Violent Backlash to Political Reform: Evidence from Anti-Jewish Pogroms in the 1905 Russian Revolution

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Abstract

Local violence often accompanies momentous political change. We examine this phenomenon during the 1905 Russian Revolution, when backlash to reform contributed to a spike in anti-Jewish pogroms. Adjudicating between work tying conflict to ethno-religious polarization, and historical accounts of a simpler relationship between group size and violence, we show that the increase in pogroms after the October Manifesto was smaller where Jews were relatively numerous. We rationalize this empirical pattern with Esteban and Ray's (2008) model of diversity and conflict, demonstrating that a group adversely affected by reform is less likely to resort to violence when comparatively small.

Keywords: Reform, backlash, conflict, pogroms, diversity, polarization

O, even now its eyes from me demand accounting,
For these the tales the spider is recounting,
Tales that do puncture the brain, such tales that sever
Thy body, spirit, soul, from life, forever!

"In the City of Slaughter," Hayim Nahman Bialik

In October 1905, two momentous events occurred in the Russian Empire. One was wonderful and unexpected: the surrender by Tsar Nicholas II of absolute power over his subjects. The other was horrible and familiar to those who remembered the period after the assassination of Nicholas's grandfather, Tsar Alexander II, in 1881: a plague of anti-Jewish pogroms, with thousands of casualties and untold material damage.

What happened in 1905 was a quintessential example of violent backlash to political reform. Revolutions, negotiated transitions, watershed elections, and other changes to *de jure* political power produce losers and winners, engendering feelings of political threat among those who backed the losing side. Accompanied by rumors and propaganda, such sentiments can activate preexisting prejudices, fueling mob violence, street fighting, and other disturbances (Snyder 2000; Horowitz 2001). Individuals with little connection to larger political struggles, but identifiable as belonging to ethnic or religious groups believed to benefit from such change, are frequent victims. There are numerous examples, including Ku Klux Klan terror following the American Civil War, the (first) Troubles in Northern Ireland during and after the Irish War of Independence, the anti-Armenian pogrom in Baku during Gorbachev's reforms of the Soviet system, and right-wing violence in Colombia following electoral reforms and the adoption of a new constitution in the late 1980s and early 1990s.

As these examples illustrate, violent backlash to political reform often plays out at the local level (e.g., Fergusson, Querubin, Ruiz, and Vargas 2021). Understanding where such violence occurs is vitally important for policymakers and other stakeholders, but existing work on conflict and reform—whether focused on liberalization (e.g., Acemoglu and Robinson 2006) or state-building (e.g., Besley and Persson 2010)—largely abstracts from local heterogeneity. A natural hypothesis is that violent backlash should be more likely in the presence of ethno-religious diversity, which increases the perceived threat that groups pose to one another (Horowitz 1985; Esteban and Ray 1994, 1999). Yet cross-country work documenting the general importance of ethno-religious *polarization* for conflict (e.g., Reynal-Querol 2002; Montalvo and Reynal-Querol 2005; Esteban, Mayoral, and Ray 2012) is silent on backlash to political reform in particular, as such studies aggregate across settings in which one group

or another is advantaged.[1] Meanwhile, as we discuss below, work that is sensitive to local context sometimes suggests a simpler, monotone relationship between relative group size and violence, though such studies rarely compare conflict systematically before and after reform. In principle, one might therefore expect violent backlash to be more likely either in highly polarized societies or when some group is comparatively numerous, with no strong reason to expect one pattern over the other.

In this paper, we ask: Within the general context of reform, which local communities are most vulnerable to violent backlash? We answer this question with a study of political violence during the 1905 Russian Revolution, which triggered anti-Jewish pogroms across the Pale of Settlement, the western region of the Russian Empire to which Jewish settlement was generally restricted. Our research design exploits the profound changes to Russia's political institutions promised by the October Manifesto, Tsar Nicholas II's response to the Revolution that ended centuries of absolute autocracy and guaranteed civil and political rights. The Manifesto—issued "suddenly, with no preliminary notice or preparation" (Surh 2024, p. 44)—was a major concession to Russia's liberal and radical movements, in which Jews were prominent, and many traditional supporters of the monarchy blamed Jews for the Tsar's action. Following publication of the Manifesto, there was a marked increase in pogrom incidence, as anti-Semitic propaganda and a history of Jewish participation in radical politics encouraged supporters of the autocracy to target Jews as the presumed instigators of reform.

Our focus is on the religious composition of "Jewish settlements" in the Pale, which in practice varied enormously in the relative size of the local Jewish community. Our research design is difference-in-differences: we examine the change in monthly pogrom incidence after October 1905 in settlements with relatively low and high a) Jewish share, or b) polarization. The latter variable is measured as $n\,(1-n)$, with n the share of the local population that is non-Jewish, so that polarization is maximized when Jews and non-Jews are equally numerous. Jewish share takes values that span the possible range, allowing us to distinguish between the (differential, pre- versus post-treatment) effect on pogrom incidence of Jewish

[1]The measure of polarization in Esteban, Mayoral, and Ray (2012) differs from that in Reynal-Querol (2002) and Montalvo and Reynal-Querol (2005) in allowing for arbitrary intergroup distances. With binary distances, polarization in either case is proportional to $\sum_{g\in G} n_g^2\,(1-n_g)$, where G is the set of groups, indexed by g, and n_g is the population share of group g. There is less empirical support for any relationship between conflict and the related concept of *fractionalization* (Fearon and Laitin 2003; Collier and Hoeffler 2004). With two competing groups (as in the setting we describe below), the difference is semantic: both polarization and fractionalization are proportional to $n\,(1-n)$. See also Arbatli et al. (2020), who emphasize intra- rather than intergroup diversity as a determinant of conflict—an important perspective that is arguably less central to reform that advantages one group over another.

share and that of polarization. To support the assumption of (strong) parallel trends in the absence of treatment, we account for weather shocks, potential diffusion across settlements, differential trends at the provincial level, and various settlement-level characteristics.

Figure 1 illustrates both our design and our main results. The sharp increase in pogroms in November 1905, following publication of the October Manifesto, is much smaller in settlements with comparatively large Jewish communities. In contrast, the change in pogrom incidence after the October Manifesto is no higher or lower in more polarized settlements.

This strong, negative relationship between Jewish population share and violent backlash to the October Manifesto is robust to the inclusion of the controls discussed above. We additionally demonstrate that any unobserved confounding would need to be much stronger than observed differential trends by population, which themselves are strongly associated with pogrom incidence. Among numerous changes to specification and sample, we also show robustness to controlling for mobilization patterns during the Russo-Japanese War, for prior pogrom incidence, and for differential trends by a host of settlement-level characteristics: proximity to provincial capitals and railroads, Jewish occupational patterns, prior pogrom incidence, size of the local industrial economy, presence of the Jewish Labour Bund, presence of police, and distance to troops.

Why was the increase in the incidence of pogroms after the October Manifesto smaller in settlements with relatively large Jewish populations? To answer this question, we turn to Esteban and Ray's (2008) influential model of diversity and conflict, which in its original presentation suggests a (negative) correlation between polarization and conflict *initiation*.[2] We elaborate this model to allow for both systematic (national) and idiosyncratic (local) contributions to the status quo distribution of benefits. Political reform is properly understood as a shock to the systematic contribution—in our context, a leveling of the playing field between Jews and non-Jews. We show that the differential probability of conflict is increasing in the population share of the group disadvantaged by reform if there is little or no open conflict under the status quo. In this environment, the costs of conflict with a relatively large group dissuade those adversely affected by the reform from initiating violence. Our empirical context broadly reflects the assumptions and conclusions of this analysis.

Taken together, our empirical and theoretical analyses suggest a new way of thinking about conflict. Violent backlash to political reform occurs when one group perceives a disadvantage relative to another. Under reasonable assumptions, the probability that the group

[2]As discussed below, Esteban and Ray (2008) argue that, because conflict is more intense—that is, costly—when societies are polarized, conflict is less likely when two competing groups are of approximately equal size. As we show, this conclusion rests critically on the implicit assumption of a status quo distributed idiosyncratically across polities.

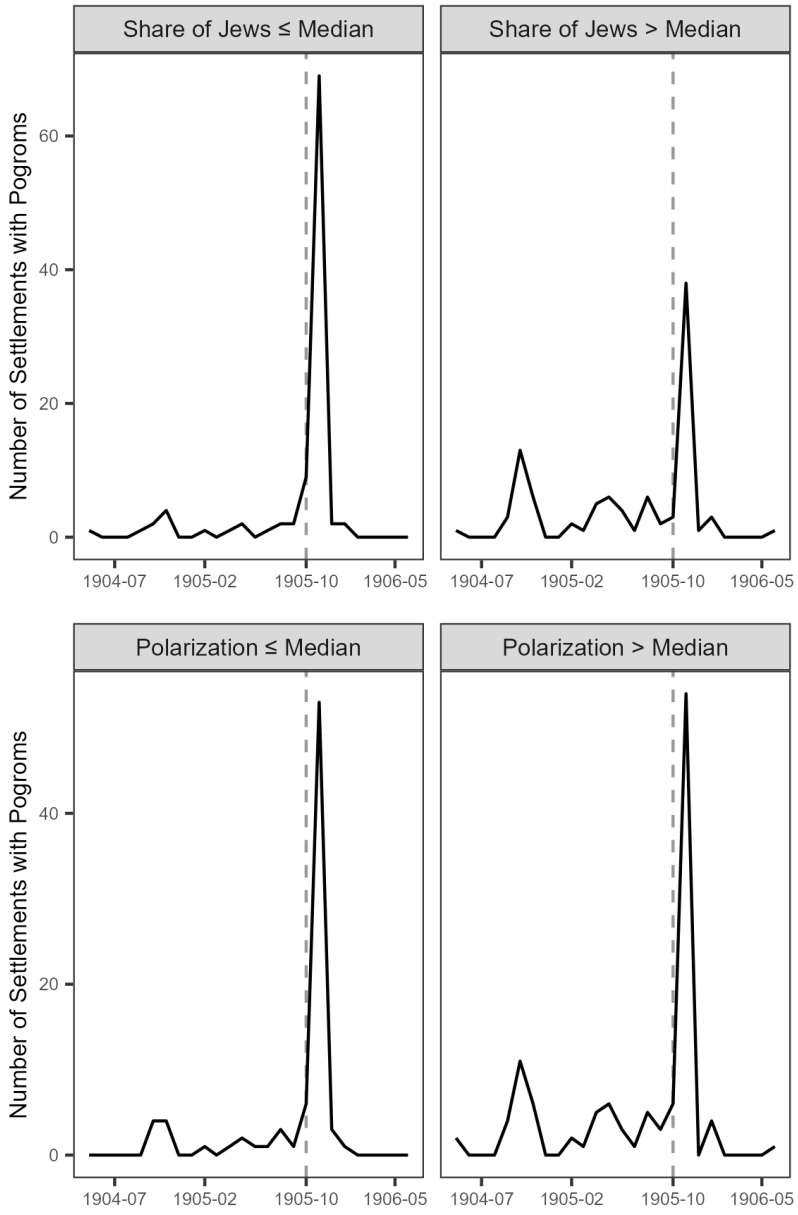

Figure 1: Population Share of Jews and Pogrom Incidence (top) versus Polarization and Pogrom Incidence (bottom). Polarization is measured as $n\,(1-n)$, with n the share of the local population that is non-Jewish.

disadvantaged by reform can claw back some of its power through force is increasing in its population share; so is its expected payoff from starting such a conflict. Without invalidating the relationship between polarization and conflict that may exist in other settings, our work thus demonstrates that, in the context of political reform, local backlash is less likely when the group that most benefits from reform is large.

Our study builds on various literatures. As noted, our research design exploits settlement-level data on violence and religious composition, distinguishing our analysis from much of the existing literature, which examines variation at the country (or, in Guarnieri 2025, country-ethnicity) level. Among the exceptions to this general characterization, our historical context (Imperial Russia) brackets the coalition politics central to the seminal work of Wilkinson (2004). We share with Amodio and Chiovelli (2018) a focus on ethnic violence following political change, though our focus is on backlash by losers from reform rather than on competition for power *within* a newly enfranchised majority (in their setting, black South Africans). When comparing settlements with more and less residential segregation, we draw inspiration from Kasara's (2017) study of violence and local ethnic segregation in Kenya. Our work is also informed by studies of non-ethnic violence, as when we follow Aidt and Leon-Ablan (2022) and Aidt, Leon-Ablan, and Satchell (2022) in explicitly modeling diffusion to reflect the circulation of both information and perpetrators. Finally, as in Desierto and Koyama (2024), we relate religious violence to the size of population groups, though in their setting it is the size of the ruling coalition that determines whether persecution is state-sanctioned or local.

As discussed, the presence of a simple relationship between population size and violence, as we find, recurs in work that is sensitive to local context, though the direction of the relationship varies with the strategic environment. Historians frequently emphasize that violence is more or less likely when some group is comparatively large; we provide examples from our empirical context below. The logic of such arguments often relates to the visibility of the targeted group, an idea that Solomon (2023) explores systematically for the Kristallnacht pogrom in Nazi Germany. Grosfeld, Sakalli, and Zhuravskaya (2020) similarly present evidence that anti-Jewish pogroms were more likely (in Imperial Russia) when political turmoil and economic shocks intersected in the presence of large Jewish communities, although in their setting this is driven by the predominance of Jews in certain middleman occupations. Kopstein and Wittenberg (2018), in turn, build on Blalock (1967) to argue that pogroms in the eastern Polish borderlands in 1941 were more likely where large Jewish communities posed a threat to the political control of non-Jews (see also Kopstein and Wittenberg 2011). Such perceptions of a common threat against a numerous out-group may be encouraged by the tendency of affectively similar groups to coalesce around a common identity as more dissimilar groups increase in size (Fouka and Tabellini 2022; see also Adida, Laitin, and Valfort 2016) and when ethnic markers are clear (Caselli and Coleman 2013).

