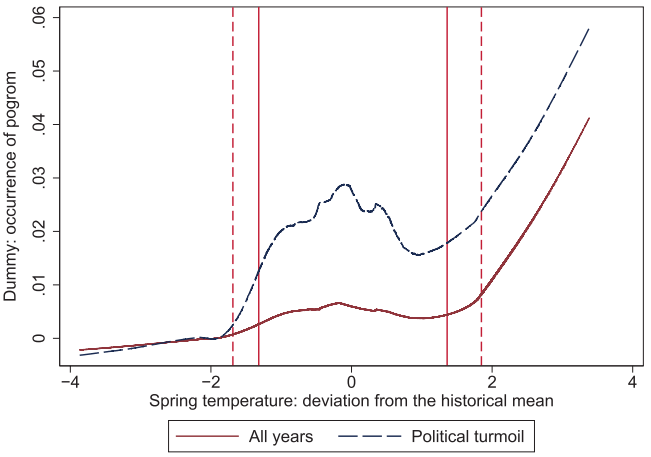

FIGURE 4

Pogroms, spring temperature, and political turmoil.

Notes: This figure presents unconditional nonparametric locally weighted regressions (LOWESS) between pogrom occurrence in a grid cell and year and the deviation of spring temperature in a grid cell and year from the historical mean (for all years and for the years with political turmoil). From left to right, the dashed vertical lines indicate the 5th and the 95th percentiles of the distribution of deviation of spring temperature from the historical mean and solid vertical lines indicate the 10th and the 90th percentiles of this distribution. The top and the bottom 0.5% of the distribution of temperature deviations are excluded.

5.2. *Pogroms and Jewish middlemen*

The next step in our analysis is to explore whether and how pogroms were related to the economic activity of Jews. First, we are interested in whether the Jewish domination of middleman occupations related to agriculture—credit and grain trade—affected pogroms. In the first two columns of Table 4, we test whether the share of Jews in these sectors had an effect on the probability of pogroms at the intersection of local economic shocks and political turmoil. To single out the effect of occupational specialization, in all specifications we control for the share of Jews in the local population interacted with economic and political shocks separately and together in addition to grid-cell and year fixed effects.[30] Column 1 shows that the coefficient on the triple interaction of the share of Jews among moneylenders with local economic shock and political turmoil is positive, large, and statistically significant, while the coefficient on the share of Jews in the population declined by 39% compared to a specification without the share of Jews among creditors (last column of Table 3) and lost statistical significance.

In column 2 of Table 4, we repeat this exercise for the share of Jews among grain traders and find no significant effect of the interaction of this variable with the local economic shock and political turmoil, yet, there is a significant effect of the interaction of the share of Jews among grain traders with political turmoil. One could hypothesize, however, that the presence of traders who belong to a minority group may matter for ethnic violence when prices of the traded commodity are particularly high. Thus, in column 3, we estimate a specification where we substitute local economic shocks by the marketwide economic shocks. We find that the effect of the share of Jews among grain traders during political turmoil presented in column 2 comes entirely from the years when political turmoil coincided with marketwide economic shocks. The coefficient on the triple interaction between the share of Jews among grain traders, the marketwide economic

30. The share of Jews in a locality is positively, but not very strongly, correlated with both the share of Jews among moneylenders and the share of Jews among grain traders, as can be seen from Panels A and B of Figure A5 in the Online Appendix.

TABLE 4
Pogroms and the Jewish domination over moneylending and grain trade

	Pogrom occurrence				
	(1)	(2)	(3)	(4)	(5)
Local econ shock × Political turmoil × Sh. Jews among creditors	6.56***				5.95**
	(2.47)				(2.48)
Local econ shock × Political turmoil × Sh. Jews among grain traders		0.82			
		(4.41)			
Marketwide econ shock × Political turmoil × Sh. Jews among grain traders			9.38***	9.42***	10.13***
			(2.75)	(2.69)	(2.83)
Political turmoil × Sh. Jews among creditors	0.34				0.08
	(0.62)				(0.61)
Local econ shock × Sh. Jews among creditors	−0.01				−0.02
	(0.10)				(0.10)
Political turmoil × Sh. Jews among grain traders		3.03***	0.05		
		(0.91)	(0.56)		
Local econ shock × Sh. Jews among grain traders		0.21			
		(0.19)			
Marketwide econ shock × Sh. Jews among grain traders			0.00		
			(0.15)		
Local econ shock × Political turmoil	3.34**	3.32**			4.23***
	(1.59)	(1.57)			(1.59)
Local econ shock × Political turmoil × Share of Jews	20.51	38.02*			26.53
	(18.05)	(21.00)			(19.61)
R^2	0.118	0.119	0.118	0.118	0.123
Grid and year FE	Yes	Yes	Yes	Yes	Yes
Other interactions with the share of Jews	Yes	Yes	Yes	Yes	Yes
Interactions with the share of creditors in total employed	Yes	No	No	No	Yes
Interactions with the share of grain traders in total employed	No	Yes	Yes	Yes	Yes
Observations	73,728	73,728	73,728	73,728	73,728
Mean of dependent var.	0.522	0.522	0.522	0.522	0.522
SD of dependent var.	7.207	7.207	7.207	7.207	7.207

Notes: The unit of analysis is grid cell × year. The dependent variable is a dummy variable that takes the value of 1 if a pogrom occurred in a given year and grid cell, and 0 otherwise. We multiply the dependent variable by 100 to measure all coefficients in percentage points of probability of pogrom occurrence. This table presents OLS regression results in which the probability of a pogrom in a grid cell and year is related to the local shares of Jews among moneylenders and grain traders, local economic shocks, marketwide economic shocks, and political turmoil, controlling for year and grid-cell fixed effects as well as the share of Jews and the shares of middleman sectors in local employment interacted with economic and political shocks. Standard errors are corrected for both spatial and temporal correlations following Hsiang (2010) in a radius of 100 km and 1 temporal lag. ***$P < 0.01$, **$P < 0.05$, *$P < 0.1$.

shock, and political turmoil is positive, significant, and three times as large as the coefficient on the interaction between the share of Jews among grain traders and political turmoil from column 2. At the same time, we find that there is no additional explanatory power of the interaction of the share of Jews among grain traders and political turmoil as the coefficient on this interaction becomes precisely estimated zero. None of the double interactions with political turmoil and with marketwide economic shocks are significant in the specification of column 3; in column 4 of Table 4, we combine P_t and E_t in a *single* dummy indicating years when the periods of political turmoil coincided with marketwide economic shocks. We do this to avoid adding noise to the estimation. The results of this more parsimonious specification are similar, and the effect of the

domination of Jews over trading in grain at the time of marketwide economic shock and political turmoil is even more precisely estimated.

In column 5 of Table 4 we jointly estimate the effects of the share of Jews among creditors interacted with the local economic shock and political turmoil and of the share of Jews among grain traders interacted with marketwide economic shock and political turmoil. Both effects are positive and statistically significant in contrast to the effect of the share of Jews in the local population interacted with both types of shocks, which remains statistically insignificant.[31]

Thus, we conclude that Jewish domination over moneylending mattered for pogroms during political turmoil in localities that were directly affected by crop failures, whereas Jewish domination over trade in grain was important for pogroms during political turmoil when grain prices were high in all localities where there was a market for grain, not only those where grain was produced. Presumably, local shocks were more important for creditors and marketwide shocks were more important for grain traders because of the differences in the nature of these middleman occupations. Credit was supplied to peasants in localities where grain was cultivated, and the crop failure meant the inability of peasants to repay the loan. In contrast, grain traders brought grain to urban areas, which were the locus of the demand for grain consumption, and also acted as a supplier of seeds to peasants during the planting season when there were shortages of the seeding material. In times of marketwide economic shocks, *i.e.*, higher grain prices, the buyers of grain both in urban and rural areas were unable to pay.

How does the effect of the presence of Jews in middleman occupations on pogrom occurrence compare to the effects of other occupations of Jews? We explore this question in Table 5. The considered occupations are: creditors, grain traders, traders of nonagricultural goods, employed in general trade (mostly peddlers), craftsmen, employed in transport, and employed in agriculture. Together they cover 58% of total Jewish employment.[32] Panel A of the table tests for the effect of the presence of Jews in all main occupations interacted with local economic shock and political turmoil and Panel B considers the effect of the presence of Jews in those occupations, but interacted with marketwide economic shock and political turmoil. Columns 1 to 7 present results where we estimate the effects of the shares of Jews among different occupations one-by-one, and column 8 presents a horse race between all occupations.[33]

We find that it is the presence of Jews in middleman occupations related to agriculture and not in other occupations that increases the likelihood of pogroms. The only robust positive effects are those that we already presented in Table 4, *i.e.*, those of the shares of Jews among moneylenders and grain traders. There are no significant positive effects of any other profession in times of marketwide or local economic shocks. We find only that the presence of Jews among craftsmen and agriculture interacted with political turmoil (without economic shocks) has a negative and

31. Online Appendix Figure A14 illustrates these effects. Panel A presents the cumulative distribution functions of the share of Jews among local moneylenders, separately for the grid cells that did and that did not experience pogroms among those grid cells that were hit by a local economic shock during political turmoil. Panel B presents the cumulative distribution functions of the share of Jews among grain traders separately for grid cells that did and that did not experience pogroms during the intersection of marketwide economic shocks with political turmoil. In both cases, distributions for localities that experienced pogroms are shifted to the right, illustrating that the effect of the presence of Jewish middlemen on pogroms is not concentrated at a particular part of its distribution.

32. All other occupations are much smaller, with the exception of trade in other agricultural goods. We do not consider trade in other agricultural goods because it is highly correlated with trade in grain.

33. The specification in Table 5 does not include the controls for the interactions of shocks with sector shares (unlike Table 4), in order to make all coefficients directly comparable to the last column, where we pool all the effects together. We also consider local and marketwide economic shocks separately to avoid multicollinearity when we include more than one occupation.