Finally, we contribute to recent scholarship on the origins and consequences of anti-Jewish violence before the Holocaust. In addition to the work cited just above, central contributions include Anderson, Johnson, and Koyama (2017), Finley and Koyama (2018),

Becker and Pascali (2019), Jedwab, Johnson, and Koyama (2019), and Johnson and Koyama (2019), who examine the institutional determinants of pogroms and other persecutions at various points in medieval, Renaissance, and early modern European history. Voigtländer and Voth (2012), in turn, trace the persistence of anti-Semitic violence from the medieval era to the 20th century, while Grosfeld, Rodnyansky, and Zhuravskaya (2013) demonstrate the legacy of anti-Semitism, expressed in part through anti-Jewish pogroms, for contemporary market attitudes. Not least, Spitzer (2021) examines the impact of pogroms in the late Russian Empire on Jewish migration to the United States, a theme explored more generally in the survey of Becker, Mukand, and Yotzov (2022).

The paper proceeds as follows. We first discuss the historical context of Jewish settlement in the Russian Empire and the events of the 1905 Russian Revolution. We then describe our data and empirical strategy. Following this, we present our results alongside numerous robustness checks. Finally, we discuss the manner in which existing perspectives on ethno-religious diversity and conflict are both helpful and misleading in understanding violent backlash to political reform, and we show how such backlash should be properly understood.

1 Historical Context

The presence of a significant Jewish population in the western lands of the Russian Empire dated from the Partitions of Poland in the late eighteenth century, when Russia annexed territories in what had to that point been the Polish-Lithuanian Commonwealth. Over previous centuries, the owners of private towns in the Commonwealth had invited Jews to settle in anticipation of economic benefits. Now in control, Russia decreed that Jewish residence would generally be restricted to its newly acquired lands, which also included the southern territories of "Novorossiya" and, from 1815, the Duchy of Warsaw. Together, this region constituted the so-called Pale of Settlement (*cherta osedlosti*). For Jews, freedom of movement and economic opportunity fluctuated over the nineteenth century, with a secular decline that marked the transition from a "golden age," shortly after the Partitions, to the impoverished *shtetls* of popular understanding (Petrovsky-Shtern 2014). In an environment of widespread anti-Semitism, the assassination of Tsar Alexander II sparked "pogroms" (the neologism accompanied the events) across southwestern Russia in 1881 and 1882. These, in turn, perversely encouraged the promulgation of the May Laws of 1882, which further restricted Jewish residency and economic rights.

The first years of the twentieth century saw significant and interconnected economic, social, and political crises in Imperial Russia. Russia stumbled into a war with Japan in 1904, which it proceeded to lose. The Russo-Japanese War, in turn, was a prime—though not the only—cause of the 1905 Russian Revolution, which followed two years of social upheaval

in the cities and the countryside. In October 1905, following a general strike that included the economically critical railway sector, Tsar Nicholas II issued a manifesto establishing a national parliament (Duma), ceding some personal authority, and decreeing broad civil and political rights. The so-called Days of Freedom that followed saw widespread celebrations-cum-demonstrations against the autocratic regime. These events, in turn, spurred a reactionary response that, in many settlements of the Pale, took the form of anti-Jewish pogroms.

The "Easter" pogrom in Kishinev in April 1903 was the initial event in this large wave of anti-Jewish violence and rioting—the first since 1881–2. As documented in Table 1, this violent episode did not immediately presage a wider series of pogroms. Rather, the wave began slowly, with a large riot in Gomel (contemporary Belarus) in September 1903; the beginning of the main wave in May 1904; an increase in frequency in the fall of 1904, as wartime conscription generated public anger against Jews, who were alleged to have collaborated with the Japanese (Lambroza 1992); a slowing in the winter of 1904–5; and then growth in the spring and summer of 1905. There was then a massive surge in anti-Jewish pogroms following publication of the October Manifesto on October 17, 1905 (Julian calendar, corresponding to October 30 in our data, which follow the Gregorian calendar), with over 250 pogroms through the subsequent month. Following this surge, the wave ended in the middle of 1906, as the state regained its monopoly on violence, in no small part due to the pogroms themselves: "the monarchy and its rightwing allies (including pogromists) [had retaken] control of the streets, while the erstwhile revolutionaries retreated and retrenched" (Surh 2024, p. 223).[3] As Figure 2 illustrates, pogroms were distributed broadly across the Pale of Settlement, with some clustering in particular areas.

Many pogromists apparently acted with the conviction that Jews were responsible for the loss of Tsarist authority. Jews were popularly associated with revolutionary politics in late Imperial Russia, and indeed Jews were active in opposition movements, including the Bund and various Zionist-socialist parties (Ascher 2008; Boix 2025): from 1903 to 1905, Jews represented more than 30 percent of political arrestees (Frankel 2009). Many Jews actively celebrated the October Manifesto, and as celebrations turned into demonstrations against the Tsar-Father (*Tsar-Batiushka*) still revered by many Russians, Jews and Jewish properties were targeted for violence (Ascher 2004). Typical was the reaction in Kyiv, where pogromists shouted, "There's your freedom, there's your constitution and revolution, there's your crown and portrait of our Tsar" (Hillis 2013, p. 167). Our own analysis suggests that political considerations played a role in nearly all pogroms documented by the Zionist Organization

[3]Official investigations into the pogroms were limited in scope, and the few punishments that were imposed were often reversed through pardons granted by the Tsar (Khiterer 2015, p. 800).

Table 1: Anti-Jewish Pogroms in Russia, 1904–6

		Spitzer		Our sample	
Month	Year	Settlements	Pogroms	Settlements	Pogroms
5	1904	2	2	2	2
9	1904	6	6	4	4
10	1904	16	16	15	15
11	1904	11	11	10	10
2	1905	3	3	3	3
3	1905	1	1	1	1
4	1905	6	7	6	7
5	1905	11	13	8	10
6	1905	4	4	4	4
7	1905	2	2	2	2
8	1905	9	11	8	10
9	1905	4	4	4	4
10	1905	15	16	12	13
11	1905	204	264	107	160
12	1905	9	9	3	3
1	1906	7	7	5	5
6	1906	5	5	1	1
	Total	268	381	154	254

Note: "Spitzer" refers to data compiled by Yannay Spitzer for Spitzer (2021) and kindly provided by the author. Our sample includes settlements (and pogroms) in the Pale of Settlement that could be geolocated and matched to the 1897 census data: see text for details. Some settlements experienced multiple pogroms.

of Berlin, of which more below, although these motives typically intersected with the more mundane desire to loot Jewish establishments and residences.[4]

Of course, even if many revolutionaries were Jews, most Jews were not revolutionaries. The confusion of one conditional probability for another was encouraged by anti-Semitic propaganda, with the implicit or explicit support of authorities.[5] ("The wildest news travels along the telegraph wires, sometimes bearing an official stamp," wrote Trotsky [1922/2016, p.

[4]We support this interpretation with an LLM-assisted analysis of the German-language reports in Motzkin (1910), as described in the Appendix. Hillis (2013, p. 169) cites an official list of "primary participants" in the Kyiv pogrom, which we obtained from the author. Among the 26 listed individuals, there were many members of the petty bourgeoisie (*meshchane*), alongside known criminals, whose participation was apparently more prominent later in the riot, and some peasants.

[5]Petersen (2002) suggests that "Perpetrators acting under the influence of Rage selectively use evidence and develop faulty or irrational beliefs."

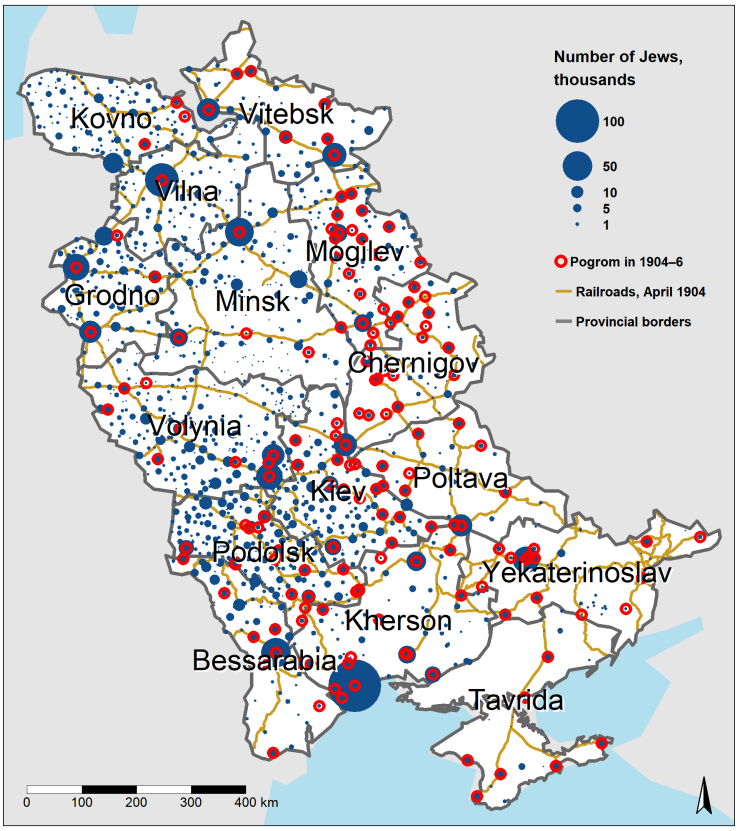

Figure 2: Jewish Communities and Pogroms in Russia, 1904–6. See text for additional information.

112].) Whatever the truth of depositions and official accounts produced after the pogroms, rumors of Jewish desecrations spread easily (Hillis 2013, pp. 164–67). Moreover, Jewish efforts at self-defense, prompted initially by the large pogroms in Kishinev and Gomel, fueled paranoia that Jews were arming to overthrow the state. The upshot was that "the principal belief animating 1905's pogroms identified Jews as an alien, disloyal threat, the chief promoters and beneficiaries of revolution" (Surh 2024, p.18).

Historians have emphasized the relative size of the Jewish population as an important determinant of anti-Semitic attitudes and violence during the 1905 Revolution, though not always in uniform fashion. Weinberg (2021, p. 86), for example, writes, "The large number of Jews [some 35% of the population] and their visible presence in the commercial and industrial life of the city contributed to resentment against Odessa's Jewish community." Surh (2024, p. 131) echoes this sentiment, arguing that the "high proportion of Jews [in Orsha, site of a pogrom in October 1905] possibly accounts for both a greater self-assurance and assertiveness felt by the Jewish community, which would have only increased the threat Orsha's Gentile population already felt from that level of Jewish ascendance." Steinberg (2017, p. 135)

similarly asserts that, once the pogroms began, "Where Jews were a large part of a city's local population, right-wing violence reached extremes." In contrast, Lambroza (1992, p. 209) emphasizes the protection afforded by a comparatively large Jewish population, noting, "At Gomel local administrators were more responsive to Jews [than in Kishinev], possibly because Jews made up 50 percent of the population." Surh (2024, p. 27), in turn, suggests that the pogroms in Gomel and (later) Zhytomyr "occurred in towns with sizeable, diverse, and well-organized Jewish communities that were clearly aware of the Kishinev events, and so, capable of contesting another massacre of Jews in their own locales." In the analysis to follow, we investigate such claims systematically.

2 Data

We explore the incidence of pogroms in Jewish settlements in the non-Polish provinces of the Pale of Settlement (henceforth "Pale of Settlement" or simply "Pale") from 1904 to 1906.[6] Geocoded information on these settlements comes from Spitzer (2021), who uses published data from the 1897 census (Troinitskii 1905) and from the Communities Database of the genealogy website JewishGen (`jewishgen.org/communities`).[7] The settlements range in size from small villages to provincial capitals, and they include a range of formal designations: *derevnia* (village), *selo* (church village), *mestechko* (small commercial town, roughly synonymous with the colloquial *shtetl*), *gorod* (city), and so forth. Settlements are nested in districts (*uezdy*), which in turn are nested in provinces (*gubernii*). In our empirical work, we classify settlements as *Urban* if they are provincial or district towns (*gubernskii gorod*, *uezdnyi gorod*, or similar), retain *Mestechko* (plural *mestechki*) as a distinct category, and classify all other settlements as *Rural*.

For settlements in the Russian empire with at least 500 inhabitants, Troinitskii (1905) provides information on the total population and on the number of residents belonging to each religious group constituting at least 10% of the settlement, with groups falling below that threshold categorized as "other." We digitize these data to define our sample (of which more below); to tally settlement-level *(Log) population*; and to derive the variables *Share of Jews*, which is the proportion of the settlement's population identified as Jewish, and *Share of Jews* × (*1 - Share of Jews*) (hereafter, often *Polarization*). As Figure 3 shows, the

[6]Congress Poland in 1905 was administratively and politically distinct from the Pale, with revolutionary violence centered on Polish nationalist and socialist mobilization. Pogroms did occur, but typical accounts present them as secondary to the broader revolutionary conflict.

[7]Spitzer (2021) defines a settlement as Jewish if either Troinitskii (1905) provides the number of Jewish residents or the settlement is included in the JewishGen Communities Database. The latter condition adds some 300 additional, typically smaller settlements to the data. We provide summary statistics in Table A1.

population share of Jews takes a wide range of values for every class of settlements, though *mestechki* tend to be more polarized. Moreover, as Figure 4 illustrates, there is substantial spatial variation in both Jewish share and polarization, even after conditioning on province fixed effects, total population, and indicators for settlement type.[8] This variation, and in particular the fact that the variable Share of Jews spans nearly the full range of possible values, allows us to distinguish between any effect of Jewish population share and that of polarization: for Share of Jews less than 0.5, both this variable and Polarization move in the same direction, whereas for values greater than 0.5 the opposite is true.[9]

Our main sample thus comprises 1,370 geo-referenced settlements, including 26 settlements for which Troinitskii anomalously records a Jewish population share under 10% and 14 urban settlements for which the number of Jewish residents is not provided (because it was under 10%) but for which we are able to back out that quantity from published *uezd*-level data from the 1897 census. We exclude 25 Jewish settlements identified by Spitzer (2021) but with unknown geographic coordinates, as well as 145 smaller localities from JewishGen's Communities Database that are not included in Troinitskii (1905). Estimates of Jewish population are available for only 39 out of these 170 communities; together, these account for just 0.2% of the Jewish population of the Pale in 1897 (Spitzer 2021). The remaining Jewish population in the Pale was distributed across small villages and hamlets or constituted less than 10% of the population in larger settlements. In total, the Jewish population in our sample accounts for approximately 83% of the Jewish population of the Pale at the turn of the century.[10]

Spitzer (2021) also provides geo-referenced data on pogroms, building on the work of surveyors from the Zionist Organization of Berlin (Motzkin 1910) and the 1906–7 edition of the *American Jewish Year Book* ("From Kisheneff to Bialystok").[11] As our outcome of

[8]Figure A1 illustrates balance across various covariates for both Jewish share and polarization.