TABLE 5
The shares of Jews in all major occupations, economic shocks, and political turmoil

Panel A: The effects of local economic shocks	Pogrom occurrence							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Local econ shock × Pol. turmoil × Sh. Jews among creditors	6.24***							6.92**
	(2.36)							(2.87)
Pol. turmoil × Sh. Jews among creditors	−0.73							−0.07
	(0.55)							(0.64)
Local econ shock × Pol. turmoil × Sh. Jews among grain traders		2.18						−0.88
		(4.45)						(5.53)
Pol. turmoil × Sh. Jews among grain traders		2.35***						3.79***
		(0.86)						(0.98)
Local econ shock × Pol. turmoil × Sh. Jews among non-agric. traders			2.29					−6.38
			(3.55)					(5.59)
Pol. turmoil × Sh. Jews among non-agric. traders			0.29					0.56
			(0.92)					(1.59)
Local econ shock × Pol. turmoil × Sh. Jews among general traders				3.18				5.70
				(3.47)				(6.72)
Pol. turmoil × Sh. Jews among general traders				0.10				−0.48
				(0.77)				(1.45)
Local econ shock × Pol. turmoil × Sh. Jews among craftsmen					8.50*			1.54
					(5.13)			(7.31)
Pol. turmoil × Sh. Jews among craftsmen					−3.83***			−4.06***
					(1.25)			(1.58)
Local econ shock × Pol. turmoil × Sh. Jews among transporters						1.83		−3.75
						(3.29)		(3.87)
Pol. turmoil × Sh. Jews among transporters						−1.30		−0.97
						(0.83)		(1.05)
Local econ shock × Pol. turmoil × Sh. Jews among peasants							131.37	122.65
							(159.61)	(156.28)
Pol. turmoil × Sh. Jews among peasants							−78.33***	−44.05**
							(23.74)	(21.60)
R^2	0.117	0.116	0.116	0.116	0.116	0.116	0.116	0.119

TABLE 5
(Continued)

Panel B: The effects of marketwide economic shocks	Pogrom occurrence							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Marketwide econ shock × Pol. turmoil × Sh. Jews among creditors	1.52							2.14
	(1.48)							(1.83)
Marketwide econ shock × Pol. turmoil × Sh. Jews among grain traders		8.19***						9.41***
		(2.51)						(2.89)
Marketwide econ shock × Pol. turmoil × Sh. Jews among non-agric. traders			4.01					2.09
			(2.47)					(3.91)
Marketwide econ shock × Pol. turmoil × Sh. Jews among general traders				2.86				−1.43
				(2.08)				(3.66)
Marketwide econ shock × Pol. turmoil × Sh. Jews among craftsmen					−2.66			−6.32
					(3.40)			(4.57)
Marketwide econ shock × Pol. turmoil × Sh. Jews among transporters						−0.50		−2.86
						(2.29)		(2.97)
Marketwide econ shock × Pol. turmoil × Sh. Jews among peasants							−134.90*	−71.28
							(75.10)	(69.55)
R^2	0.114	0.116	0.114	0.114	0.114	0.113	0.114	0.117
Grid and year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interactions with the share of Jews	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interactions with local economic shocks in Panel A	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	73,728	73,728	73,728	73,728	73,728	73,728	73,728	73,728
Mean of dependent var.	0.522	0.522	0.522	0.522	0.522	0.522	0.522	0.522
SD of dependent var.	7.207	7.207	7.207	7.207	7.207	7.207	7.207	7.207

Notes: The unit of analysis is grid cell × year. The dependent variable is a dummy variable that takes the value of 1 if a pogrom occurred in a given year and grid cell, and 0 otherwise. We multiply the dependent variable by 100 to measure all coefficients in percentage points of probability of pogrom occurrence. This table presents OLS regression results in which the probability of a pogrom in a grid cell and year is related to the economic shocks, political turmoil, and the shares of Jews among the locally employed in different occupations, controlling for year and grid-cell fixed effects, and the interactions of the share of Jews with economic and political shocks. Panel A focuses on local economic shocks and Panel B focuses on marketwide economic shocks. Standard errors are corrected for both spatial and temporal correlations following Hsiang (2010) in a radius of 100 km and 1 temporal lag. ***$P < 0.01$, **$P < 0.05$, *$P < 0.1$.

significant effect on the probability of pogroms. Below, in Section 5.3.2, we discuss the effect of the presence of Jews in agriculture. As far as Jewish craftsmen are concerned, this was one of the most popular and the least narrowly defined occupations among the Jewish occupations. Thus, the negative coefficient could be due to a residual effect, *i.e.*, the choice of Jews not to

engage in middleman occupations may be associated with a larger number of cobblers, tailors, watchmakers, and other craftsmen.[34]

Overall, the evidence presented in Tables 4 and 5 shows that the domination of the Jewish minority in moneylending and grain trading, but not in other occupations, significantly increased the likelihood of anti-Jewish violence in the face of economic shocks coinciding with political turmoil; economic shocks without political turmoil did not cause pogroms.

The magnitudes of these effects are large and similar for the two middleman occupations. According to point estimates reported in column 7 of Table 4, a one-standard-deviation increase in the share of Jews among creditors (=0.28), conditional on the local share of Jews, led to an increase in the probability of a pogrom by 1.7 percentage points, *i.e.*, 23% of a standard deviation of pogrom occurrence. If one compares localities hit by a local shock with no Jews among creditors and with all creditors being Jews, the difference in the probability of pogroms between these localities during political turmoil was 6 percentage points. A one-standard-deviation increase in the share of Jews among grain traders (which is equal to 18 percentage points) increased the probability of pogrom occurrence by 1.8 percentage points, or 25% of the standard deviation of pogrom occurrence. If one compares those localities in which Jews constituted 17.6% of all grain traders (the minimum across localities in the Pale) to those localities where all grain traders were Jews, the difference in the probability of pogrom occurrence in times of high grain prices during political turmoil was 8.3 percentage points. In localities hit by the local economic shock during the times when marketwide economic shock coincided with political turmoil, the predicted probability of pogrom varies from zero in localities where middlemen were not Jewish to 12.5% in localities where all middlemen were Jews.

As we mentioned above, once we account for the variation in the share of Jews among middleman, the effect of the share of Jews in the local population is not statistically different from zero. However, to evaluate the importance of the Jewish presence in middleman occupations, we can use the magnitude of the effect implied by the point estimate of the (insignificant) coefficient on the share of Jews interacted with local economic shock and political turmoil (in column 7 of Table 4) and compare the magnitudes with the effects of the share of Jews among middlemen. Consider an average grid cell with total population of 73,891, including 7,645 Jews. This grid cell had only 28 creditors, of whom 15 were Jewish; and 367 grain traders, of whom 323 were Jewish. To get an increase in the probability of pogroms at the intersection of the economic shock and political turmoil equivalent to just *one* additional Jewish creditor and *one* additional Jewish grain trader, one needs to increase the Jewish population of the grid cell by 670 people. As the effect of the share of Jews is imprecisely estimated, we cannot have much confidence in this comparison. However, the upper end of the confidence interval for the coefficient on the interaction between the share of Jews, local economic shock, and political turmoil implies that the number of Jews in the mean grid cell needed to increase by more than 360 people to produce an effect on the probability of pogroms equal to the one generated by adding just one Jewish creditor and one Jewish grain trader. These numbers suggest that the domination of middleman occupations by the Jews was a crucial factor in starting pogroms; *i.e.*, the origin of pogroms at the intersection of economic shocks with political turmoil was related to the presence of Jewish moneylenders and grain traders, despite the fact that the victims of large pogroms included people of both genders, all ages, and many occupations.

34. In addition, among all occupations, the share of Jews among craftsmen correlates most with the share of Jews, as can be seen on Online Appendix Figure A5. The inclusion of the share of Jews in the local population together with the share of Jews in crafts into the set of covariates may result in multicollinearity.

5.3. *Identification*

Below, in Section 5.4, we document robustness of our main empirical findings. In particular, we replicate our results using alternative measures of local economic shocks, marketwide economic shocks, and political turmoil. We show that the results hold under different assumptions about the structure of clusters in the error term, in different subsamples, and with different aggregation levels. We also use a battery of additional controls. In this section, we consider the assumptions under which our results can be interpreted as causal.

5.3.1. Identification assumptions and potential biases. The identification assumption behind the OLS estimates reported in the previous two subsections is that, conditional on grid-cell and year fixed effects, the share of Jews among middlemen interacted with political and economic shocks is exogenous and properly measured. This assumption is most certainly violated. Both endogeneity and measurement error may bias the estimation of equation (1). There are no credible instruments for the share of Jews among middlemen. Therefore, we need to understand the sources and the direction of potential biases and try to directly control for variables that, if omitted, could generate an upward bias in our estimates.

Let us consider the sources of downward and upward biases in order. First, there is a substantial measurement error in the shares of Jews in middleman occupations and in economic and political shocks. The error in the proxies for the shocks comes from the crudeness of the measurement inherent in research using historical data. As far as the share of Jews in middleman occupations, and, particularly, in moneylending is concerned, historians document that many Jews whose reported primary occupation was inn and bar owners and leaseholders also lent money to the Gentile majority at interest (e.g. Cahnman, 2004). As these measurement errors most likely are uncorrelated with pogroms, they lead to an attenuation bias in our estimates.

Second, reverse causality is a possible concern. The data on the ethnic and occupational composition come from the 1897 census, which took place after the first pogrom wave of 1881–1882. The share of Jews in the local population and the ethno-occupational structure of localities were affected by the first pogrom wave, as it caused a significant number of Jewish deaths in some areas and also triggered substantial outmigration of Jews to the U.S. and to large cities, in which it was easier to hide.[35] We do not have data about local ethnic or occupational composition before 1897. However, we do have some data on local ethnic composition from the 1926 Soviet Census and the 1931 Polish Census, which together cover the area of the Pale of Settlement, and also on the number of Jews employed in trade and credit (as a single category) from the 1926 Soviet Census. In order to illustrate how local ethnic and occupational composition reacted to pogroms, we regress the changes in the share of Jews in the local population and in the share of Jews in local trade and the credit sector between 1897 and the subsequent census on the number of pogroms that occurred between the two censuses. We find that there is a negative significant relationship between the change in the population share of Jews and in Jewish share among middlemen (conditional on the change in the Jewish population share) and the number of pogroms in a locality once we eliminate from the sample one outlier – Kiev, *i.e.* the largest town in Ukraine. We describe the exact specifications, present regression results, and plot the residual scatterplots together with the regression lines in Figure 5. This relationship suggests that Jews fled pogroms and Jews in middleman occupations fled pogroms more than proportionally to the rest of the Jewish population. The outlier Kiev is the exception that proves the rule, as in the big

35. The outmigration of Jews from the Russian Empire following pogrom waves originated not only from localities where pogroms took place, but also from localities where violence did not occur, but Jews nonetheless feared pogroms (Spitzer, 2015b).