[9]The 1897 census additionally provides data on language use at the district (not settlement) level, which we depict alongside religious composition in Figure A2. The pairwise correlation between share of the district population speaking Yiddish and share identified as Jewish is 0.999.

[10]As discussed below, we check the robustness of our results using an extended sample with an additional 73 settlements that are (i) identified by Spitzer (2021) as having Jewish communities, (ii) included in Troinitskii (1905), but (iii) have Jewish share under his 10% threshold. For these settlements, we impute number of Jews as the number of individuals in the residual "other" category.

[11]Figures A3 and A4 depict typical entries from these sources. Spitzer's data are more comprehensive for this wave of pogroms than those collected by Grosfeld, Rodnyansky, and Zhuravskaya (2013) and Grosfeld, Sakalli, and Zhuravskaya (2020), who do not use Motzkin (1910).

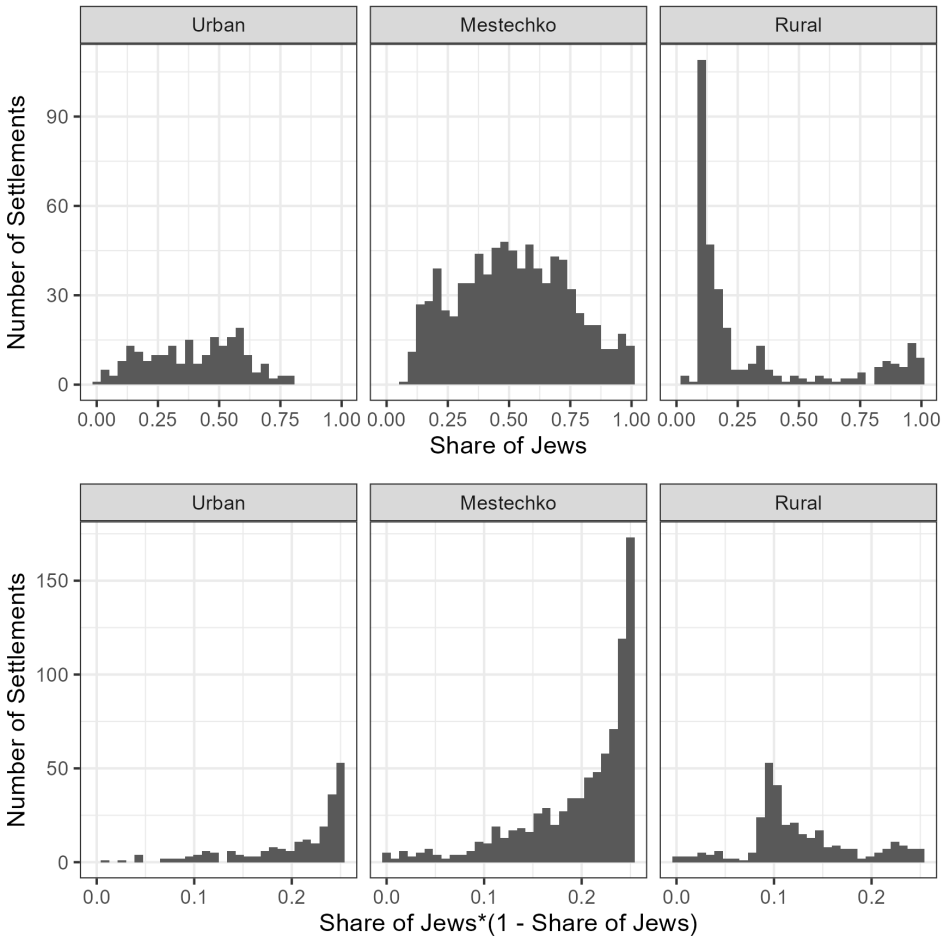

Figure 3: Population Share of Jews (Top) and Religious Polarization (Bottom), 1897. Of the 1370 settlements in our sample, 16% are urban, 61% are small commercial towns (*mestechki*), and 23% are rural.

interest, we define the indicator *Pogrom*, which, for the unit of observation—settlement-month—takes a value of one if the data record at least one pogrom.[12] Spitzer's data list 381 pogroms in the Pale of Settlement (excluding Poland) during this period; as discussed, some of these are in (typically small) settlements that are not geolocated or cannot be matched to the 1897 census data. After excluding such events, we observe 254 pogroms from May 1904 to June 1906, with some of the 154 affected settlements experiencing pogroms in multiple months (see Table 1). Below, we describe other variables used in our analysis.

Appendix Tables A2 and A3 present conditional correlates of pogrom incidence in the

[12]We exclude five pogroms in 1903, including the Kishinev and Gomel pogroms discussed above, which served as precursors to the main wave that began in May 1904. Below, we report robustness to extending the sample back to April 1903. After June 1906, there were almost no recorded pogroms in the Russian Empire until World War I.

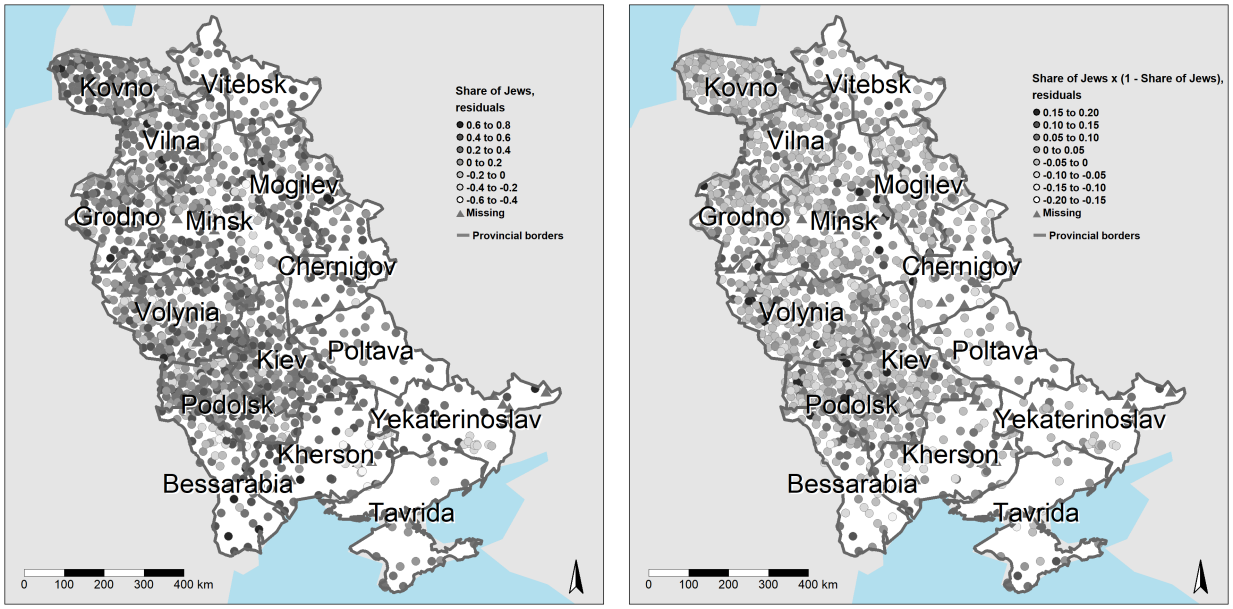

Figure 4: Population Share of Jews (Left) and Religious Polarization (Right), 1897. These figures depict residuals after regressing the two measures on province fixed effects, total settlement population, and indicators for settlement type.

period before the spike in pogroms depicted in Figure 1. We collapse the data into a cross-section, where the outcome is the binary incidence of at least one pogrom between May 1904 and October 1905, inclusive. We include provincial fixed effects in all regressions. Table A2 demonstrates that Jewish population share is significantly correlated with pogrom incidence prior to November 1905, but the magnitude is quite small: using the estimates in Columns 4–8, an increase in Jewish share of one standard deviation (approximately 26 percentage points) is associated with an increase in pogrom probability of roughly 1.8 percentage points, or 9 percent of the pre-treatment outcome standard deviation of 21 percent.[13] Moreover, as Table A3 shows, there is no robust relationship between polarization and pogrom incidence in the pre-treatment period. With respect to both Jewish population share and polarization, in other words, pogroms are, roughly speaking, neither more nor less likely in the period before the October Manifesto. In the following section, we describe our empirical strategy to identify the effect of that reform on pogrom incidence by fully exploiting the panel nature of our data.

[13]As we discuss below, in some panel exercises, we condition on prior pogrom incidence to account for the possibility that settlements with comparatively large Jewish populations were targeted first.

3 Empirical strategy

Our empirical strategy is difference-in-differences. Our baseline specification is the two-way fixed effects (TWFE) regression

$$y_{spt} = \kappa \times \Pi_s \times Z_t + \mathbf{X}_{st}\beta + \phi_s + \alpha_t + \gamma_p \times Z_t + \epsilon_{spt}, \tag{1}$$

where Π_s is either $JewishShare_s$ or polarization, $JewishShare_s \times (1 - JewishShare_s)$, and Z_t is an indicator for months after October 1905. The outcome y_{spt} is the binary incidence of a pogrom in settlement s in province p during month t, where $t = 1, 2, \ldots, 26$ indexes months from May 1904 to June 1906. The vector $\mathbf{X}_{st}$ is a list of time-varying covariates; in most specifications, these include (standardized) temperature and precipitation deviations from the long-run average.[14] We include settlement fixed effects ϕ_s and month fixed effects α_t. We also typically allow for differential trends at the provincial (*guberniia*) level, before versus after the October Manifesto, as captured by the interaction term $\gamma_p \times Z_t$. In some specifications, we additionally allow for differential trends by time-invariant characteristics (1897 census population and indicators for settlement type) as well as by temperature and precipitation deviations. The variable ϵ_{spt} is an idiosyncratic error term.

Recent scholarship—unpublished, as of this writing—has explored the properties of this estimator in the context of a more general reevaluation of difference-in-differences designs. With a continuous treatment ($\Pi_s \times Z_t$), the TWFE estimate is contaminated with selection bias associated with treatment-effect heterogeneity under the standard parallel-trends assumption. Callaway, Goodman-Bacon, and Sant'Anna (2024) demonstrate that this bias is absent under an alternative assumption, which they term "strong parallel trends," which restricts the path of potential outcomes at all doses d, not just at the untreated dose $d = 0$.[15] (Here, the "dose" is either Jewish share or polarization.) Under this assumption, the TWFE estimator recovers the weighted average of the average causal response (the change in the treatment effect from a marginal change in the dose) at all possible doses. The weights—all positive—are centered on the expected value of the dose, which, as Figure 3 illustrates, implies for our setting that the TWFE estimate leans on values of the treatment well represented in the empirical distribution. (To further characterize the dose-response relationship,

[14] Broadly, these climate variables pick up economic shocks in what was a largely agrarian economy. Such shocks—or even the weather more directly—may have shaped the perceived costs or benefits of conflict for affected groups (Anderson, Johnson, and Koyama 2017; Grosfeld, Sakalli, and Zhuravskaya 2020).

[15] See also Xu, Zhao, and Ding (2024), who examine the two-group (i.e., discrete rather than continuous treatment), two-period version of what they call "factorial difference-in-differences."

we additionally report results from "binning" regressions that allow for heterogeneity across quintiles of the treatment variable.)

Broadly speaking, the assumption of strong parallel trends rules out selection-on-gains into the treatment dose. In our setting, such selection could take one of two forms. First, Jews may have migrated (before 1897, the year that subjects of the Empire were enumerated) to places that were more or less likely to respond to reform with violence—say, to settlements in which previous shocks did not generate pogroms. Second, even absent internal migration, settlements with more or fewer Jews may have been different in ways that affected responsiveness to reform other than through the direct effect of religious composition—for example, through proximity to provincial capitals, the proximate source of much anti-Semitic propaganda. In what follows, we report numerous robustness checks to account for such explicit or implicit sorting of Jewish population across the Pale.

To account for the spatial and temporal correlation of pogroms, we report Conley standard errors with a 50-km distance cutoff and a two-period lag. In addition, in some specifications we include a spatial lag (of pogroms), $\mathbf{W}\mathbf{y}_t$, where $\mathbf{y}_t$ is the vector of pogrom incidence at time t for all settlements in the sample. The spatial weight matrix, $\mathbf{W}$, has zeroes along the diagonal and for $i \neq j$ takes a value of one if settlement j is within 50 km of settlement i and zero otherwise.[16]

4 Results

Baseline estimates and sensitivity analysis

In Tables 2 and 3, we present results from our baseline difference-in-differences design. In Table 2, where we identify the effect of Jewish population share on (the change in) pogrom incidence, the treatment effect is everywhere negative, with significance at the 5% level or better. The saturation of models with controls somewhat reduces the magnitude of the estimated effect, but the qualitative result is robust to controlling for differential trends by settlement type, population, and temperature/precipitation deviations (Column 4), as well as to accounting for the spatial lag in the distribution of pogroms, with possibly different effects before and after the October Manifesto (Column 5). If we take the coefficient on the interaction equal to -0.008—roughly the average value across Columns 2–5—then an increase of one standard deviation in the share of Jews in a settlement implies a decrease in pogrom incidence of more than one-third its mean value.

The left panel of Figure 5 plots coefficients from the corresponding event study, where

[16] As our sample comprises settlements with substantial Jewish communities, we implicitly bracket the question of what happens when violence "reaches" settlements without potential victims, as in Klašnja and Novta (2016).

Table 2: Share of Jews and Pogrom Incidence

	(1)	(2)	(3)	(4)	(5)
Share of Jews × Post–Oct 1905	−0.0151***	−0.0091**	−0.0082**	−0.0086**	−0.0077**
	(0.0037)	(0.0039)	(0.0038)	(0.0039)	(0.0038)
Temperature deviation		0.0034***	0.0023**	0.0043***	0.0033***
		(0.0011)	(0.0011)	(0.0011)	(0.0011)
Precipitation deviation		0.0012	0.0005	0.0000	0.0001
		(0.0008)	(0.0007)	(0.0004)	(0.0004)
W × pogroms			0.0388***		0.0233**
			(0.0066)		(0.0101)
Urban × Post–Oct 1905		0.0176***	0.0173***	0.0065	0.0062
		(0.0053)	(0.0052)	(0.0053)	(0.0051)
Mestechko × Post–Oct 1905		0.0057**	0.0051**	0.0016	0.0009
		(0.0026)	(0.0025)	(0.0029)	(0.0028)
Log Population × Post–Oct 1905				0.0051***	0.0050**
				(0.0020)	(0.0019)
Temperature deviation × Post–Oct 1905				−0.0025	−0.0031
				(0.0031)	(0.0027)
Precipitation deviation × Post–Oct 1905				0.0038	0.0002
				(0.0025)	(0.0022)
W × pogroms × Post–Oct 1905					0.0241*
					(0.0135)
Settlement fixed effects	✓	✓	✓	✓	✓
Month fixed effects	✓	✓	✓	✓	✓
Province fixed effects × Post–Oct 1905		✓	✓	✓	✓

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.10$. In each column, 1,370 settlements and 35,620 observations, with a mean outcome of 0.0055. See text for additional details and variable definitions.

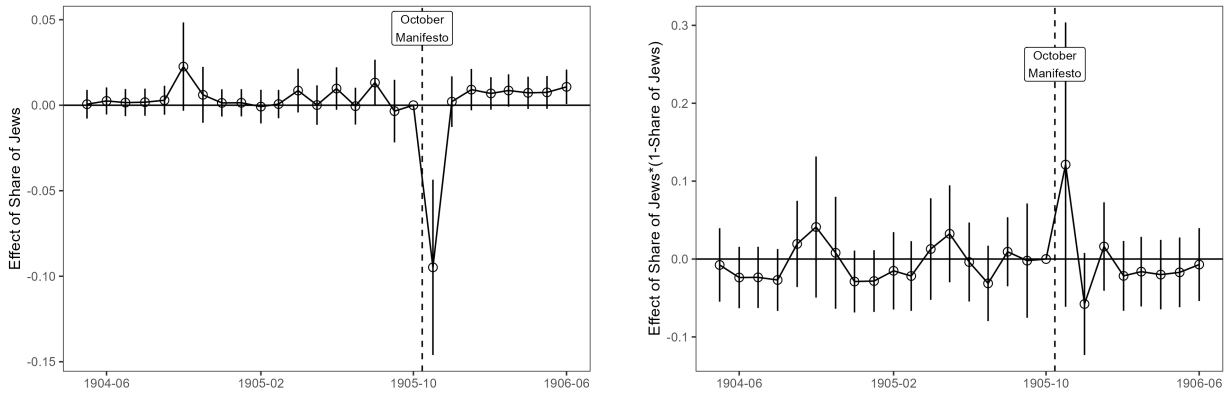

Figure 5: Event Studies: Share of Jews (left) and Religious Polarization (right). Other than time-varying treatment effects, the specification corresponds to Column 5 of Tables 2 and 3, respectively.

Table 3: Polarization and Pogrom Incidence

	(1)	(2)	(3)	(4)	(5)
Share of Jews × (1 - Share of Jews)	−0.0029	0.0047	0.0036	−0.0104	−0.0105
× Post–Oct 1905	(0.0132)	(0.0157)	(0.0153)	(0.0167)	(0.0160)
Temperature deviation		0.0034***	0.0023**	0.0043***	0.0033***
		(0.0011)	(0.0011)	(0.0011)	(0.0011)
Precipitation deviation		0.0012	0.0005	0.0000	0.0001
		(0.0008)	(0.0007)	(0.0004)	(0.0004)
W × pogroms			0.0388***		0.0233**
			(0.0066)		(0.0101)
Urban × Post–Oct 1905		0.0162***	0.0161***	0.0059	0.0057
		(0.0054)	(0.0053)	(0.0053)	(0.0050)
Mestechko × Post–Oct 1905		0.0036	0.0033	0.0003	−0.0002
		(0.0026)	(0.0025)	(0.0028)	(0.0026)
Log Population × Post–Oct 1905				0.0053***	0.0052***
				(0.0020)	(0.0020)
Temperature deviation × Post–Oct 1905				−0.0024	−0.0030
				(0.0031)	(0.0027)
Precipitation deviation × Post–Oct 1905				0.0039	0.0002
				(0.0025)	(0.0022)
W × pogroms × Post–Oct 1905					0.0241*
					(0.0135)
Settlement fixed effects	✓	✓	✓	✓	✓
Month fixed effects	✓	✓	✓	✓	✓
Province fixed effects × Post–Oct 1905		✓	✓	✓	✓

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.10$. In each column, 1,370 settlements and 35,620 observations, with a mean outcome of 0.0055. See text for additional details and variable definitions.

we interact *Jewish share* with indicators for pre- and post-treatment months, and otherwise follow the specification in Column 5 of Table 2. Pre-trends can be considered parallel: indeed, any noticeable deviations from zero in the effect of share of Jews before October 1905 are positive rather than negative. The negative effect of Jewish population share post-treatment is concentrated in the first month following publication of the October Manifesto, with a reversion to the pre-treatment baseline (or slightly greater than that) in the remaining sample period. For ease of presentation, we report results with a uniform treatment effect in the various robustness checks and alternative specifications reported below.

In contrast, the results in Table 3 demonstrate no robust relationship between ethno-religious polarization and pogrom incidence in our sample period. (As discussed above, we are able to distinguish between any effect of polarization and that of Jewish population share because the variable *Share of Jews* spans nearly the full range of possible values in our sample.) Across all specifications, the estimated treatment effect is small and insignificant,

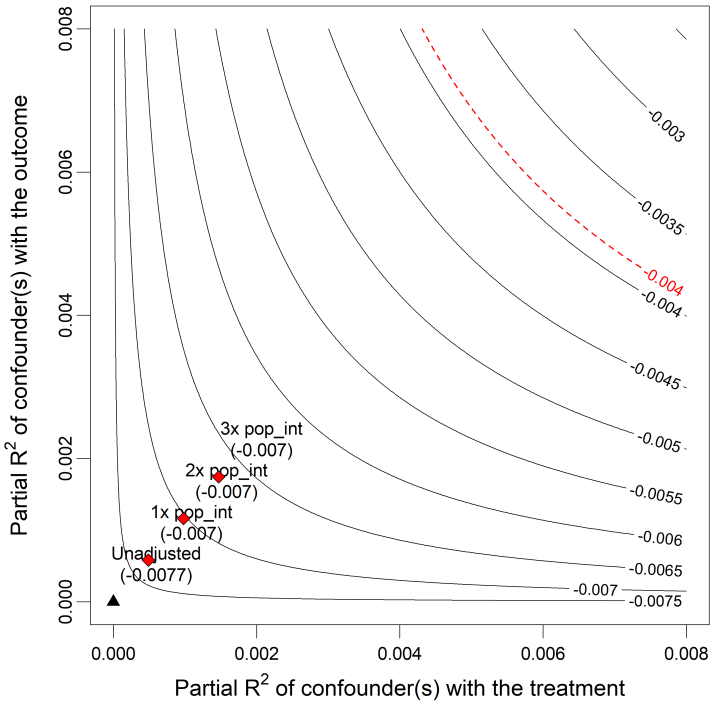

Figure 6: Sensitivity Analysis. The figure follows Cinelli and Hazlett (2020) in showing how much confounding would be necessary to overturn our results. Even confounding three times as strong as the interaction between population size and the timing of the October Manifesto would reduce the differential effect of Jewish population share by less than half that estimated in Column 5 of Table 2.

with inconsistent sign. The event study in the right panel of Figure 5 provides suggestive evidence of a positive effect of polarization in November 1905, but the estimated coefficient is statistically insignificant. Given the strong contrast with the results in Table 2 and associated event plot, we focus in what follows on Jewish population share rather than polarization.

Although the regressions in Table 2 control for a great deal of heterogeneity, including differential provincial trends, our results could nevertheless be biased by unobserved confounders. Figure 6 uses the method in Cinelli and Hazlett (2020), a bounding exercise in the spirit of Oster (2019), to show how much confounding would be necessary to overturn our results. Even confounding three times as strong as the interaction between population size and the timing of the October Manifesto would reduce the differential effect of the Jewish population share by less than half that estimated in Column 5 of Table 2.

Under the assumption of strong parallel trends, the TWFE estimator recovers the weighted average of the causal response at all possible doses—here, Jewish share—with more weight placed on doses close to the mean. Table 4 further characterizes the dose-response rela-

Table 4: Binning Regressions

	(1)	(2)	(3)	(4)	(5)
Share of Jews, 2nd Quintile	0.0034	−0.0010	0.0001	−0.0009	0.0001
× Post–Oct 1905	(0.0033)	(0.0037)	(0.0036)	(0.0037)	(0.0036)
Share of Jews, 3rd Quintile	−0.0056*	−0.0058	−0.0052	−0.0071*	−0.0064*
× Post–Oct 1905	(0.0031)	(0.0039)	(0.0038)	(0.0040)	(0.0038)
Share of Jews, 4th Quintile	−0.0041	−0.0027	−0.0020	−0.0042	−0.0033
× Post–Oct 1905	(0.0033)	(0.0041)	(0.0039)	(0.0041)	(0.0039)
Share of Jews, 5th Quintile	−0.0087***	−0.0064*	−0.0054	−0.0062*	−0.0052
× Post–Oct 1905	(0.0028)	(0.0036)	(0.0034)	(0.0035)	(0.0034)
Settlement fixed effects	✓	✓	✓	✓	✓
Month fixed effects	✓	✓	✓	✓	✓
Province fixed effects × Post–Oct 1905		✓	✓	✓	✓

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.10$. These models estimate binning regressions to examine the association between Jewish population share and increase in pogrom incidence (after the Manifesto) across the distribution of Jewish share. Columns 2–5 include temperature and precipitation deviations and interactions of post–Oct 1905 with indicators for settlement type. Columns 4 and 5 additionally include interactions of post–Oct 1905 with population and temperature and precipitation deviations, while Columns 3 and 5 additionally include a spatial lag of the dependent variable and its interaction with post–Oct 1905, respectively. Consistent with the observation that the effect of Jewish share is distinguishable from that for polarization only for Jewish share greater than 0.5, the results show that the negative effect of Jewish share is driven by the difference between the third through fifth quintiles and the first and second quintiles. In each column, 1,370 settlements and 35,620 observations, with a mean outcome of 0.0055.

tionship by partitioning Jewish share into quintiles. Across specifications, we find a strong contrast between lower and higher Jewish shares, with uncertain differences among the highest quintiles. (As discussed, the contrast between the lowest and highest Jewish shares is especially informative, as polarization is a monotone transformation of Jewish share for values of Jewish share less than 0.5.)

Robustness

To more systematically explore the robustness of the relationship between Jewish population share and the incidence of pogroms, and to account for factors that might otherwise violate the assumption of strong parallel trends, we examine various changes to specification and sample suggested by the historiography of the 1905 Russian Revolution.

Proximity to provincial capitals and railroads: A recurring motif in the literature on the 1905 Russian Revolution is the diffusion of violence across Jewish settlements.[17] In Tables 2 and 3, we account for this possibility through the inclusion of a spatial lag, alone and in interaction with a post–October dummy, but we can also directly control for

[17]In the LLM-assisted analysis we summarize in the Appendix, roughly two-thirds of pogroms were perpetrated by a mix of local residents and outsiders, though seemingly only one pogrom was actually initiated by non-locals.

Table 5: Proximity to Provincial Capitals and Railroads

	(1)	(2)	(3)
Share of Jews × Post–Oct 1905	−0.0088**	−0.0085**	−0.0090**
	(0.0039)	(0.0039)	(0.0039)
Travel time to provincial capital × Post–Oct 1905	−0.1296		
	(0.0839)		
Travel time to provincial capital > median × Post–Oct 1905		−0.0040*	
		(0.0021)	
Distance to railroad × Post–Oct 1905			−0.1310**
			(0.0577)
Settlement fixed effects	✓	✓	✓
Month fixed effects	✓	✓	✓
Province fixed effects × Post–Oct 1905	✓	✓	✓

***$p < 0.01$; **$p < 0.05$; *$p < 0.10$. Travel time to provincial capital is defined using time in hours on road and rail between the unit of observation and the nearest provincial capital. Russia's rail network is measured as of April 1904. All regressions include temperature and precipitation deviations, along with interactions of post–Oct 1905 with indicators for settlement type, population variables, and these temperature and precipitation deviations. In each column, 1,370 settlements and 35,620 observations, with a mean outcome of 0.0055.

potential channels of contagion. Anti-Semitic propaganda was often published in provincial capitals, and both propaganda and pogromists traveled along (or alongside, via telegraph lines) Russia's rail system. In Table 5, we allow for differential trends by travel time to the provincial capital (measured either in hours or by an indicator that takes a value of one if travel time is greater than median) and by distance to the nearest railroad.[18] All three interactions work as anticipated, but their effect is essentially orthogonal to the treatment effect of Jewish population share.