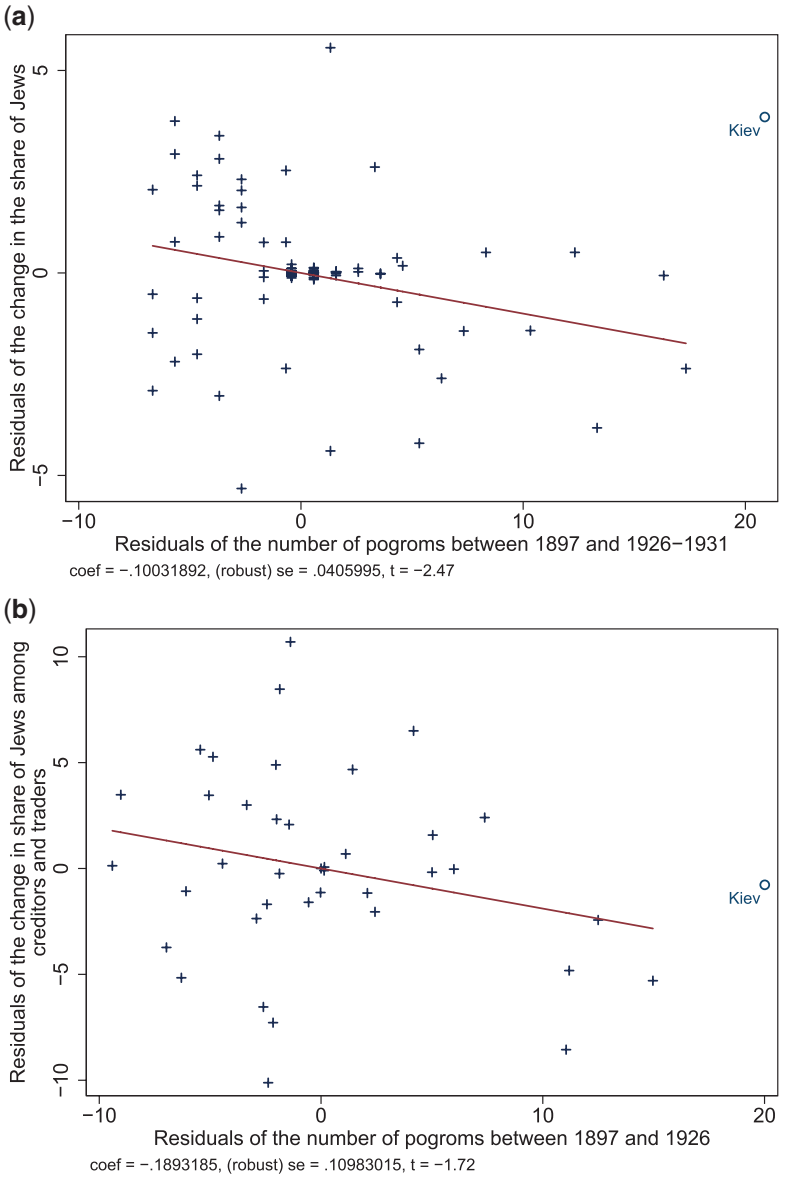

FIGURE 5

Reaction of Jews to the pogroms. (a) Correlation between the number of pogroms and the change in the share of Jews (all Pale). (b) Correlation between the number of pogroms and the change in the share of Jews among middlemen.

Notes: This figure presents the partial correlation between the number of pogroms, on the one hand, and the change in the share of Jews (Panel A) or the change in the share of Jews among middlemen (Panel B), on the other hand, from the OLS estimates. The scatter plot in Panel A is conditional on country fixed effects; and that in Panel B is conditional on country fixed effects, the change in the share of Jews, and the change in the share of middleman occupations in total employment. The regression lines and regression output are reported for regressions without Kiev (which is presented on the scatter plots separately). Heteroskedasticity-robust standard errors are reported. In Panel B, the sample of locations that were in the USSR in 1926 are used due to data limitations.

cities the probability of being hit by a pogrom for any individual Jewish family was substantially lower than in any of the shtetls or smaller towns and villages because the number of victims as a share of the total Jewish population was much smaller in the large cities in case of a pogrom. Thus, pogroms made Jews move to Kiev in order to hide. Below, in Section 5.4, we show that our results are unaffected by the exclusion of large cities from the sample. Outside large cities, reverse causality generates a downward bias in our estimates.

Third, a bias could arise from omitted variables. In contrast to measurement error and reverse causality, omitted variables could bias our estimates in both directions depending on the sign of their correlation with pogroms and the share of Jews in middleman occupations. In particular, a preexisting propensity to ethnic violence could explain the occurrence of pogroms in different localities, and it could correlate with the share of Jews among creditors and grain traders. If Jewish middlemen self-selected into places where the non-Jewish majority was less prone to ethnic violence, such endogenous location decisions would create a negative bias in the relationship between pogroms and the share of Jews in middleman occupations. In contrast, if Jews, for some reason, constituted a larger share of middlemen in places which were more prone to anti-Jewish pogroms, the bias in the OLS estimates would be positive. Thus, the main threat to identification is the possibility that in localities with higher preexisting antisemitism, Jews were somehow pushed into middleman occupations; and this preexisting antisemitism and not the ethno-occupational composition was the driver of pogroms. However, this possibility is extremely unlikely. First, grain traders and creditors were not common Jewish occupations (0.1% of Jews were creditors and 5% were grain traders): thus, for a Jew in the Pale of Settlement, there was always a choice of crafts (25.3% of all Jews were craftsmen) or general trade, *i.e.*, trade in non-agricultural goods (17.6% of Jews were traders of non-agricultural goods) as a career path. There were restrictions on occupational choice of Jews, but they only concerned involvement of Jews in agriculture. Jews were forbidden to work on the land or own it in most parts of the Pale (outside Novorossiya). The Russian crown wanted to protect the majority from ethnic competition and agriculture was the main traditional occupation of the majority. In the next subsection, we address directly the possibility that the share of Jews among creditors and moneylenders is correlated with the measures of preexisting antisemitism.

5.3.2. Preexisting antisemitism and selection on observed and unobserved variables. As described in the data section, we have constructed two alternative proxies for anti-Jewish sentiment before the start of our observation period, *i.e.*, 1800: past anti-Jewish violence and antisemitic books. In Table 6, we present correlations between the measures of prior antisemitism and the shares of Jews among creditors and grain traders across grid cells. The dependent variables are the dummy for past violence in columns 1 to 4 and the interpolated share of antisemitic books in columns 5 to 8. In columns 1 and 5, we show that conditional on the share of Jews, the share of Jews among creditors is uncorrelated with either measure of preexisting antisemitism, whereas the share of Jews among grain traders is positively and significantly correlated with both measures.[36]

In the rest of the table, we explore what drives this correlation. First, we look at how the correlation between ethnic segregation in trade in grain and preexisting antisemitism compares to the correlation between ethnic segregation in other occupations and preexisting antisemitism; namely, we test whether the correlation of antisemitism with the share of Jews in trade in grain is higher than that with the share of Jews in other professions. Following

36. In all regressions with past violence as a dependent variable, we control for the distance to the geographical origin of Khmelnytsky's 1648 uprising to account for travel costs of Khmelnytsky's Cossacks. This control increases the precision of estimated coefficients of interest.

TABLE 6
Correlation between past anti-Jewish violence, antisemitic books in 18th century, and Jewish occupations

	Occurrence of past anti-Jewish violence				The share of antisemitic books			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share of Jews among creditors	9.11	−7.35	−7.89	−0.54	0.011	−0.005	−0.008	−0.010
	(13.79)	(15.71)	(15.97)	(14.28)	(0.017)	(0.018)	(0.018)	(0.019)
Share of Jews among grain traders	44.002***	15.63	22.23	−5.50	0.028*	−0.003	0.000	−0.006
	(15.03)	(16.46)	(14.49)	(15.52)	(0.017)	(0.016)	(0.014)	(0.010)
Occupational segregation index		78.93**				0.082**		
		(36.50)				(0.039)		
Occupational segregation index (w/o middlemen)			75.07**				0.087**	
			(33.17)				(0.036)	
Share of Jews among peasants				−1,702.29***				−0.693
				(587.41)				(0.537)
Share of Jews among non-agr. traders				23.73				−0.013
				(22.34)				(0.023)
Share of Jews among general traders				40.29*				0.042**
				(24.01)				(0.021)
Share of Jews among craftsmen				−49.80**				0.056
				(25.02)				(0.040)
Share of Jews among transporters				49.73***				0.013
				(17.50)				(0.035)
Share of Jews	82.10	67.15	56.91	241.08***	0.233***	0.193***	0.170**	0.086
	(63.23)	(55.64)	(55.50)	(91.81)	(0.073)	(0.067)	(0.068)	(0.112)
Distance to the origin of 1648 uprising (km)	−2.30**	−3.02***	−3.19***	−4.35***				
	(1.05)	(1.10)	(1.13)	(1.02)				
R^2	0.091	0.113	0.113	0.245	0.128	0.146	0.152	0.164
Observations	576	576	576	576	576	576	576	576

Notes: The unit of analysis is a grid cell. In columns 1–4, the dependent variable is a dummy variable that takes the value of 1 if violence against Jews occurred in the grid cell before 1800, and 0 otherwise. We multiply this dependent variable by 100 to measure all coefficients in percentage points of probability of occurrence of past violence against Jews. In columns 5–8, the dependent variable is the interpolated share of antisemitic books in total books published. Standard errors are corrected for spatial correlation following Hsiang (2010) in a radius of 100 km. ***$P < 0.01$, **$P < 0.05$, *$P < 0.1$.