Segregation (sample excluding provincial capitals): Beyond the presence of anti-Semitic propaganda, provincial capitals were different in another respect: Jews in these larger cities were more likely to live in segregated communities than was the case in other settlements (Surh 2024, p. 209). Following Allport (1954), the absence of regular contact between Jews and non-Jews may have encouraged violent backlash to reform—especially so, perhaps, where segregated Jewish communities were small and vulnerable. Although we already account for differential trends by type of settlement, we further check robustness to

[18]We measure travel time as the least-cost path to the provincial capital traveling afoot and/or by rail. Following Nurutdinov (2019, pp. 130–136), we assume the following effective speed of travel: 1.56 km per hour afoot, 35.20 km per hour by rail. Unlike in Nurutdinov (2019), we disregard train fares and omit water transportation. We assume terrain to be flat. For calculations, we employ the R package `gdistance`. GIS data on railroads are from Kofanov (2020).

Table 6: Segregation (Sample Excluding Provincial Capitals)

	(1)	(2)
Share of Jews × Post–Oct 1905	−0.0089**	−0.0081**
	(0.0039)	(0.0037)
Settlement fixed effects	✓	✓
Month fixed effects	✓	✓
Province fixed effects × Post–Oct 1905	✓	✓

***$p < 0.01$; **$p < 0.05$; *$p < 0.10$. The sample excludes settlements that are also provincial capitals, in which residential segregation of Jews was typically greater. All regressions include temperature and precipitation deviations, along with interactions of post–Oct 1905 with indicators for settlement type, population variables, and these weather deviations. Column 2 additionally includes a spatial lag of the dependent variable and its interaction with post–Oct 1905. In each column, 1,355 settlements and 35,230 observations, with a mean outcome of 0.0047.

dropping all provincial capitals. Table 6 shows that our estimates are little affected by this sample restriction.

Middleman occupations: Jews in Eastern Europe were the quintessential "middleman" minority, facilitating trade and finance to non-Jews—a role that may have alternately encouraged scapegoating and discouraged the splintering of economic relationships.[19] In Table 7, we interact the post-treatment indicator with the 1897 district-level share of Jews among creditors and grain traders, respectively, using data from Grosfeld, Sakalli, and Zhuravskaya (2020). In both cases, the treatment effect of Jewish population share is very similar to that in our baseline specifications.[20]

Past pogroms: In Table 8, we include a (time-varying) indicator for the prior incidence of a pogrom during the sample period to account for the possibility that settlements with larger Jewish communities were targeted first. The regression estimates support the conjecture that pogroms are less likely in settlements where a pogrom has already occurred,

[19]Bauer et al. (2023) provide experimental evidence of minority scapegoating, which is widely seen as contributing to pogroms and other mob violence. For the importance of inter-ethnic occupational complementarities in reducing ethnic violence, see, e.g., Jha (2013), Becker and Pascali (2019), and Jedwab, Johnson, and Koyama (2019).

[20]Grosfeld, Sakalli, and Zhuravskaya (2020) tie the incidence of pogroms from 1800 to 1927 to the presence of Jews in middlemen occupations when political turmoil and economic shocks coincide. For our sample period, which Grosfeld et al. code as political turmoil, there is limited variation in economic shocks, operationalized as in Grosfeld et al.: no hot spring anywhere in 1904 or 1905, hot spring almost everywhere in 1906 (with the wave of pogroms that we examine ending before the 1906 harvest).

Table 7: Middleman Occupations

	(1)	(2)	(3)	(4)
Share of Jews × Post–Oct 1905	−0.0089**	−0.0077**	−0.0092**	−0.0080**
	(0.0039)	(0.0038)	(0.0040)	(0.0038)
Share of Jews among creditors × Post–Oct 1905	−0.0067	−0.0029		
	(0.0052)	(0.0042)		
Share of Jews among grain traders × Post–Oct 1905			−0.0125	−0.0154
			(0.0253)	(0.0228)
Settlement fixed effects	✓	✓	✓	✓
Month fixed effects	✓	✓	✓	✓
Province fixed effects × Post–Oct 1905	✓	✓	✓	✓

***$p < 0.01$; **$p < 0.05$; *$p < 0.10$. All regressions include temperature and precipitation deviations, along with interactions of post–Oct 1905 with indicators for settlement type, population variables, and these temperature and precipitation deviations. Columns 2 and 4 additionally include a spatial lag of the dependent variable and its interaction with post–Oct 1905. In each column, 1,367 settlements and 35,542 observations, with a mean outcome of 0.0055.

though, as before, there is little effect on our baseline results. Similarly, using data from Spitzer (2021), we allow for differential trends by presence of a pogrom in 1881–2, as, in principle, Jews might have moved after that wave to settlements with a lower propensity for violence. We find no such pattern, consistent with Spitzer's (2021) finding that this first large wave of pogroms did not spur Jewish emigration, at least from Russia; again, our estimates of the main treatment effect are qualitatively unchanged.[21]

Industrial workers and industrial output: Table 9 follows Wynn (1992) in considering the possibility that the October Manifesto generated particular hostility among industrial workers, who had joined the general strike hoping for better working conditions rather than political reform. The inclusion of interactions between the post–October dummy and industrial employment or output does little to change our baseline estimates.[22]

[21]To be precise, Spitzer (2021) finds no association between the location of pogroms in 1881–2, which was concentrated in the southern and southeastern Pale, and emigration through Ellis Island in the subsequent two decades, which largely came from the northern and northwestern parts of the Pale. To our knowledge, there is no systematic evidence on the direction of internal Jewish migration within the Pale after 1881 (and under the May Laws, issued in 1882), although several works hypothesize mobility from smaller to larger towns (e.g., Boustan 2007; Dubnow 2001 [1918]). To the extent that earlier pogroms drove such migration, the results in Table 8 suggest no confounding of the relationship between Jewish share and pogrom incidence during the 1905 Revolution.

[22]To define measures of industrial workers and (gross) output at the settlement level, we collect data at the establishment level on these measures from the Imperial Factory Census of 1903 (Ministry of Finance 1903) and hand-match the results to our settlements. See Gregg (2020) for additional information on that Census.

Table 8: Past Pogroms

	(1)	(2)	(3)	(4)
Share of Jews × Post–Oct 1905	−0.0079**	−0.0071*	−0.0086**	−0.0077**
	(0.0040)	(0.0038)	(0.0039)	(0.0038)
Past pogrom in current wave	−0.0763***	−0.0722***		
	(0.0166)	(0.0160)		
Past pogrom in current wave × Post–Oct 1905	−0.0195	−0.0173		
	(0.0142)	(0.0138)		
Pogrom in 1881–1882 × Post–Oct 1905			0.0103	0.0048
			(0.0124)	(0.0115)
Settlement fixed effects	✓	✓	✓	✓
Month fixed effects	✓	✓	✓	✓
Province fixed effects × Post–Oct 1905	✓	✓	✓	✓

***$p < 0.01$; **$p < 0.05$; *$p < 0.10$. All regressions in this table include temperature and precipitation deviations, along with interactions of post–Oct 1905 with indicators for settlement type, population variables, and these temperature and precipitation deviations. Columns 2 and 4 additionally include a spatial lag of the dependent variable and its interaction with post–Oct 1905. In each column, 1,370 settlements and 35,620 observations, with a mean outcome of 0.0055.

Table 9: Industrial Workers and Industrial Output

	(1)	(2)	(3)	(4)
Share of Jews × Post-Oct 1905	−0.0086**	−0.0084**	−0.0078**	−0.0075**
	(0.0040)	(0.0039)	(0.0038)	(0.0038)
Log industrial workers per capita	0.0031		−0.0117	
× Post–Oct 1905	(0.0258)		(0.0265)	
Log industrial output per capita		0.0012		0.0011
× Post–Oct 1905		(0.0008)		(0.0007)
Settlement fixed effects	✓	✓	✓	✓
Month fixed effects	✓	✓	✓	✓
Province fixed effects × Post–Oct 1905	✓	✓	✓	✓

***$p < 0.01$; **$p < 0.05$; *$p < 0.10$. All regressions in this table include temperature and precipitation deviations, along with interactions of post–Oct 1905 with indicators for settlement type, population variables, and these temperature and precipitation deviations. Columns 3 and 4 additionally include a spatial lag of the dependent variable and its interaction with post–Oct 1905. In each column, 1,370 settlements and 35,620 observations, with a mean outcome of 0.0055.

Mobilization during Russo–Japanese War: Table 10 accounts for the role of troop mobilization during the Russo-Japanese War, which, as discussed above, was coincident with the spread of conspiracy theories alleging Jewish collaboration with the Japanese. Anti-

Table 10: Mobilization during Russo-Japanese War

	(1)	(2)
Share of Jews × Post–Oct 1905	−0.0087**	−0.0078**
	(0.0039)	(0.0038)
Partial mobilization in uezd	0.0069**	0.0053***
	(0.0027)	(0.0019)
Settlement fixed effects	✓	✓
Month fixed effects	✓	✓
Province fixed effects × Post–Oct 1905	✓	✓

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.10$. All regressions in this table include temperature and precipitation deviations, along with interactions of post–Oct 1905 with indicators for settlement type, population variables, and these temperature and precipitation deviations. Additionally, Column 2 includes a spatial lag of the dependent variable and its interaction with post–Oct 1905. In each column, 1,370 settlements and 35,620 observations, with a mean outcome of 0.0055.

Jewish pogroms were indeed more likely during "partial" mobilizations, which were staggered across districts during the pre–Manifesto period, but there is no impact on the relationship between Jewish population share and pogrom incidence.[23]

Presence of Jewish Labour Bund: Table 11 allows for differential trends according to the presence of the General Jewish Labour Bund, which was often central in organizing self-defense against pogroms (e.g., Lambroza 1992). Figure A5 shows that the presence of this group was driven predominantly by proximity to Vilna (contemporary Vilnius, Lithuania), where the Bund was founded. Interestingly, we see no differential effect on pogrom incidence of Bund presence, however measured, though it is worth noting that Zionist-socialist parties, for which we do not have systematic data at the settlement or district level, were seemingly active in the southern Pale, where the Bund had less of a foothold (Goldstein 1985).[24]

Presence of police office and distance to troops: Finally, numerous historical accounts emphasize the active collaboration of local police in violence against Jews, with

[23]We collect these mobilization data from decrees published in the Complete Collection of Laws of the Russian Empire (*Polnoe sobranie zakonov Rossiiskoi Imperii* 1907–08). Our mobilization variable is an indicator that takes a value of one in any month in which a supplementary (beyond normal conscription) mobilization of reservists occurred in the surrounding district.

[24]We digitize data on the location of Bund branches from the document collection of Stepanskii, ed. (2010, pp. 391–392).

Table 11: Presence of Jewish Labour Bund

	(1)	(2)	(3)
Share of Jews × Post–Oct 1905	−0.0078**	−0.0082**	−0.0077**
	(0.0037)	(0.0037)	(0.0038)
Bund branch × Post–Oct 1905	0.0017		
	(0.0080)		
Log Bund members per 1,000 people × Post–Oct 1905		0.0025	
		(0.0027)	
Log distance to Bund branch × Post–Oct 1905			0.0000
			(0.0016)
Settlement fixed effects	✓	✓	✓
Month fixed effects	✓	✓	✓
Province fixed effects × Post–Oct 1905	✓	✓	✓

***$p < 0.01$; **$p < 0.05$; *$p < 0.10$. All regressions in this table include temperature and precipitation deviations, along with interactions of post–Oct 1905 with indicators for settlement type, population variables, and these temperature and precipitation deviations. Additionally, these models include a spatial lag of the dependent variable and its interaction with post–Oct 1905. In each column, 1,370 settlements and 35,620 observations, with a mean outcome of 0.0055.

Imperial troops and very occasionally the same police intervening to end a pogrom only after a customary three days of rioting (Surh 2024, p. 75).[25] In Table 12, we regress interactions of the post–October indicator and newly digitized data on the presence of a police "office" and distance to Imperial troops.[26] Consistent with the historiography, settlements with police offices see a greater increase in the incidence of pogroms after the Manifesto, while distance to Imperial troops has no effect. The estimated coefficient on Jewish population share retains its significance and magnitude after conditioning on these differential trends.

Other changes to specification and sample: Across all of these models, the estimated (differential) effect of Jewish population share is stable, negative, and precisely estimated. In the Appendix, we report results from various other exercises and robustness checks: including both Jewish population share and polarization in the same model (Ta-

[25]The LLM-assisted analysis we describe in the Appendix supports this reading, with nearly three-quarters of pogroms documented in Motzkin (1910) either incited or organized by local authorities.

[26]We collect data on police presence from provincial Memorial Books (*pamiatnye knizhki*) and Address Calendars (*adres-kalendari*, various years, 1887–1904), from which we generate a settlement-level indicator for presence of an office of three types of police supervisors: *stanovoi pristav*, *politseiskii nadziratel'*, and *politseimeister*. We further extract the quartering location of troops in 1903 from Bolotov (1903). We define distance as the great circle distance (kms) from a given settlement to the nearest settlement where troops were stationed.

Table 12: Presence of Police Office and Distance to Troops

	(1)	(2)
Share of Jews × Post–Oct 1905	−0.0097**	−0.0089**
	(0.0040)	(0.0039)
Police office × Post–Oct 1905	0.0055**	0.0056**
	(0.0028)	(0.0028)
Distance to troops × Post–Oct 1905	−0.0000	−0.0000
	(0.0001)	(0.0001)
Settlement fixed effects	✓	✓
Month fixed effects	✓	✓
Province fixed effects × Post–Oct 1905	✓	✓

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.10$. All regressions in this table include temperature and precipitation deviations, along with interactions of post–Oct 1905 with indicators for settlement type, population variables, and these temperature and precipitation deviations. Additionally, these models include a spatial lag of the dependent variable and its interaction with post–Oct 1905. In each column, 1,370 settlements and 35,620 observations, with a mean outcome of 0.0055.

ble A4); regressing pogrom incidence on a second-degree polynomial in Jewish share (Table A5—with a negative effect of Jewish share for all observed values of that variable, albeit with some loss of precision); collapsing the data to pre– and post–October periods (Table A6); operationalizing polarization (and fractionalization) with respect to Jewish share and the shares of multiple non-Jewish religious groups (Table A7); operationalizing "polarization" as share of Jews × share Russian Orthodox, as the tsar was not only head of state but also head of the Russian Orthodox Church (Table A8); imputing "Other" religion as Jewish share when that is less than 10 percent of the local population (Table A9); using rye and wheat prices as alternative proxies for weather shocks (Table A10); differentiating between villages with churches (*sela*), which may have been a locus for collective action, and other rural settlements (Table A11); extending the sample to April 1903, the date of the first pogrom in Kishinev (Figure A6); using alternative distance cutoffs for Conley standard errors (Table A12); and examining heterogeneity by settlement type (Table A13; in the fully saturated model, the negative treatment effect is concentrated in urban and rural settlements, as opposed to *mestechki*). Throughout, the basic picture holds: the sharp increase in pogroms following publication of the October Manifesto was smaller in Jewish settlements with relatively large Jewish communities, with no analogous effect of polarization.

5 Understanding Violent Backlash to Political Reform

Why was the spike in anti-Jewish pogroms in late 1905 concentrated in Jewish settlements in which there were comparatively few Jews? Theory offers some guidance, but only if one recognizes the ways in which established models of ethno-religious diversity and conflict are simultaneously misleading and helpful.

Existing models are misleading in that they implicitly consider environments in which *any* social group might be tempted to initiate conflict, depending on their relative position under the status quo and what they can expect to receive through violence. We seek to understand violent backlash to political reform, which is different. Following reform, it is only the group that is comparatively weakened—in our setting, conservative nationalists, Orthodox monarchists, and others supportive of the status quo—that is more likely than before to initiate conflict. The central question, then, is how this *asymmetric* response depends on the relative size of competing groups.

At the same time, established models are helpful in explicitly accounting for the possibility that groups targeted for violence can fight back. As discussed above, the memory of pogroms in the early 1880s and the shock of Kishinev and Gomel, vividly depicted in Hayim Nahman Bialik's 1904 poem "The City of Slaughter," encouraged the organization of Jewish self-defense. Among the pithy comments that accompany some of the pogroms documented in the 1906–7 edition of the American Jewish Year Book (see Figure A4), "Self-Defense" is mentioned in roughly one-quarter (25 of 91; most of the 254 listed pogroms include no general remarks), with frequent reference also to "resistance." At times, Jewish organization was capable of imposing substantial losses on the pogromists. In the city of Zhytomyr, for example, "Self-Defense [was] so active that [the] number of Christians killed exceed[ed] that of Jewish victims." Our textual analysis of the far more detailed reports compiled by the Zionist Organization of Berlin, summarized in the Appendix, documents similar discussion of organized Jewish defense in over half of all entries.

Unfortunately, we lack narrative accounts for Jewish settlements where pogroms did *not* occur; collecting such reports was not the remit of organizations documenting violence in the Pale. Nonetheless, various forms of evidence suggest that the anticipation of resistance was capable of deterring violence. Le Foll (2010, p. 163) makes this point explicitly with respect to the previous wave of pogroms in 1881–2, stating that the "missing pogroms" in Belorussia (the northeastern extent of the Pale) during this period were due in part to dense Jewish settlement, which "helped to expedite the formation and work of self-defense organizations."[27] Surh (2024), in turn, notes that self-defense in 1904–5 sometimes followed

[27]Le Foll (2010, p. 163) also observes that, in 1881–2, the "numerical superiority of Jews in

a first pogrom, which may help to explain the lower incidence of pogroms in settlements that had already experienced one (see Table 8). In addition, one can infer that self-defense may have helped to deter pogroms entirely by noting that it sometimes reduced their severity. The same detailed reports from the Zionist Organization of Berlin note, for example, that in Yekaterinoslav (contemporary Dnipro, Ukraine), self-defense "was not only taken into account by the murderers and looters, who were not at all inclined to serve as targets for the Jews' bullets, but also by the authorities... Through its mere existence, self-defense saved thousands of families from destruction and the city from complete devastation" (Motzkin 1910, Volume II, p. 190). Similarly, in Chernihiv, "the pogrom did not proceed as smoothly as its instigators had hoped," as "perpetrators were met with vigorous and highly successful resistance" from a self-defense organization that possessed some 100 revolvers and various other weapons (Motzkin 1910, Volume II, p. 269).

To explore these ideas in more detail, consider the model of conflict initiation and intensity of Esteban and Ray (2008), which builds on Esteban and Ray (1999). In this model, an arbitrary number of groups decide simultaneously and independently whether to initiate conflict. If any group does so, then members of each group decide on contributions of resources to the conflict—that is, they decide how much to fight. A group's probability of winning is equal to the group's share of total resources expended on conflict. Members of the winning group implement their preferred policy, which provides a differential payoff normalized to one.

We examine the special case of the model in which there are two groups, $g = A, B$, with population shares n_g. Esteban and Ray (2008) show that when the cost of resource contributions is quadratic, the probability that group g wins a conflict is equal to its population share n_g, and the expected payoff of any member of group g is equal to $\frac{n_g(1+n_g)}{2}$.

Without loss of generality, let $n \coloneqq n_A$, so that $n_B = 1-n$. Similarly, let γ_g be the weight placed on group g's preferred policy under the status quo, with $\gamma \equiv \gamma_A$, so that $\gamma_B = 1-\gamma$. Then *neither* group wants to initiate conflict if

$$\gamma \geq \frac{n\,(1+n)}{2} \text{ and } 1-\gamma \geq \frac{(1-n)\,(2-n)}{2},$$

or

$$\gamma \in \left[n - \frac{n(1-n)}{2}, n + \frac{n(1-n)}{2}\right]. \tag{2}$$

By inspection, the range of status quo policies such that neither group wants to initiate

Belorussian towns... made the pogroms more difficult to organize than in Ukrainian towns, where the Jewish population was less concentrated." Between 1882 and 1905, industrialization and Jewish emigration combined to reduce Jewish population shares in Belorussia.

conflict is increasing in polarization, $n\,(1-n)$.

Esteban and Ray conclude from this analysis that conflict *initiation* is less likely when polarization is high, given that conflict is more intense when societies are polarized.[28] Implicit in this conclusion is the assumption that the status quo policy γ is distributed idiosyncratically across polities and that the relevant variation is in polarization. Across localities within a given polity, however, there is likely to be a systematic component to the status quo. Political reform represents a shock to that systematic component. In this context, we ask: Given the shock of reform, how does the incidence of conflict—of violent backlash—depend on the relative size of competing groups?

To answer this question, consider the following elaboration of the model in Esteban and Ray (2008). Let $\gamma = \theta + \epsilon$, where θ is the systematic (e.g., national) and ϵ the stochastic (e.g., local) contribution to the share of benefits for group A. Assume that the distribution F of ϵ is differentiable and strictly increasing on some interval, with density f, and assume that the support of f is such that γ is bounded by 0 and 1. Then group A initiates conflict if

$$\theta + \epsilon < \frac{n\,(1+n)}{2},$$

which occurs with probability

$$F\left(\frac{n\,(1+n)}{2} - \theta\right).$$

Without loss of generality, let group A be the group disadvantaged by reform, such that θ_R (reform) is less than θ_{SQ} (status quo). Clearly, only group A is more likely to initiate conflict under reform than under the status quo. The following proposition shows how the differential probability that group A starts a conflict,

$$F\left(\frac{n\,(1+n)}{2} - \theta_R\right) - F\left(\frac{n\,(1+n)}{2} - \theta_{SQ}\right), \tag{3}$$

after versus before reform, depends on the relative population shares of groups A and B.

[28]Ideally, we would examine conflict intensity among the subset of settlements in our data that experienced pogroms. Unfortunately, neither Spitzer's (2021) data nor the underlying sources on which they are based provide information corresponding to "contributions" (i.e., effort) in the Esteban-Ray model. Among candidate variables, estimates of casualties and material damage arguably come closest, but these are poor approximations of the effort expended by Jews and non-Jews, respectively. In particular, Jewish casualties and damage to Jewish property could be high either because Jews and non-Jews engaged in open conflict, corresponding to high "contributions" by each side, or because Jews did not have the numbers to mount an effective resistance to murder and destruction (for pogromists, not merely an incidental outcome but the goal of the conflict).

Proposition 1. *Consider a reform (R) that generates a systematic shock to the status quo (SQ) policy, with $\theta_R < \theta_{SQ}$. Then the differential probability that the group* disadvantaged *by reform starts a conflict is decreasing in the population share of the group* advantaged *by reform if and only if*

$$f\left(\frac{n\,(1+n)}{2} - \theta_R\right) > f\left(\frac{n\,(1+n)}{2} - \theta_{SQ}\right). \tag{4}$$

Proof. The statement follows from differentiation of Expression 3 with respect to n and recognition that the population share of the disadvantaged group is $1 - n$. □

Intuitively, the group left worse off by reform is more likely to initiate conflict after reform than before—but, under Condition 4, this change is smaller when the group that benefits from reform is numerous. To understand the empirical implications of this result, note that Condition 4 holds (weakly) if the group disadvantaged by reform does not initiate conflict under the status quo, which (modeling polities as a continuum) implies $f\left(\frac{n(1+n)}{2} - \theta_{SQ}\right) = 0$.

Our empirical context broadly reflects the assumptions and conclusions of this analysis. There were comparatively few pogroms prior to the October Manifesto—the vast majority of settlements saw no conflict before November 1905. The Tsar's accommodation to revolutionary activity was to issue the Manifesto, which systematically disadvantaged conservative nationalists, Orthodox monarchists, and others threatened by increased civil and political rights, especially for Jews. The violent backlash that followed was more pronounced in settlements where Jews constituted a relatively small part of the local population.

Jews often organized to defend themselves after the reemergence of pogroms in 1903 and 1904. Having organized, Jews frequently fought back where pogroms occurred. That much is clear from the historical record. As mentioned, neither the Zionist Organization of Berlin nor the authors of the *American Jewish Year Book*, from whose records the pogrom data that we use are assembled, collected information on communities where pogroms did *not* occur, for this was not their task. What we can say is the following: Pogroms were systematically less likely in settlements with relatively large Jewish populations. It is natural to expect that, in such settlements, Jews would have been able to mount a stiffer defense against pogromists. In turn, where such a defense was anticipated, it is reasonable to anticipate that potential pogromists would have been more deterred from violence. These are the implications of our empirical analysis and our elaboration of Esteban and Ray's (2008) model. As noted above, Le Foll (2010) makes precisely the same argument with respect to the "missing pogroms" in Belorussia during the last major wave of anti-Semitic violence, in 1881–2.

Considering other candidate theories, the nonmonotone relationship implied by a naive application of Esteban and Ray's (2008) model to political reform finds no empirical sup-

port, as we have shown. The various arguments suggesting *greater* conflict when groups are polarized are also inconsistent with the patterns that we document. Allport's (1954) contact hypothesis raises a similar problem: if contact is most frequent when groups are of similar size, then the relationship between pogrom incidence and Jewish population should be nonmonotone—again, counter to what we find. Reaching beyond conflict studies, the relative numbers of Jews and non-Jews could affect the ability of either group to coordinate. However, Olson's (1965) theory of collective action suggests that such coordination should have been easiest for Jews, and hardest for non-Jews, where Jews were a minority. Theory, empirics, and contemporaneous accounts thus converge on a simple story: violent backlash to the October Manifesto was most pronounced where Jews were outnumbered and least able to defend themselves.

A few additional notes are in order. First, taken at face value, the rational calculus implied by this discussion sits uncomfortably with the savage behavior of the pogromists. Yet reason, emotion, and prejudice all play a role in ethnic conflict. Surh (2024, p. 128), for example, writes of the "interaction and communication between 'higher' and 'lower' forms of pogrom participation"—of religious leaders and officers of the state, on the one hand, and a drunk and rapacious crowd, on the other. The point of our theoretical analysis is simply to emphasize that Jews, when sufficiently numerous, were able to fight back against pogroms, and that this may have discouraged some of the more "rational" members of the non-Jewish community from speech and actions that could incite violence.

Second, this same theoretical framework arguably underpredicts conflict, in that there are settlements in our sample with Jewish population share close to one (see Figure 3), yet we do not observe "pogroms" against non-Jews, even prior to the October Manifesto. In practice, it would have been exceedingly costly for Jews to initiate mob violence, even if they were so inclined. In addition, Jews as well as non-Jews would have experienced costs of conflict above and beyond the expenditure of resources assumed by the Esteban-Ray framework—physical casualties, loss of income, and more. Any "symmetric" cost of conflict widens the "peace interval" in Expression 2, while any "asymmetric" cost of initiating violence further moves one of the endpoints. Such an accommodation to verisimilitude leaves intact the argument in Proposition 1. Only the group disadvantaged by reform is more likely to initiate conflict after than before reform, with the differential probability of conflict increasing in the population share of the disadvantaged group under a condition that mirrors Condition 4.

Third, Esteban and Ray's "behavioral framework" (1999, p. 384), which we adopt above, assumes that the impact of individual contributions (towards conflict) on the welfare of other group members is fully internalized. In the Appendix, we relax this assumption. When free riding is allowed, and the status quo is distributed idiosyncratically across polities, then,

contra Esteban and Ray (2008), conflict initiation is more, not less, likely when polarization is high. At the same time, there is a straightforward analog to Proposition 1, and the implication here survives, if political reform systematically alters the distribution of benefits across groups. Our rationalization of the empirical patterns we observe is thus robust to assumptions about the degree to which local institutions in the Pale discouraged free riding.