Reardon and Firebaugh (2002) and Alesina and Zhuravskaya (2011), for each grid cell, we calculate the ethno-occupational segregation index across all occupations between Jews and Gentiles.[37] This variable measures to what extent Jews and Gentiles tend to engage in different occupations in a particular locality, giving larger weights to occupations with a larger share of total

37. In our case of two groups–Jews and Gentiles–the multigroup segregation formula from Alesina and Zhuravskaya (2011) simplifies to: $S_i = \sum_{l=1}^{L} \frac{N_i^l}{N_i} \frac{\left(J_i^l - J_i\right)^2}{J_i(1-J_i)}$, where superscript l indexes occupations and subscript i indexes localities; L is the total number of occupations; N_i is the total population of the locality; N_i^l is total employment in occupation l in

employment. Columns 2 and 6 show that in places with higher preexisting antisemitism, the two groups tend to have more different occupations than they do in places with lower preexisting antisemitism. Furthermore, conditional on the overall segregation, the share of Jews among grain traders is no longer positively significantly correlated with the measures of preexisting antisemitism. The share of Jews among grain traders, however, is one of the components of the overall ethno-occupational segregation index. In order to make sure that the correlation between the segregation index and preexisting antisemitism does not reflect a particular (non-linear) functional form in a relationship between the share of Jews among middlemen and preexisting antisemitism, in columns 3 and 7, we use a segregation index, which considers all occupations but moneylending and grain trading. Similarly to columns 2 and 6, this segregation index is positively and significantly correlated with antisemitism measures and the shares of Jews in both middleman occupations are uncorrelated with preexisting antisemitism once the variation in this segregation index is accounted for. Thus, we conclude that there is no relationship between the share of Jews in middleman occupations related to agriculture and antisemitism beyond the overall economic separation of Jews and Gentiles that exists in other occupations. In columns 4 and 8, we show that controlling for the shares of Jews in other main occupations also eliminates the positive correlation between the share of Jews among grain traders and the measures of preexisting antisemitism. This is important because, as reported in the last column of Table 5, our results are robust to controlling for the shares of Jews in these other occupations interacted with economic and political shocks.[38]

The fact that the share of Jews among grain traders is positively correlated with the preexisting antisemitism (if not conditioned on the overall segregation or the shares of Jews in other occupations) creates a potential threat to identification. Thus, we need to verify that our results are robust to controlling for the measures of preexisting antisemitism and for the overall ethno-occupational segregation. We do this in Table 7. The two panels of the Table establish the robustness of the results separately for the interactions of the share of Jews in credit with local economic shock and political turmoil and for the interactions of the share of Jews in trade in grain with marketwide economic shock and political turmoil. Column 1 replicates the baseline results without any additional controls.[39] In column 2, we add to the list of covariates the interactions of dummy for past violence against Jews with economic and political shocks. In column 3, instead, we control for the interactions of the number of antisemitic books and of the number of all books published with the economic and political shocks; in addition, we include the quadruple interaction of the number of antisemitic books with the log number of literate non-Jews (as books can have an effect on violence only if they are read by people). In column 4, we include all covariates from columns 2 and 3 together. In columns 5 and 6, we also control for the interactions of political and economic shocks with the overall ethno-occupational segregation indices calculated with and without middleman occupations, respectively. The first row in each panel of the table shows that our main results are very robust: the coefficients on the interactions of the share of Jews among middlemen with relevant economic shocks and political turmoil remain positive and statistically

the locality i, J_i is the share of Jews in this locality; and J_i^l is the share of Jews among employed in occupation l in the locality i.

38. Interestingly, we find a strong negative correlation between the share of Jews in agriculture and past violence, which suggests that, first, the restrictions on Jews working on the land were driven, at least in part, by the underlying antisemitism and, second, that the negative coefficient on the interaction between the share of Jews in agriculture and economic and political shocks in explaining pogroms (columns 7 and 8 of Table 5) could be driven by the omitted-variable bias (in contrast to the effect of the shares of Jews among middlemen once we control for the shares of Jews in other occupations).

39. In Panels A and B, we restate the results of the specifications presented in columns 1 and 4 of Table 4, respectively. We explore the effects of local and marketwide economic shocks separately in order not to create multicollinearity when we add additional covariates.

significant in all specifications. In addition, we do find a robust positive effects of the interactions of the alternative proxies for past antisemitism with political turmoil (not interacted with economic shocks). In particular, in times of political turmoil there was higher probability of pogroms in localities with past violence against Jews, in localities where there were more antisemitic books published (conditional on the total number of books published) and where there were more literate non-Jews, who could read those books, and in localities where Jews and Gentiles generally were more segregated. In addition, there is a positive coefficient on the quadruple interaction of the marketwide economic shock, political turmoil, the number of antisemitic books, and number of literate non-Jews. All of these results do indicate that prior antisemitism increases the probability of pogroms, particularly during political turmoil.[40]

The main results can be interpreted as causal insofar as one is willing to assume that the observables capture all the relevant variation and there is no remaining unobserved variation that drives both the share of Jews in middleman occupations and the probability of pogroms at the intersection of economic and political shocks. This assumption is not testable directly. Yet, one can shed light on how much more important the effect of unobservables needs to be compared to all observables in order to account fully for our results using the technique developed by Oster (forthcoming) on the basis of Altonji *et al.* (2005). We take the estimates presented in column 1 of Table 7 as a baseline and in each of the subsequent columns in the last row of both panels, we report the Oster's δ for which the true β from equation (1) equals zero.[41] This statistic shows the degree of selection on unobservables relative to observables that would be necessary to fully explain the result by omitted-variable bias. The value of Oster's δ with all additional controls put together (column 6) is 1.8 for the effect of the share of Jews in credit interacted with local economic shock and political turmoil and 2.4 for the effect of the share of Jews in trade in grain interacted with marketwide economic shock and political turmoil. Given the nature of observables that we include, it is extremely unlikely that there are unobserved factors that are twice as important as all observables. We conclude that our results are unlikely to be driven by omitted-variable bias.

5.3.3. Spatial matching: contiguous grid cells. We use spatial matching as an alternative identification strategy. First, we define two treatments: a grid cell is said to be treated with the domination of Jews over credit sector ($T1$) if its share of Jews among creditors is above median, and a grid cell is said to be treated with the domination of Jews over grain trade ($T2$) if it has above-median share of Jews among grain traders. Then, we identify control groups for each of these treatments. In particular, for each grid cell treated with $T1$, we search for a "control" grid cell among its immediate geographic neighbors such that the control's share of Jews among creditors is below median, but we require the control grid cell to have the same treatment status with respect to $T2$ as the cell treated with $T1$. In addition, if the grid cell treated with $T1$ had a local

40. While the coefficients on the interactions of antisemitic books with literate non-Jews and with political shocks are positive and significant, the coefficients on the double-interactions of political shocks with the number of antisemitic books are negative and significant (unreported for conciseness). To understand the magnitude of the cumulative effect, we have calculated the effect of the political (and economic) shocks for every grid cell depending on the grid cell's numbers of antisemitic books and of the literate non-Jews and found that the cumulative effect is positive for the vast majority of grid cells. In particular, the values of antisemitic books and literacy of non-Jews predict a positive probability of pogrom occurrence during political turmoil and marketwide economic shock in 74.8% of the grid cells (according to the estimates presented in column 3 of Panel B). Furthermore, 86.7% of pogroms that did occur during political turmoil and marketwide economic shock took place in those grid cells, where the values of antisemitic books and literacy of non-Jews predict a positive probability of a pogrom.

41. The calculation of δ requires an assumption about R_{max}, *i.e.* the maximum r-squared that could be attained. Following Oster (forthcoming), we use the rule of thumb of $R_{max} = 1.3R$ after we partial out the grid-cell and year fixed effects.

TABLE 7
Controlling for the measures of preexisting antisemitism and occupational segregation

Panel A: The effect of the local economic shocks	Pogrom occurrence					
	(1)	(2)	(3)	(4)	(5)	(6)
Local econ shock × Pol. turmoil × Sh. Jews among creditors	6.56***	6.89***	5.72**	6.14**	6.18***	5.88**
	(2.47)	(2.51)	(2.46)	(2.51)	(2.36)	(2.31)
Political turmoil × × Sh. Jews among creditors	0.34	−0.01	0.91	0.43	−0.95	−0.77
	(0.62)	(0.60)	(0.65)	(0.61)	(0.71)	(0.69)
Local econ shock × Pol. turmoil × Past violence against Jews		−1.70		−1.48	−1.47	−1.51
		(2.14)		(2.33)	(2.35)	(2.35)
Political turmoil × Past violence against Jews		2.04***		1.96***	1.67***	1.75***
		(0.57)		(0.59)	(0.57)	(0.57)
Local econ shock × Pol. turmoil × Log literate non-Jews × Antisemitic books			0.16	0.14	0.13	0.14
			(0.13)	(0.14)	(0.15)	(0.15)
Political turmoil × Log literate non-Jews × Antisemitic books			0.06*	0.09**	0.12***	0.12***
			(0.04)	(0.04)	(0.04)	(0.04)
Local econ shock × Pol. turmoil × Occupational segregation					−0.15	
					(9.78)	
Political turmoil × Occupational segregation					8.54***	
					(2.23)	
Local econ shock × Pol. turmoil × Occupational segregation (w/o middlemen)						1.72
						(9.45)
Political turmoil × Occupational segregation (w/o middlemen)						7.36***
						(2.10)
R^2	0.118	0.120	0.120	0.122	0.123	0.123
Oster's δ for $\beta = 0$		1.918	1.067	2.239	2.238	1.834

economic shock during political turmoil, we require the control also to have the local economic shock. Similarly, for $T2$, the control grid cell is required to be an immediate geographic neighbor of the treated grid cell and to have the same status with respect to $T1$ as the cell treated with $T2$. For some treated grid cells, we were not able to find control grid cells that satisfy all the requirement (*e.g.*, this happened when all neighboring grid cells of a treated grid cell were also treated). In that case, we dropped them from the matched sample. If there were several potential control grid cells for a treated grid cell, we picked as the control the grid cell that was least likely to serve as a control grid cell for another treated grid cell. (This was done to increase the resulting sample.) In case of a tie, we picked the grid cell that was the closest to the treated cell in terms of share of Jews in total population. Online Appendix Figure A15 presents the grid cells in the matched samples for each of the two treatments. In the resulting matched sample,