Finally, the same theoretical framework can accommodate other determinants of conflict, such as those investigated in various robustness checks above. As Esteban and Ray (2008, p. 168) note, n can be interpreted broadly as "capturing all the factors that may influence the effectiveness of a given effort," including but not only population share. To see this, let $\tilde{n} : \mathbb{R} \times \mathbb{R}^k \to \mathbb{R}$ be a function defined by $(n, \Lambda) \mapsto \tilde{n}(n, \Lambda)$, where n is population share and Λ is a k-dimensional vector capturing other factors that determine the effectiveness of effort. Assume that $\tilde{n}$ is increasing in n, and order each component Λ_i of Λ such that $\tilde{n}$ is increasing in Λ_i. Then there is an analog to Proposition 1 such that the differential probability of conflict is increasing in n and each component of Λ if and only if

$$f\left(\frac{\tilde{n}(1+\tilde{n})}{2} - \theta_R\right) > f\left(\frac{\tilde{n}(1+\tilde{n})}{2} - \theta_{SQ}\right).$$

In our context, for example, if $\Lambda_i = -$(travel time to nearest railroad), then the differential probability that non-Jews initiate a pogrom is increasing in proximity to the nearest railroad, as suggested by various historical accounts and for which we find empirical support in Table 5.[29]

6 Conclusion

The idea that ethno-religious diversity is related to conflict has a long lineage, even if the particular relationship depends on how diversity is operationalized and the outcome in question. Our contribution is to examine an outcome not emphasized in the literature on diversity and conflict: How does the likelihood of violent backlash to political reform depend on the ethno-religious makeup of the communities affected by reform? In our study of anti-Jewish pogroms during the 1905 Russian revolution, we find robust evidence that backlash was less likely in settlements with relatively large Jewish populations. We argue that the key to understanding this pattern, and violent backlash more generally, is to recognize reform as a systematic shock to the status quo—one that the group disadvantaged by reform is less likely to contest when the other group is comparatively large, and thus has strength in

[29]With additional structure on $\tilde{n}$, we could say more about how the relationship between population share and violent backlash to reform depends on the components of Λ—that is, we could say something about triple interactions—but the historiography does not generally suggest strong priors in this regard.

numbers. Motivated by the re-emergence of pogroms in 1903 and 1904, as memorialized by the poem we cite in our epigraph, Jews across the Pale of Settlement organized self-defense in anticipation of violence. When that violence came, following publication of the October Manifesto across the Russian Empire, settlements with relatively large Jewish communities were seemingly better prepared, which helped deter attacks by conservative nationalists, Orthodox monarchists, and others who blamed Jews for reform.

Empirical study of a particular case raises natural questions about external validity. It would clearly be helpful to replicate our results in other settings. At the same time, one should be conscious of features of our environment that are not present everywhere. Although the Pale of Settlement was noted for its ethno-religious diversity, there was one divide that especially mattered: between Jews and non-Jews. Not every setting is characterized by this dichotomy—though the examples we cite in the introduction generally are. Jews, moreover, could not depend on protection by the state, such as might exist elsewhere. Indeed, as we show, the spike in pogroms following publication of the October Manifesto was greater in settlements with police "offices," reflecting the active participation of the police in some attacks.

It is also worth emphasizing that we focus on local events in a larger political context. Empirically, we do not take a stance on the "success" of the reforms associated with the Manifesto: the creation of national elections for a new parliamentary body (the Duma), the ceding of some Imperial powers to this body and other parts of government, and the formal expansion of civil rights. Many of these advances were subsequently counteracted, ignored, or ineffectively enforced (e.g., Ascher 2004; Steinberg 2017). For our purposes, it was the perception of change from the status quo at the time of the Manifesto that mattered, but the fact of pogroms and of Jewish resistance played a role in what followed (Surh 2024). The Fundamental Laws of April 1906, which reaffirmed supreme autocratic power, and the tsar's dissolution of the first Imperial Russian Duma three months later, represented victories for "reactionary nationalism" (Boix and Kofanov 2025). It was during this period that the wave of pogroms came to an end.

More broadly, our results speak to debates about modernization in diverse societies. The goal of the October Manifesto was to short-circuit revolutionary activity by granting rights to subjects of the Tsar. An incidental consequence of this decision was to level the playing field between historically advantaged and disadvantaged groups. For supporters of the monarchy, the sight of Jews with placards denouncing the Tsar was a symbol of power lost. Reform, we have stressed, has losers as well as winners. When the line between the two coincides with ethno-religious boundaries, violence can result. In Imperial Russia, Jews who lived in settlements with few other Jews were the most vulnerable of the historically disadvantaged.

They were the victims of violent backlash to political reform.

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Violent Backlash to Political Reform: Online Appendix

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Conflict initiation and intensity with free riding

Esteban and Ray (2008, p. 384) write: "Our behavioral framework ignores free-rider problems within each group... [T]here is little we wish to add to this problem and so we assume that external effects within a group are fully internalized by group members." Here, we ask whether their conclusions, and ours, change when free riding is allowed.[1]

Consider the special case of Esteban and Ray (2008) in which there are two groups, $g = A, B$, with population shares n_g, the differential payoff from winning is normalized to one, and the cost of resource contributions is quadratic. For any individual i in group g, the expected payoff is

$$\frac{r_{ig} + \sum_{j \in g \setminus i} r_{jg}}{R} - \frac{(r_{ig})^2}{2}. \tag{A1}$$

The first term is the probability that group g wins, assumed equal to the share of total contributions R across both groups made by individuals in g, where R is also a function of r_{ig}. The second term is the cost of resource contributions for individual i in group g.

By inspection, Expression A1 is strictly concave in r_{ig}. The first-order condition is

$$\frac{1}{R} - \frac{r_{ig} + \sum_{k \in i \setminus g} r_{jg}}{R^2} = r_{ig}.$$

Imposing symmetry for members of A and B, respectively, gives

$$\frac{1}{R} - \frac{n_g r_g}{R^2} = r_g,$$

where r_g is the common contribution for members of group g. Multiplying through by $\frac{n_g}{R}$, and using $p_g = \frac{n_g r_g}{R}$, we have

$$\frac{n_g}{R^2}(1 - p_g) = p_g. \tag{A2}$$

With two groups, where $n \equiv n_A$ and $p \equiv p_A$, we can rewrite Equation A2 as

$$\frac{n(1-p)}{p} = \frac{(1-n)p}{1-p} = R^2. \tag{A3}$$

Rearranging gives the probability that group A wins as a function of its population share n:

$$p = \frac{\sqrt{n}}{\sqrt{n} + \sqrt{1-n}}. \tag{A4}$$

As in Esteban and Ray (2008), where $p = n$, a group's probability of winning a conflict is

[1] We assume strategic but identical individuals, such that there is no meaningful collective-choice problem when deciding whether to initiate conflict.

increasing in its population share. Here, however, the relationship is concave for $p < \frac{1}{2}$ and convex for $p > \frac{1}{2}$.

From Equations A3 and A4, we can derive

$$R^2 = \sqrt{n\,(1-n)},$$

so that, using $p^2 = \left(\frac{nr_A}{R}\right)^2$ and $(1-p)^2 = \left[\frac{(1-n)r_B}{R}\right]^2$,

$$r_A^2 = \frac{\sqrt{1-n}}{\sqrt{n} + 2n\sqrt{1-n}} \quad \text{and } r_B^2 = \frac{\sqrt{n}}{\sqrt{1-n} + 2\sqrt{n}\,(1-n)}.$$

The expected utility for any individual in group A is therefore

$$p - \frac{r_A^2}{2} = \frac{\sqrt{n}}{\sqrt{n} + \sqrt{1-n}} - \frac{1}{2} \cdot \frac{\sqrt{1-n}}{\sqrt{n} + 2n\sqrt{1-n}}, \tag{A5}$$

which is increasing in n for all $n \in (0, 1)$. Similarly, the expected utility for any individual in group B is

$$(1-p) - \frac{r_B^2}{2} = \frac{\sqrt{1-n}}{\sqrt{n} + \sqrt{1-n}} - \frac{1}{2} \cdot \frac{\sqrt{n}}{\sqrt{1-n} + 2\sqrt{n}\,(1-n)}, \tag{A6}$$

Expression A5 is negative for all $n < n'$, where $n' \approx 0.3$, whereas Expression A6 is negative for all $n > n''$, where $n'' \approx 0.7$. Moreover, for $n \in [n', n'']$, the range of status quo policies such that neither group wants to initiate conflict,

$$\gamma \in \left(\frac{\sqrt{n}}{\sqrt{n} + \sqrt{1-n}} - \frac{1}{2} \cdot \frac{\sqrt{1-n}}{\sqrt{n} + 2n\sqrt{1-n}}, \frac{\sqrt{n}}{\sqrt{n} + \sqrt{1-n}} + \frac{1}{2} \cdot \frac{\sqrt{n}}{\sqrt{1-n} + 2\sqrt{n}\,(1-n)} \right)$$

is *smallest* at $n = 0.5$, that is, at maximal polarization. In other words, if we relax the assumption of fully internalized within-group benefits but continue to assume (as implied by Esteban and Ray 2008) that the status quo γ is distributed idiosyncratically across polities, then conflict initiation is less likely when polarization is low, not high.

Nonetheless, the qualitative effect of a *systematic* shock to the status quo is the same as in the main manuscript. To see this, assume, as in the extension to Esteban and Ray (2008) discussed in the main text, that $\gamma = \theta + \epsilon$, where θ is the systematic and ϵ the idiosyncratic contribution to the share of benefits for group A. Using Equation A5, the probability that

group A initiates conflict is

$$F\left(\frac{\sqrt{n}}{\sqrt{n}+\sqrt{1-n}}-\frac{1}{2}\cdot\frac{\sqrt{1-n}}{\sqrt{n}+2n\sqrt{1-n}}-\theta\right).$$

The following proposition is a straightforward analog to Proposition 1.

Proposition A1. *Consider a reform (R) that generates a systematic shock to the status quo (SQ) policy, with $\theta_R < \theta_{SQ}$. Then the differential probability that the group disadvantaged by reform starts a conflict is increasing in its population share n if and only if*

$$f\left(\frac{\sqrt{n}}{\sqrt{n}+\sqrt{1-n}}-\frac{1}{2}\cdot\frac{\sqrt{1-n}}{\sqrt{n}+2n\sqrt{1-n}}-\theta_R\right) >$$
$$f\left(\frac{\sqrt{n}}{\sqrt{n}+\sqrt{1-n}}-\frac{1}{2}\cdot\frac{\sqrt{1-n}}{\sqrt{n}+2n\sqrt{1-n}}-\theta_{SQ}\right).$$

As with Proposition 1, this proposition implies that the differential probability of conflict is (weakly) increasing in the population share n of the group disadvantaged by reform if there is no conflict under the status quo, given that the expression $\frac{\sqrt{n}}{\sqrt{n}+\sqrt{1-n}}-\frac{1}{2}\cdot\frac{\sqrt{1-n}}{\sqrt{n}+2n\sqrt{1-n}}$ is increasing in n.

Qualitative evidence

To help contextualize our theoretical model and structure the empirical analysis, we explored many sources of qualitative evidence on the wave of anti-Semitic violence in Imperial Russia from 1903 to 1906. Our initial goal was to develop a more detailed picture of the causes and perpetrators of the pogroms: Were the pogroms driven by political grievances and backlash to reform (as understood and/or explicitly announced in the Manifesto), or were other explanations paramount? And who perpetrated the violence: local residents or individuals outside the locality? We subsequently also examined 1) pre-pogrom relations between Jewish and non-Jewish residents, 2) the role of authorities before or concurrent with the outbreak of violence, 3) whether major violence was deterred, 4) if Jewish "self-defense" was present, and 5) how the pogrom ended.

To develop this understanding, we directly examined a large number of primary and secondary sources, some of which are cited in the main manuscript. In addition, we used a large language model to query the narrative descriptions (in German) of 93 individual pogroms compiled by the Zionist Organization of Berlin (Motzkin 1910). As discussed, this collection of documents is a key source for Spitzer (2021), who geolocates and otherwise characterizes pogroms during this period, and whose data we use in our analysis.[2] We subsequently checked the results for key features against a more traditional hand-coding, by a German-speaking research assistant, of 21 randomly sampled narratives from the same corpus.[3]

Method

In particular, for the LLM analysis, we used the OpenAI model `GPT-5.6-terra` to generate one-paragraph summaries (in English) of the original German-language entries; to construct a simple categorization of pre-pogrom relations; to characterize the causes of the pogrom, the role of authority figures, and the identity of perpetrators; to highlight mentions of Jewish self-defense or other efforts to deter the pogrom; and to describe what brought the pogrom to an end. We additionally prompted the model to describe its reasoning and provide any notes on ambiguity. To accomplish this, we sequentially processed the original texts through the OpenAI API with the following prompt:

```
You are helping code historical narratives about Jewish pogroms in the Russian
Empire during the First Russian Revolution.

You will receive a German-language narrative for one locality/event.  Code only
```

[2] Figure A3 depicts the beginning of one such narrative.

[3] The hand-coding applies to causes and the identity of perpetrators only.

what is supported by this specific narrative. Do not use outside historical knowledge. If the narrative does not state something clearly, use unknown/false and explain briefly.

Some narratives describe more than one locality/event. In those cases:

- return one overall structured record for the complete narrative; do not split it into location-level records and do not select labels by majority across locations.
- in locations_coded, list every location whose pogrom or attempted pogrom is substantively described; exclude locations mentioned only in passing, for comparison, or in an undetailed list.
- combine the supported evidence across the coded locations. Use mixed_or_ambiguous, both, or multiple_apply when the relevant evidence differs across locations, and explain the variation briefly.

Pre-pogrom relations between Jewish and non-Jewish residents:

- good or cooperative: the narrative explicitly describes friendly, peaceful, cooperative, protective, solidaristic, or unusually good relations between Jews and other local residents before the pogrom.
- ordinary coexistence: the narrative suggests routine coexistence or normal everyday relations, without explicit prior hostility or unusual cooperation.
- tense or conflictual: the narrative describes prior hostility, agitation, threats, disputes, ethnic/religious resentment, economic conflict, boycotts, accusations, or previous anti-Jewish incidents.
- mixed or ambiguous: the narrative gives conflicting or unclear evidence about pre-pogrom relations.
- unknown: the narrative does not describe relations between Jews and other locals before the pogrom.

Causes of the pogrom:

- political cause: anti-revolutionary mobilization, accusations of revolutionary activity, election/party/police/state-political conflict, nationalist or regime-loyalist mobilization, rumors about Jews as revolutionaries or anarchists, or other explicitly political triggers.
- material cause: debt, economic competition, wages, property disputes, commercial resentment, or other economic/material grievances described as causes.
- both: both political and material causes are clearly present as causes, not merely consequences.