TABLE 7
(Continued)

Panel B: The effect of the marketwide economic shocks	Pogrom occurrence					
	(1)	(2)	(3)	(4)	(5)	(6)
Marketwide econ shock × Pol. turmoil × Sh. Jews among grain traders	9.42***	7.85***	9.36***	7.85***	7.07**	7.41***
	(2.69)	(2.59)	(2.70)	(2.63)	(3.02)	(2.75)
Marketwide econ shock × Pol. turmoil × Past violence against Jews		2.90*		3.40**	3.34**	3.36**
		(1.55)		(1.63)	(1.68)	(1.68)
Marketwide econ shock × Pol. turmoil × Log literate non-Jews × Antisemitic books			0.26***	0.31***	0.32***	0.32***
			(0.08)	(0.08)	(0.09)	(0.09)
Marketwide econ shock × Pol. turmoil × Occupational segregation					1.62	
					(5.60)	
Marketwide econ shock × Pol. turmoil × Occupational segregation (w/o middlemen)						1.05
						(5.12)
R^2	0.118	0.120	0.120	0.122	0.122	0.122
Oster's δ for $\beta = 0$		5.490	−10.05	9.531	1.789	2.442
Grid and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Interactions with the share of Jews and the middleman sector shares	Yes	Yes	Yes	Yes	Yes	Yes
Interactions with total number of books published	No	No	Yes	Yes	Yes	Yes
All lower-level interactions	Yes	Yes	Yes	Yes	Yes	Yes
Observations	73,728	73,728	73,728	73,728	73,728	73,728
Mean of dependent var.	0.522	0.522	0.522	0.522	0.522	0.522
SD of dependent var.	7.207	7.207	7.207	7.207	7.207	7.207

Notes: The unit of analysis is grid cell × year. The dependent variable is a dummy variable that takes the value of 1 if a pogrom occurred in a given year and grid cell, and 0 otherwise. We multiply the dependent variable by 100 to measure all coefficients in percentage points of probability of pogrom occurrence. The table presents OLS regression results that show the relationship between pogroms and the share of Jews in middleman occupations interacted with economic and political shocks conditional on the measures of preexisting antisemitism and overall occupational segregation interacted with economic and political shocks. Standard errors are corrected for both spatial and temporal correlations following Hsiang (2010) in a radius of 100 km and 1 temporal lag. ***$P < 0.01$, **$P < 0.05$, *$P < 0.1$.

some grid cells serve as control for several treatment grid cells. To account for this, we reweight the control sample accordingly to calculate the geographic neighbour matching estimates.[42] We use the procedure developed by Colella *et al.* (2018), which allows for regression weights while

42. In particular, the matching estimator of the average treatment effect (for each of the two treatments $T \in \{T1; T2\}$) is calculated using the formula: $\beta^m = \frac{1}{K_{T=1}} \sum_{T_i=1} V_i - \frac{1}{K_{T=1}} \sum_{T_j=0} \left(\sum_{T_i=1} \omega(i,j) \right) V_j$, where i indexes the treatment observations and j indexes the control observations in the matched sample, V is the occurrence of pogroms, T is the treatment status, and $K_{T=1}$ is the number of treated observations. The regression weight of each control observation j is given by $\sum_{T_i=1} \omega(i,j)$, where $\omega(i,j) = 1$ if the grid cells i and j are matched as treatment and control, and zero, otherwise. Note that we suppress the subscript t for simplicity of presentation, but it is implicit in the definition of the treatment and control observations: in $T2$, it is completely redundant because all grid cells are affected by economic (and political) shocks in the same way in all years; however, in $T1$, we require the treatment and control to have the same value of local economic shock for any given year

taking into account spatial and temporal correlation in the error term following Conley (1999) and Hsiang (2010).

The identification assumption behind this empirical strategy is that unobservables are similar in the neighbouring grid cells. Online Appendix Table A10 presents the balancing tests for observed variables. The first three columns present unconditional average differences between treatment and control for both treatments. We find that the treatment grid cells are slightly but significantly more rural, which also means that there was a smaller credit sector, a higher share of peasants, and a lower literacy rate among non-Jews in the treatment grid cells compared to the control. However, if we control for the urbanization rate (the share of grid-cell population living in towns), we find perfect balance across treatment and control for both treatments, as reported in the last three columns of the table. Thus, once we control for urbanization, the unobservables are also likely to be balanced across treatment and control.

The results of the spatial matching analysis are presented in Table 8. The first columns replicates the main results in the full sample with the dummy for the share of Jews among middleman occupations above median rather than the share itself. The subsequent columns use the matched samples of grid cells with columns 2–5 reporting results including all years, and columns 6 and 7 reporting results only for years with political turmoil and relevant economic shock (*i.e.* local economic shock in Panel A and marketwide economic shock in Panel B). Column 2 presents the average unconditional difference between treatment and control groups. Column 3 presents exactly the same specification as column 1, but for the matched samples. Column 4 adds urbanization rate interacted with economic and political shocks to the set of covariates (to make sure that the results are not driven by imbalances in the matched samples). Column 5 adds latitude and longitude (to make sure that the results are driven by variation among the neighbouring grid cells). Columns 6 and 7 use the matched samples with both political and economic shocks turned on (essentially, as a cross-section), and we control for urbanization in these specifications to ensure the balance. In column 7, in addition, we control for fixed effects for each treatment and control pair. In this column, whenever an observation serves as a control for several treatment cells, it is included as many times as there are corresponding treatment cells into the sample, which leads to the increase in the sample by about 23%. In all specifications, we find a positive and significant effect of the interactions between the share of Jews in middleman occupations and economic and political shocks. Moreover, controlling for urbanization does not change the magnitude of point estimates: according to the most demanding specification, in column 7, at the time of economic shocks and political turmoil, the difference in the probability of pogroms between the two neighbouring grid cells—with the same urbanization level, but with different values of the dummy indicating whether the share of Jews among middlemen is above the median—is 2.4 to 3.5 percentage points. Overall, our results are robust to using spatial matching.

5.4. *Additional robustness checks*

In this section, we briefly describe the battery of robustness checks.

5.4.1. Alternative measures of shocks. Online Appendix Table A11 presents the results, in which instead of a dummy for local economic shock, we use a continuous variable to measure crop failure: the standardized deviation of spring temperature from its historical grid-

t when there is political turmoil even though in the full sample local economic shock varies both across grid cells and years.

TABLE 8
Spatial matching

Panel A: The effect of the treatment $T1$:	Pogrom occurrence						
Share of Jews among creditors above the median	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Local econ shock × Pol. turmoil × 𝟙(Jews among creditors > median)	4.76***	4.30***	4.13***	4.17***	4.07***	4.30***	3.52**
	(1.33)	(1.54)	(1.54)	(1.52)	(1.49)	(1.42)	(1.37)
Local econ shock × Pol. turmoil × Share of Jews	15.29		16.88	16.93	30.74	17.88	105.31**
	(18.13)		(21.64)	(21.96)	(36.14)	(17.96)	(41.07)
Local econ shock × Pol. turmoil	1.08	0.70	−0.40	−0.58	−26.24		
	(1.61)	(3.00)	(4.36)	(4.36)	(17.32)		
R^2	0.119	0.152	0.156	0.156	0.162	0.109	0.247
Observations	73,728	24,652	24,652	24,652	24,652	649	800
Mean of dependent var.	0.522	0.564	0.564	0.564	0.564	3.852	3.250
Panel B: The effect of the treatment $T2$:	Pogrom occurrence						
Share of Jews among grain traders above the median	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Marketwide econ shock × Pol. turmoil × 𝟙(Jews among gr.traders > median)	1.99**	2.33**	2.18**	2.23**	2.49***	2.23**	2.44**
	(0.93)	(1.02)	(0.99)	(0.98)	(0.96)	(1.06)	(0.98)
Marketwide econ shock × Pol. turmoil × Share of Jews	−2.33		−14.73	−16.62	19.15	−17.53	−5.72
	(10.67)		(22.27)	(26.90)	(29.00)	(32.61)	(23.53)
R^2	0.117	0.108	0.111	0.111	0.125	0.041	0.141
Observations	73,728	27,136	27,136	27,136	27,136	2,120	2,600
Mean of dependent var.	0.522	0.604	0.604	0.604	0.604	5.802	5.462
Sample grid cells (M for matched):	All	M	M	M	M	M	M
Sample years (EP for econ & pol. shocks only):	All	All	All	All	All	EP	EP
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes	Yes	No	No
Treatment-control pair FE	No	No	No	No	No	No	Yes
Interactions with the share of Jews and sectors shares	Yes	No	Yes	Yes	Yes	Yes	Yes
Interactions with urbanization rate	No	No	No	Yes	Yes	Yes	Yes
Interactions with latitude and longitude	No	No	No	No	Yes	No	No

Notes: The unit of analysis is grid cell × year. The dependent variable is a dummy variable that takes the value of 1 if a pogrom occurred in a given year and grid cell, and 0 otherwise. We multiply the dependent variable by 100 to measure all coefficients in percentage points of probability of pogrom occurrence. This table presents the results of the spatial matching analysis. The first column presents regressions on the full sample. In columns 2–5, the sample is the matched sample of grid cells observed during all years. In column 6, the sample is a pooled cross-section of grid cells from the matched sample with economic shock during political turmoil. In column 7, the sample is as in column 6, but those control grid cells that serve as control for several treatment grid cells are included in the sample as many times as there are treatment grid cells matched to this control grid cell. In column 7, we control for the fixed effects for each pair of treatment and control. Standard errors are corrected for both spatial and temporal correlations following Hsiang (2010) in a radius of 100 km and 1 temporal lag. ***$P < 0.01$, **$P < 0.05$, *$P < 0.1$.

specific mean. The results show that our findings are not driven either by the use of government grain assistance in 1880–1882 as the local economic shock or by the choice of threshold of top 5% of the distribution of the deviation of spring temperature to define the extremely hot spring. In columns 1 and 2 of Table 9, we also show that the results are robust to using the definition of local economic shock as in the baseline, but deleting from the sample the years 1880–1882 (for which we used government grain-assistance data), and to using the local economic shock based on reconstructed temperature data without any use of government grain-assistance data. Columns 1 and 2 of Panel A of Table 10 use the alternative definition of political turmoil: a dummy for the rising sovereign bond yield spread. In Columns 3 to 5 of Table 9, we also show that the results are not driven by the fact that the baseline political turmoil dummy is switched on for the year following the turmoil events: alternative definitions have no additional years, two additional lags, or one additional lead. Column 3 of Panel A of Table 10 uses an alternative definition of the marketwide economic shocks: the time series of grain prices.[43]

5.4.2. Subsamples. In column 6 of Table 9, the results are presented for the subsample of years after the emancipation of serfs in 1861 and in column 7 in the subsample of grid cells without big cities (cities that served as capitals in the past or throughout the observation period: Kiev, Warsaw, and Vilnius).

5.4.3. Aggregation Level. In column 8 of Table 9, we show the results for the grid cells that are four times as large: 1×1 degree; and in column 9, we use the administrative boundaries of districts (*uezds*) rather than grid cells as a cross-sectional unit of observation.

5.4.4. Clusters. Online Appendix Table A13 shows that the results are robust to different assumptions about variance–covariance matrix. We take the specification from column 5 of Table 4, in the first three rows we change the radius of Conley correction; in the next three rows change the length of the over-time correlation; in row 7, we present the most demanding specification with a 500-kilometer Conley radius and a 10-year-long autocorrelation; in the next two rows, we show the results with clusters by grid cell and by district.

5.4.5. Controlling for the presence of other minorities. Online Appendix Table A14 establishes robustness of our results to controlling for the two other local minorities that were engaged in middleman occupations in the Pale of Settlement: Germans and Russians. We control for their share in the population and their share among middlemen interacted with economic and political shocks. In addition, we control for the share of the most sizeable local minority group other than Jews in the middleman sector. The presence of Russians and Germans decreases the probability of pogroms during 1905, when political turmoil did not coincide with the

43. In this specification, we use log number of grain traders rather than the share of grain traders because there is a lot more variation across localities in the latter; this helps to gain power, as the sample is substantially smaller due to data limitation for grain prices. As can be seen from the distribution of the share of Jews among grain traders (presented in Panel C of Online Appendix Figure A4), the distribution of the share of Jews among grain traders is skewed to the right and the variation is rather limited. In Online Appendix Table A12, we show that the results are robust to using log numbers of Jews in both middleman occupations rather than shares of Jews among middlemen in the full sample. As both sovereign bond yield spread and price series are available only for a subset of years, in Panel B of Table 10, we also replicate results using our baseline measures on the subsamples, for which the alternative measures are available.

TABLE 9
Robustness to changes in the sample, aggregation, and definition of political turmoil, and the construction of climate data

	Pogrom occurrence								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Robustness to:	Climate data:		Definition of political turmoil:			Sample:		Aggregation:	
	without years 1880–82	no data correction	do not add year after	add 2 years after	add 1 year before	post 1861	without large cities	1x1 degree level	uezd level
Local econ shock × Pol turmoil × Sh. Jews among creditors	5.52**	5.58**	6.83**	6.13**	6.78**	5.70**	5.77**	14.08**	9.66***
	(2.18)	(2.20)	(3.40)	(2.45)	(3.19)	(2.66)	(2.48)	(6.89)	(3.18)
Marketwide econ shock × Pol turmoil × Sh. Jews among grain traders	7.65***	7.72***	10.84***	8.37***	7.85***	11.25***	9.07***	21.96***	8.14**
	(2.32)	(2.33)	(3.49)	(2.35)	(2.51)	(2.87)	(2.80)	(7.67)	(3.77)
Local econ shock × Pol turmoil	1.00	0.94	5.42***	4.05**	4.87***	4.20***	4.32***	6.01*	6.98**
	(0.75)	(0.75)	(2.04)	(1.58)	(1.54)	(1.59)	(1.61)	(3.37)	(3.15)
Observations	72,000	73,728	73,728	73,728	73,728	38,016	72,576	21,376	30,208
R^2	0.117	0.120	0.125	0.122	0.123	0.133	0.119	0.243	0.206
Grid and year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interactions with the share of Jews	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interactions with the share of creditors in total employed	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interactions with the share of grain traders in total employed	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dependent var.	0.407	0.522	0.522	0.522	0.522	1.005	0.508	1.244	0.943
SD. of dependent var.	6.366	7.207	7.207	7.207	7.207	9.974	7.112	11.09	9.667

Notes: The unit of analysis is grid cell × year. The dependent variable is a dummy variable that takes the value of 1 if a pogrom occurred in a given year and grid cell, and 0 otherwise. We multiply the dependent variable by 100 to measure all coefficients in percentage points of probability of pogrom occurrence. This table explores the robustness of the specification presented in column 5 of Table 4. Column 1 drops years 1880–1882 from the sample, column 2 does not apply correction for the local economic shock variable based on government grain-assistance data, but uses the full sample. Columns 3, 4, and 5 use augmented definition of political turmoil: without a lag added, with two lags added, and with a forward added. Column 6 restricts the sample to 1861 and after. Column 7 drops large cities (which served as capitals of any polity in the past or during the observation period). Column 8 presents results for the aggregated 1×1 degree grid cells. Column 9 replicates the results with the unit of analysis being the district (*uezd*). Standard errors are corrected for both spatial and temporal correlations following Hsiang (2010) in a radius of 100 km and 1 temporal lag. ***$P < 0.01$, **$P < 0.05$, *$P < 0.1$.

TABLE 10
Alternative measures of political turmoil and marketwide economic shocks

Panel A: Rising spread as political turmoil; grain price as marketwide shock	Pogrom occurrence		
	(1)	(2)	(3)
Rising sovereign bond yield spread × Local econ shock × Sh. Jews among creditors	5.62*		
	(2.97)		
Rising sovereign bond yield spread × Marketwide econ shock × Sh. Jews among grain traders		6.73*	
		(3.46)	
Log grain price × Political turmoil × Log Jewish grain traders			11.37***
			(3.62)
R^2	0.149	0.148	0.160
Panel B: Baseline specifications on the same samples	Pogrom occurrence		
	(1)	(2)	(3)
Political turmoil × Local econ shock × Sh. Jews among creditors	7.10**		
	(3.19)		
Political turmoil × Marketwide econ shock × Sh. Jews among grain traders		8.04*	
		(4.13)	
Marketwide econ shock × Political turmoil × Log Jewish grain traders			2.94***
			(0.79)
R^2	0.151	0.149	0.151
Grid and year FE	Yes	Yes	Yes
Interactions with the share of Jews; all lower-level interactions	Yes	Yes	Yes
Interactions with dummy for former capital cities	Yes	Yes	No
Observations	24,192	24,192	31,225
Mean of dependent var.	1.009	1.009	0.781
SD of dependent var.	9.992	9.992	8.805

Notes: The unit of analysis is grid cell × year. The dependent variable is a dummy variable that takes the value of 1 if a pogrom occurred in a given year and grid cell, and 0 otherwise. We multiply the dependent variable by 100 to measure all coefficients in percentage points of probability of pogrom occurrence. Panel A presents OLS regressions in which we use alternative measures of political turmoil (columns 1 and 2) and of the marketwide economic shock (column 3). Panel B presents results with the baseline measures on the subsamples where alternative measures are defined. In this table, in addition to the standard set of controls we include a dummy for Kiev, Warsaw, and Vilnius—cities that served as a capital city of any polity in the past or during observation period—interacted with economic and political shocks. This control increases precision of estimates in regressions using the sovereign bond yield spread; and it does not at all affect the baseline estimates. In column 3, instead of the share of Jews among grain traders, we use the log of the number of Jewish grain traders, because this variable has more variation. In Online Appendix Table A12, we show that our baseline results hold for the log numbers of Jews in both middleman occupations. Standard errors are corrected for both spatial and temporal correlations following Hsiang (2010) in a radius of 100 km and 1 temporal lag. ***$P < 0.01$, **$P < 0.05$, *$P < 0.1$.

economic shock, but the inclusion of any of these controls does not affect the coefficients of interest.

6. MECHANISMS

In this section, we consider several mechanisms that could potentially explain our results. We start with the presentation of the mechanism that is consistent with the evidence and then discuss why several alternative mechanisms are not fully consistent with the data.

6.1. *Repeated interactions and uncertainty*

Historians agree that credit contracts were relatively well enforced in the Russian Empire.[44] However, both the complete forgiveness of debt and rolling it over until the debtor was expected to gain sufficient income to repay were very common (Antonov, 2016).

Both professional moneylenders and grain traders extended credit. Creditors gave loans to peasants during the planting season (spring for summer crops and fall for winter crops), to be repaid after the harvest was collected (in the subsequent summer and early fall). When yields were low and peasants could not repay their loans, debt was often forgiven. In times of high grain prices, grain traders extended trade credit to buyers of grain who were unable to pay right away. This happened to grain consumers outside agriculture and to peasants in cases of shortages of the stock of seeds, which could occur after two consecutive bad harvests because long-term grain storage was prohibitively costly for peasants (*e.g.* Kahan, 1989). Creditors and grain traders, therefore, effectively served as providers of insurance.

Of course, debt forgiveness as a business model for creditors and grain traders could only be sustained in equilibrium because of the value of future transactions in an environment with repeated interactions: creditors lent to peasants and grain traders extended credit both to urban buyers of grain and to peasants in rural areas regularly. Middlemen found it worthwhile to forgive nonpayers in case of default because they valued maintaining the economic relationship in the future (Kranton and Swamy, 1999). This was possible in times of political stability. In contrast, during political turmoil the continuation of the relationships between creditors and grain traders, on the one hand, and their debtors, on the other hand, became too uncertain. Debtors were unable to credibly commit to business as usual in the future due to uncertainty created by political shocks, such as the threat of bringing back serfdom, the threat of displacement or conscription in the case of a war, and the threat of nationalization in the case of a revolution. Thus, during the episodes of political turmoil creditors required immediate repayments of debt and grain traders demanded immediate payments for grain even in the case of a severe economic shock. Middlemen could not forgive debts and extend new credit because they anticipated that their economic relationship with debtors will be disrupted in the future by political turmoil. When the middleman sector was dominated by the Jews, the combination of severe political and economic shocks led to ethnic conflict because the majority turned against the group, whose representatives (*i.e.* creditors and grain traders) could not serve as the providers of insurance according to the norm that emerged in times of political stability.

One could argue that the severity of economic shocks differed depending on whether peasants were subsistence farmers or commercial farmers. Commercial farmers could have higher grain reserves and therefore were less in need of loans in case of crop failure. We explore the heterogeneity of our results with respect to commercialization of agriculture using the share of wheat in total rye and wheat production (at the district level) as a proxy for commercialization. (Rye was the main crop used for the growers' own consumption and wheat was the main cash crop.) Column 1 of Online Appendix Table A15 presents the results. Consistent with the hypothesis that commercialization of agriculture allowed peasants to smooth shocks better, even without the intervention of middlemen, the coefficients on the interactions of economic and political shocks with the proxy for agricultural commercialization are negative and significant. In addition, the coefficients on the quadruple interaction terms of the shares of Jews in middleman occupations

44. A creditor's complaint of nonpayment with presentation of a promissory note, which was the most common form of credit contract, led the authorities to send a policeman who was supposed to seize valuable items (if any) in favour of the creditor or, in case of the absence of valuable property, to arrest the debtor (provided that the creditor paid a fee) (Jezierski and Leszczynska, 2003; Antonov, 2016).

with economic and political shocks and commercialization are also negative (the effect for Jewish grain traders and marketwide economic shocks is significant and, for the Jewish creditors and local economic shocks, it is imprecisely estimated).[45]

Overall, the mechanism that relates violence against Jews to political uncertainty as the factor that breaks repeated-interactions equilibrium is fully consistent with the evidence.

6.2. *Alternative mechanisms*

6.2.1. Scapegoating. The history of religious persecution of Jews suggests that the majority often channeled anger and frustration from economic or political hardships into violence against Jews, as the minority was falsely accused of being a reason for majority's misfortunes (*e.g.* Girard, 1986; Glick, 2008). Could religious intolerance be the primary driver of pogroms in the Russian Empire? Both cross-sectional and over-time determinants of pogroms that we have uncovered strongly suggest a more complicated mechanism.

Group stereotyping. Religious persecution implies that Jews are targeted as a group irrespective of their occupations. This is inconsistent with the evidence as pogroms were associated with the domination of Jews over credit and grain trading, not any other occupation. Given that finance and trading were the traditional occupations of Jews (*e.g.* Slezkine, 2004; Grosfeld *et al.*, 2013), observing Jews in these occupations in particular may trigger higher animosity towards Jews because of a stereotype of an "unproductive" minority, which these occupations may reinforce. However, such group stereotyping should have concerned all middleman occupations, including traders of nonagricultural goods; whereas we find that the domination of Jews over trade unrelated to agriculture was not associated with pogroms.

State capacity to enforce order. When one considers scapegoating as an explanation for our findings, one also needs to explain why pogroms did not occur during severe economic shocks without political turmoil, as scapegoating implies that Jews are blamed for all problems of the majority. We have shown that economic shocks without political turmoil were no less severe than those that occurred during political turmoil. One reason why political turmoil could be important for scapegoating is that it could affect the probability of punishment of potential perpetrators. The weakness of the state and, in particular, its inability to enforce law and order, has been shown to affect violence against minorities (see, for instance, the arguments presented by Arendt (1973) about anti-Jewish violence in general and by Snyder (2016) about the Holocaust). All the three waves of pogroms took place during episodes of political turmoil. However, only the last wave of pogroms (which took place after the 1917 Revolution) was associated with a decrease in the probability of punishment—the October 1917 Revolution and the subsequent Civil War led to the collapse of state institutions, in particular law enforcement. In contrast, during the first two waves of pogroms, the state had full capacity to punish pogrom perpetrators and actually did it in many cases.[46] Since the first two waves of pogroms were not associated with a decrease in the probability of punishment, the weakness of the state could not be the main driver of pogroms.

45. In Column 2 of Online Appendix Table A15, we also explore heterogeneity with respect to access to railroads, which could potentially be considered as a proxy for market integration. We find no statistically significant heterogeneity in our main effects. Yet, the direct effect of the distance to railroads at the times of political turmoil is negative and significant, which is consistent with the historical fact that the violent mob travelled by rail to neighbouring localities at the peak of the pogrom waves Aronson (1990, p. 110).

46. For example, according to government reports, 3,675 people were arrested for participation in pogroms in 1881, of whom 2,359 were tried in court and over 1,000 were subjected to administrative exile to Siberia (Klier, 2011, p. 49). However, police did not intervene to stop some of the most devastating pogroms, particularly during the second wave of pogroms, when the outbursts of anti-Jewish violence took a form of mass terror.

TABLE 11
Enforcement capacity of the state and pogrom determinants

Panel A: The effects of Jewish creditors and local econ shocks	Pogrom occurrence		
	(1)	(2)	(3)
Sample of pogroms waves: 1 (1881–1882); 2 (1903–1906); 3 (1917–1922):	Waves 1 and 2	Wave 3	All waves
Local econ shock × Sh. Jews among creditors	10.62***	6.46*	10.62***
	(3.93)	(3.78)	(3.93)
Local econ shock × Sh. Jews among creditors × 𝟙(Wave 3)			−4.16
			(5.45)
Local econ shock × Share of Jews	−6.15	57.19***	−6.15
	(22.20)	(20.49)	(22.20)
Local econ shock × Share of Jews × 𝟙(Wave 3)			63.34**
			(30.21)
R^2	0.285	0.279	0.203
Grid FE	Yes	Yes	Yes
Panel B: The effects of Jewish grain traders and marketwide shocks	**Pogrom occurrence**		
	(1)	(2)	(3)
Sample of pogroms waves: 1 (1881–1882); 2 (1903–1906); 3 (1917–1922):	Waves 1 and 2	Wave 3	All waves
Marketwide econ shock × Sh. Jews among grain traders	7.59*	8.57***	7.59*
	(4.23)	(2.85)	(4.23)
Marketwide econ shock × Sh. Jews among grain traders × 𝟙(Wave 3)			0.98
			(5.10)
Marketwide econ shock × Share of Jews	−25.76	18.44	−25.76
	(17.41)	(13.95)	(17.41)
Marketwide econ shock × Share of Jews × 𝟙(Wave 3)			44.20**
			(22.31)
R^2	0.066	0.043	0.061
Grid FE	No	No	No
Year FE, all lower-level interactions	Yes	Yes	Yes
Observations	3,456	3,456	6,912
Mean of dependent var.	6.887	3.993	5.440
SD of dependent var.	25.33	19.58	22.68

Notes: The unit of analysis is grid cell × year. The dependent variable is a dummy variable that takes the value of 1 if a pogrom occurred in a given year and grid cell, and 0 otherwise. We multiply the dependent variable by 100 to measure all coefficients in percentage points of probability of pogrom occurrence. The sample in column 1 includes only years of the first two waves of pogroms. The sample in column 2 includes only years of the third wave of pogroms. The sample of column 3 includes years of all pogrom waves. Standard errors are corrected for both spatial and temporal correlations following Hsiang (2010) in a radius of 100 km and 1 temporal lag. ***$P < 0.01$, **$P < 0.05$, *$P < 0.1$

In Table 11, we explore differences in cross-sectional determinants of pogroms separately before and after the collapse of the state in 1917. We zoom in to the subsample of pogrom waves and show that our main results hold, separately, in the first two waves that occurred before 1917 and in the last wave that started with the 1917 revolution. Column 1 presents the result for the first two pogrom waves, and column 2 presents the result for the third wave of pogroms. Column 3 pools observations from the three pogrom waves together and include interaction terms with a dummy for the third wave of pogroms. In Panel A, we explore the effect of the local economic shock, and in Panel B, the effect of the marketwide economic shock (political turmoil is always present in this sample). We find a positive and significant effect of the share of Jews among middlemen both before and after 1917. Moreover, the coefficients on the interaction term between the shares of Jews in middleman occupations, economic shocks, and the dummy for the third wave of pogroms are not statistically significant; thus, we cannot reject the hypothesis about the equality of the magnitude of these effects between the two subsamples. We conclude that the variation in the probability of punishment is not what is driving the results about Jewish middlemen.

Does enforcement matter for pogroms in general? We find that the effect of the share of Jews in the local population is much bigger in the last wave of pogroms than in the first two waves of pogroms. The coefficients on the interactions of the share of Jews with economic shocks and the dummy for the third wave of pogroms are large, positive, and statistically significant, suggesting that general ethnic violence against Jews was exacerbated by the lack of law and order. As we do not find a similar effect for the share of Jews among middlemen, the violence related to the presence of creditors and grain traders must have been driven by a different mechanism.

State-sponsored violence. As mentioned in the background section, the first historical analyses of pogroms suggested that the Russian government conspired to incite pogroms in order to redirect discontent of the masses away from the monarch and the state toward a scapegoat. The evidence gathered later on led to the emergence of a new consensus among the contemporary historians that the state was not involved in pogroms. In contrast, the state viewed pogroms as a dangerous factor contributing to instability and was trying to prevent the spread of violence by mobilizing police forces and to punish perpetrators in order to avoid violence in the future (Dekel-Chen et al., 2011; Klier, 2011).

6.2.2. General crime and arsons. Overall levels of crime may increase with both economic and political shocks (*e.g.* Bignon *et al.*, 2017). Could our estimates be picking up this effect? To address this question, we consider thefts, homicides, and arsons as outcomes. The data for these aspects of general crime exist only for a rather short period (1900–1912) including the second wave of pogroms at the province level. We match the province-level data to grid cells and run panel regressions at the grid-cell level for pogrom occurrence and the three indicators of general crime as outcomes. The results are presented in Table 12. Column 1 replicates our main results in this subsample; the other columns of the table show that there is no relationship between Jewish presence in localities or their occupations and general crime. These results suggest that pogroms were not just a subcomponent of an increase in general crime at the time of severe shocks, because, otherwise, we would have found similar results for the measures of general crime.[47]

6.2.3. Inequality. Differences in absolute income and wealth. Crises may lead to ethnic violence when minorities are richer than the majority. Ethnic groups that constitute the economic elite may be targeted both in order to steal their resources and for reasons of scapegoating. However, Jews were discriminated against in the Russian Empire. Double taxation and severe restrictions on economic activity caused widespread poverty among Jews in the Pale. Historians suggest that, on average, Jews were poorer than the majority. There are no systematic data on income or wealth either by ethnic group or by occupation. Historical accounts suggest that it is reasonable to believe that Jewish middlemen were wealthier than Jews in many

47. Despite the fact that we do not find any effect of Jewish presence on arson, according to Jewish historians (e.g. Dubnow, 1920; Klier, 2011), arsons sometimes were used as a form of intimidation and violence directed specifically at Jews. Klier (2011) suggests that this was the case, but argues that this claim remains to be proven. During the first wave of pogroms, there were "serious fires in Jewish centers such as Slonim, Novogruda, Bobruisk, and Minsk. A letter describing the fire in Minsk noted that 'the state of minds here is extremely troubled: many see the fires as a variant of the south-Russian pogroms; all Christian homes have icons placed in their windows; the Jews are all packing up, and await new fires from day to day"' (Klier, 2011, pp. 54–55). Frierson (2002), a historian of the "Red Rooster", a term that denoted arsons in the Russian Empire, showed that Pale provinces were among the most arson-prone in the empire. Unlike pogroms, arsons, were much harder to prove, especially during droughts and hot weather. However, for that same reason, arson was a high-risk strategy for pogrom perpetrators as in hot weather fires spread easily and in many settlements Jews and non-Jews lived side by side.

TABLE 12
Placebo: general crime, 1900–1912

Panel A: The effects of local econ shocks	Pogrom occurrence	Theft per capita	Homicide per capita	Arson per capita
	(1)	(2)	(3)	(4)
Local econ shock × Political turmoil × Sh. Jews among creditors	9.84***	−98.87	−8.91	−77.83
	(3.44)	(76.71)	(9.13)	(59.32)
Local econ shock × Political turmoil × Share of Jews	−6.11	413.97	35.03	298.28
	(20.33)	(334.49)	(45.93)	(254.76)
R^2	0.205	0.921	0.831	0.915
Panel B: The effects of marketwide econ shocks	Pogrom occurrence	Theft per capita	Homicide per capita	Arson per capita
	(1)	(2)	(3)	(4)
Marketwide econ shock × Political turmoil × Sh. Jews among grain traders	3.77*	91.60	30.78	7.81
	(2.13)	(88.67)	(23.79)	(13.70)
Marketwide econ shock × Political turmoil × Share of Jews	14.79	−168.53	−40.87	−11.75
	(9.43)	(145.73)	(31.98)	(9.54)
R^2	0.201	0.921	0.831	0.915
Grid and year FE	Yes	Yes	Yes	Yes
Interactions with the share of Jews; all lower-level interactions	Yes	Yes	Yes	Yes
Observations	7,488	6,422	6,422	5,434
Mean of dependent var.	1.950	50.51	7.758	20.45
SD of dependent var.	13.83	624.6	102.9	287.7

Notes: The unit of analysis is grid cell × year. This table considers theft, homicide, and arson per capita as placebo outcomes, and reports results for pogrom occurrence for the sample of years in which the placebo outcomes are available: 1900–1912. Data on arson have even shorter time span: 1900–1910. Due to the smaller sample, we make a less conservative assumption about clusters in this table than in other tables: standard errors are clustered at grid-cell level without accounting for spatial correlation. ***$P < 0.01$, **$P < 0.05$, *$P < 0.1$

other occupations, such as cobblers or tailors. Yet, there is no evidence that Jewish creditors and grain traders were richer than other Jewish middlemen engaged in activities unrelated to agriculture, such as Jewish jewelry traders or traders of other non-agricultural goods. The fact that only Jewish domination over the middleman sector *related to agriculture* was associated with pogroms suggests that differences in absolute income and wealth do not account for our results. Furthermore, if the shocks to economic inequality were the main mechanism behind pogroms, it is not clear why pogroms did not occur during severe crop failures outside political crises, as political turmoil did not have a direct effect on incomes.

Even though there are no proxies for ethnic inequality, there is a proxy for inequality within the majority group, namely, inequality in land ownership (Nafziger, 2013; Dower and Markevich, 2017). We use these data to test for differential effects. Column 3 of Online Appendix Table A15 shows that there is a positive significant coefficient on land Gini interacted with political turmoil (and with marketwide economic shocks), suggesting that land inequality (and, thus, a worse economic situation for peasants) was associated with higher probability of pogroms in times of political crises and high grain prices. However, our main coefficients of interest remain robust to controlling for land Gini interacted with economic and political shocks, and there are no heterogeneous effects of our main explanatory variables with respect to land Gini.[48]

48. In medieval Europe, antisemitic violence was often exacerbated by the fiscal extractions of the state (Baron, 1965; Koyama, 2010). We use cross-sectional variation in tax rates to explore differential effects with respect to fiscal

Differences in relative incomes. Economic shocks led to changes in differences in income levels between Jews and Gentiles. Mitra and Ray (2014) use the case of Hindu–Muslim violence in India to show that ethnic and religious violence can be caused by changes in the relative incomes of two competing ethnic groups. The changes in the income gap between non-Jews and Jews following crop failure depended on Jewish occupational composition in a way that is inconsistent with changes in relative income being the mechanism behind our results. Peasants (*i.e.* the main non-Jewish occupation throughout the Pale) were hit the hardest by crop failures. The income of Jews changed differently depending on whether their primary occupation was grain trader, creditor, trader of non-agricultural goods, or craftsman. Incomes of Jews in occupations that were not directly related to agriculture, such as industrial workers and traders of non-agricultural goods, were affected by crop failures only via a fall in demand caused by the shock to peasants' income. In contrast, middlemen related to agriculture were affected directly, but differently, depending on the type of middleman occupation. The income of creditors, who lent money to peasants, must have declined sharply due to the increase in the default rate as peasants could not repay their loans. In contrast, the income of grain traders should not be affected much as traders passed the purchase-price increases onto grain buyers. If traders exercised monopoly power, the incomes of grain traders could even rise with an increase in grain prices. Overall, it is reasonable to conclude that the relative income of Jews compared to non-Jews increased following crop failures more in localities where Jews were grain traders than in localities where they were traders of non-agricultural goods and even more compared to localities where the Jews were creditors. As the effects of crop failures (during political turmoil) in places where Jews dominated trade in grain and moneylending are similar, and there is no effect in localities where Jews dominated other professions, it is unlikely that shocks to relative income is the mechanism at play.[49]

7. CONCLUSION

We study the conditions under which anti-Jewish pogroms took place in the 19th and early 20th century in Eastern Europe. Pogroms occurred when severe economic shocks coincided with political turmoil, and mostly in localities where Jews dominated credit and trade in grain. Economic shocks in times of political stability did not result in pogroms, and Jewish domination over any other sector of the local economy including traders of non-agricultural goods was not associated with an increase in the probability of pogroms. We consider several potential explanations for these findings and conclude that neither the ethnic violence caused by scapegoating nor ethnic inequality can explain the evidence. We argue that Jewish middlemen were the providers of insurance to the majority during economic crises: they forgave outstanding debts and extended new credit. However, political turmoil introduced uncertainty about the continuation of the long-term relationship between Jewish creditors and grain traders, on the one hand, and the majority group, on the other hand, making the implicit insurance contracts nonviable, because

pressure on peasants. Column 4 of Online Appendix Table A15 reports the results. In contrast to our expectations, we find a negative and significant coefficient on the quadruple interaction term between the share of Jews in credit, local economic shock, political turmoil, and local tax rate. This effect could be explained by the endogeneity of tax rates to commercialization of agriculture considered above; in which case, the level of tax rate should reflect a better, not worse, economic situation of peasantry. Our measure of commercialization of agriculture is positively and significantly correlated with tax rates: in a bivariate OLS regression explaining tax rates, the coefficient on the share of wheat is 1.34 with a standard error, corrected for spatial correlation, of 0.65 and p-value of 0.04.

49. Mitra and Ray (2014) stress the importance of economic competition between the two groups as a channel through which relative income affects group violence. They recognize that in the case of occupational segregation, theoretical results about the link between economic inequality and conflict may not hold. As Jews and non-Jews were largely segregated into different occupations throughout the Pale, competition-driven violence could not explain pogroms.

they were based on the continuation value in repeated interaction. As a result, the concomitance of economic shocks and political turmoil resulted in three major waves of pogroms, during which Jewish middlemen were the primary target: peasants organized pogroms when neither the repayment nor renegotiation of loans from Jewish creditors was possible, and buyers of grain turned against Jews when grain prices were high and there was no credible way to commit to payment in installments for the grain that Jewish traders brought to the market.

Our analysis suggests broader lessons. First, political shocks interact with income shocks to trigger ethnic conflict. Second, occupational segregation across ethnic groups might not reduce conflict, even though it does reduce interethnic competition; this happens when minorities specialize in middleman occupations, but the uncertain environment makes longer-term relationships difficult to sustain.

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Supplementary Data

Supplementary data are available at *Review of Economic Studies* online.

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