- other: a stated cause exists but is neither political nor material under these definitions.
- unknown: no cause is stated clearly enough.

Perpetrators of the pogrom:

- locals: described as residents/townspeople/peasants/workers from the locality or nearest areas, or locally stationed soldiers/Cossacks/police.
- outsiders: described as arriving from elsewhere, peasants travelling to arrive, strangers, soldiers/Cossacks/police not locally resident, or organized external groups.
- both: both local and outsider perpetrators are clearly described.
- unknown: perpetrators are not identified clearly enough.

Authorities' role before or at the outbreak (this field concerns what authorities did before the pogrom or at the moment it was developing/outbreaking, not how they acted after violence was already underway):

- incited or organized: authorities encouraged, organized, ordered, prepared, or helped mobilize the pogrom. This includes officials, police, soldiers, administrators, or other state-linked actors spreading rumors, giving permission, directing crowds, or coordinating attackers.
- tolerated or enabled: authorities did not necessarily incite the pogrom, but allowed it to develop despite warning signs. This includes ignoring threats, refusing protection, withdrawing police, delaying intervention, disarming Jewish defenders, permitting outside rioters to arrive, or otherwise enabling the attack by inaction.
- opposed or preventive: authorities acted before or at outbreak to prevent the pogrom, including arrests of agitators, dispersing crowds, warning/protecting Jews, deploying troops preventively, or blocking rioters before large-scale violence began.
- mixed or ambiguous: different authorities behaved differently, or the narrative gives conflicting/unclear evidence.
- unknown: the narrative does not describe what authorities did while the pogrom was brewing.
- Do not code later suppression as "opposed_or_preventive" unless the narrative says authorities acted before or at the outbreak to prevent escalation.
- Do not code mere absence of police as ‘‘tolerated_or_enabled’’ unless the narrative suggests authorities knew, should have known, withdrew, refused help, or otherwise allowed the threat to develop.

```
Deterrence means an attack/pogrom was prevented, stopped before major violence,
aborted, or substantially limited because of the Jewish self-defense or an
intervention by authorities, military, police, local notables, clergy, etc.

Jewish self-defense means armed organized Jewish protection, patrols, barricades,
fighting back, warning networks, or similar action by Jews.

How the pogrom ended:

   - military or police intervention:  the pogrom was stopped or substantially
     ended by soldiers, army units, Cossacks, troops, police or other armed state
     forces.

   - self defense or jewish defense:  the pogromists were stopped, driven off,
     deterred, or blocked by Jewish self-defense, armed Jewish residents, guards,
     organized defense groups, or similar non-Jewish local defensive action.

   - rioters finished plunder:  the narrative indicates that the pogrom ended
     because the rioters finished looting, found nothing more to take, exhausted
     available targets, dispersed after plunder, or stopped after completing
     destruction.

   - authorities pressure:  the pogrom was stopped by the command or pressure from
     the government authorities or officials without the coercive intervention.

   - other:  the ending was due to a different reason.

   - multiple apply:  a combination of the above factors.

   - unknown:  means the ending was unclear, not stated, or cannot be confidently
     classified.

   - If both military intervention and self-defense appear, choose the one
     described as actually ending the pogrom.  If the narrative does not make this
     clear, use "multiple_apply" and explain in the evidence/uncertainty fields.

Return the structured record requested by the schema.  Keep text fields concise.
Include short evidence snippets when possible.
```

The structured record to which the prompt refers codes the narratives along several dimensions—pre-pogrom Jewish-local relations; primary cause; origin of perpetrators; the role of authorities; whether a pogrom was deterred, prevented, or aborted; whether Jewish self-defense occurred; and how the pogrom ended. Each record is paired with a brief evidentiary quote/paraphrase, and in most cases a rationale, plus an overall confidence rating and notes on ambiguity.

Results

We ran this protocol three times for the 91 incidents in the regression study period and averaged the resulting distributions across runs.[4] For causes and perpetrator identity, averages across runs are as follows:

Primary cause	**Share of events**
Political	0.62
Both political and material	0.25
Material	0.04
Other or Unknown	0.09

Identity of perpetrators	**Share of events**
Locals	0.32
Both locals and outsiders	0.65
Outsiders	0.01
Unknown	0.02

Direct reading of the original texts, alongside the summaries and descriptions generated by the GPT model, provides additional context. Pogroms mostly start with politically motivated violence by locals (often workers), in rare cases incited or fueled by instigators from the nearest large city. The direct trigger is typically a demonstration in support of the October Manifesto, which results in counterrevolutionary violence. The modal pogrom then turns into a looting event, often with peasants from nearby villages joining in.[5]

We also look at pre-pogrom relations between Jewish and non-Jewish residents of the locality. As with the coding of causes above, we are unable to say anything about the localities where pogroms did *not* occur; nonetheless, the descriptions in the narratives can shed light on local conditions before the outbreak of violence. In Jewish settlements that experienced pogroms, the most common state of affairs before violence occurred was "tense

[4]To compare the distributions from hand-coding and `GPT-5.6-terra`, we use χ^2 and Fisher's exact tests. Although there is some disagreement across runs for individual texts, the resulting frequency distributions are very similar: p-values for the χ^2 test are between 0.78 and 0.99 for individual features, and all are equal to 0.99 after adjusting for multiple comparisons. Comparing the three hand-coded distributions of causes and perpetrators to that from the GPT coding (due to the small sample size, we report results from Fisher's test only) similarly returns unadjusted p-values for the pairwise tests—one hand-coded vs. three GPT-coded—of between 0.48 and 0.75 (Holm-adjusted values all are 1.0). Examining individual items, hand coding and GPT coding produce identical classification of causes between 62% and 71% of the time, depending on the run, whereas for perpetrators the match rate is 91% in each run.

[5]There is a single case of "hooligans" arriving by train (to the village of Birsula) to start the pogrom themselves.

or conflictual relations," defined as an existing history of conflict or animosity. In contrast, explicitly good or cooperative pre-pogrom relations, or ordinary coexistence without an explicit history of either prior hostility or solidarity, together account for just over 20% of cases.

Pre-pogrom relations	**Share of events**
Tense or conflictual	0.41
Mixed or ambiguous	0.21
Ordinary coexistence	0.03
Good or cooperative	0.19
Unknown	0.16

Most Jewish communities organized some form of self-defense. Analysis of the original texts, summaries, and descriptions, however, reveals enormous variation in the scope of this activity, from preemptive measures and (partly) successful armed deterrence to less coordinated and (fully) unsuccessful efforts.

Self-defense mentioned	**Share of events**
Yes	0.57
No	0.43

This reading is supported by the related coding of "deterrence" (expanded beyond Jewish self-defense to include other actors attempting to prevent or limit violence), where nearly three-quarters of Jewish settlements that *experienced a pogrom* experienced some form of deterrence:

Deterrence mentioned	**Share of events**
Yes	0.73
No	0.27

To investigate one potential source of this deterrence, we examine the role of local authorities before the outbreak of violence. As described in the narratives, in the vast majority of cases, authorities either incited or outright organized the pogrom. In only a fifth of all pogroms were authorities divided or did they try to oppose violence:

Role of local authority	**Share of events**
Incited or organized	0.72
Tolerated or enables	0.07
Mixed or ambiguous	0.18
Opposed or preventive	0.02
Unknown	0.01

Finally, we ask how pogroms ended. The narratives often do not provide much detail (a quarter of events provide no information at all), but we are able to identify a few common

ways the violence stopped. In 27% of cases, a pogrom was ended by coercive intervention by armed state actors—mostly the military, but in one isolated instance, the police. A related but different mechanism is pressure or direct command to stop the pogrom from government officials, whether local or higher up (e.g., provincial governor). Together, these two mechanisms cover 40% of all pogroms discussed in the narratives. Less common mechanisms include Jewish self-defense or protection from other non-state actors (e.g., German colonists), or simply an exhaustion of opportunities to plunder.

Ending of pogrom	**Share of events**
Military or police intervention	0.27
Authority pressure	0.13
Self-defense or other defense	0.08
Rioters finished plunder	0.08
Multiple apply	0.11
Other	0.06
Unknown	0.26

Additional figures

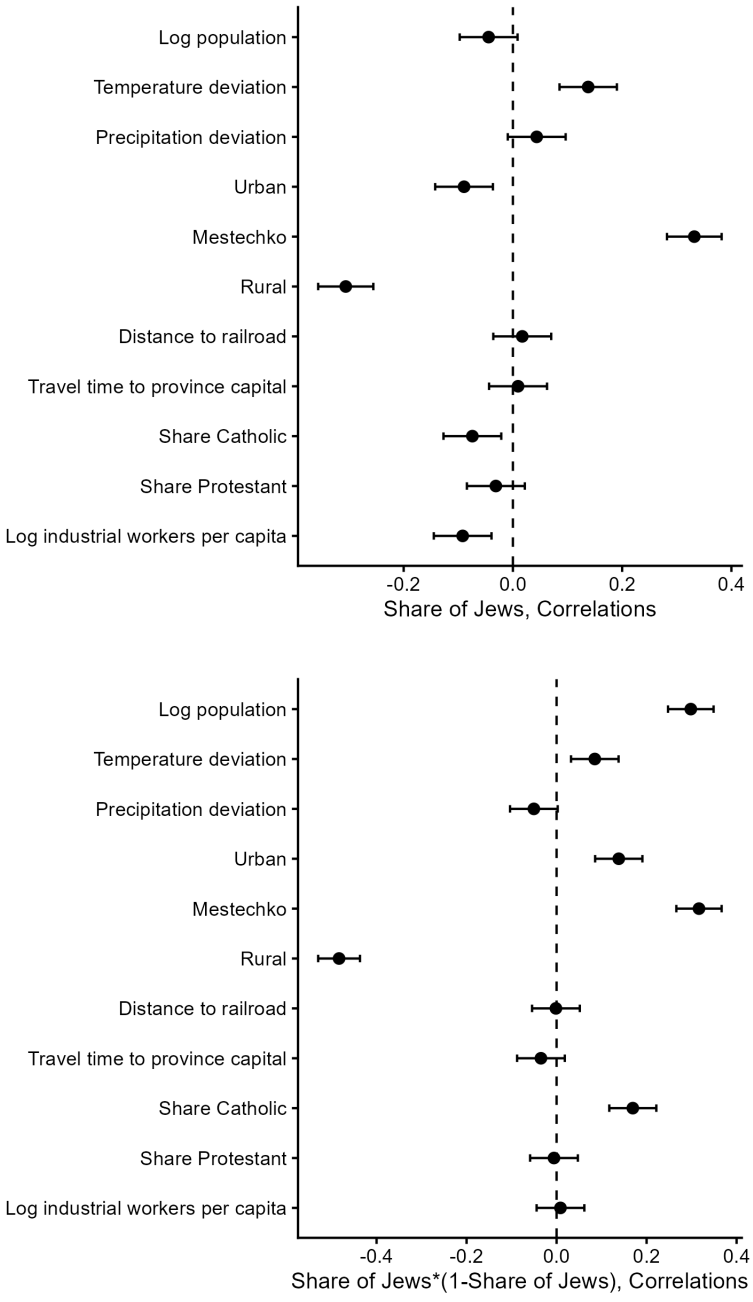


Figure A1: Coefficients from pairwise regressions of Jewish share and polarization, respectively, on various standardized variables.

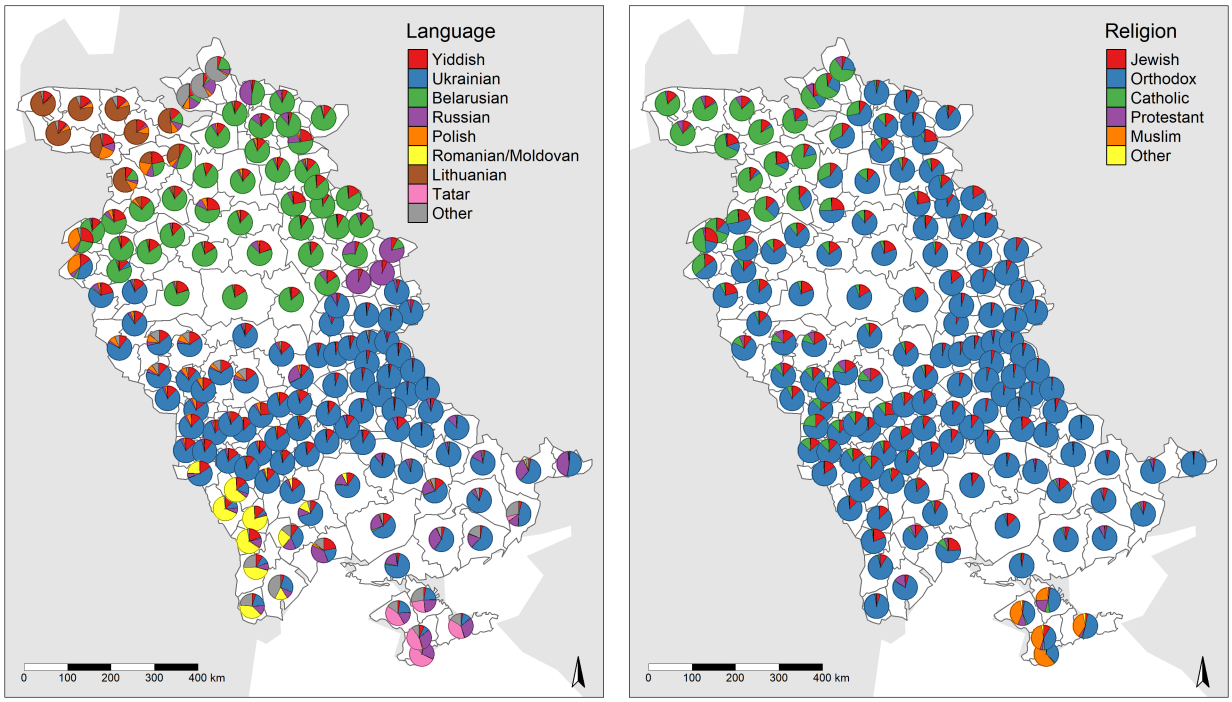


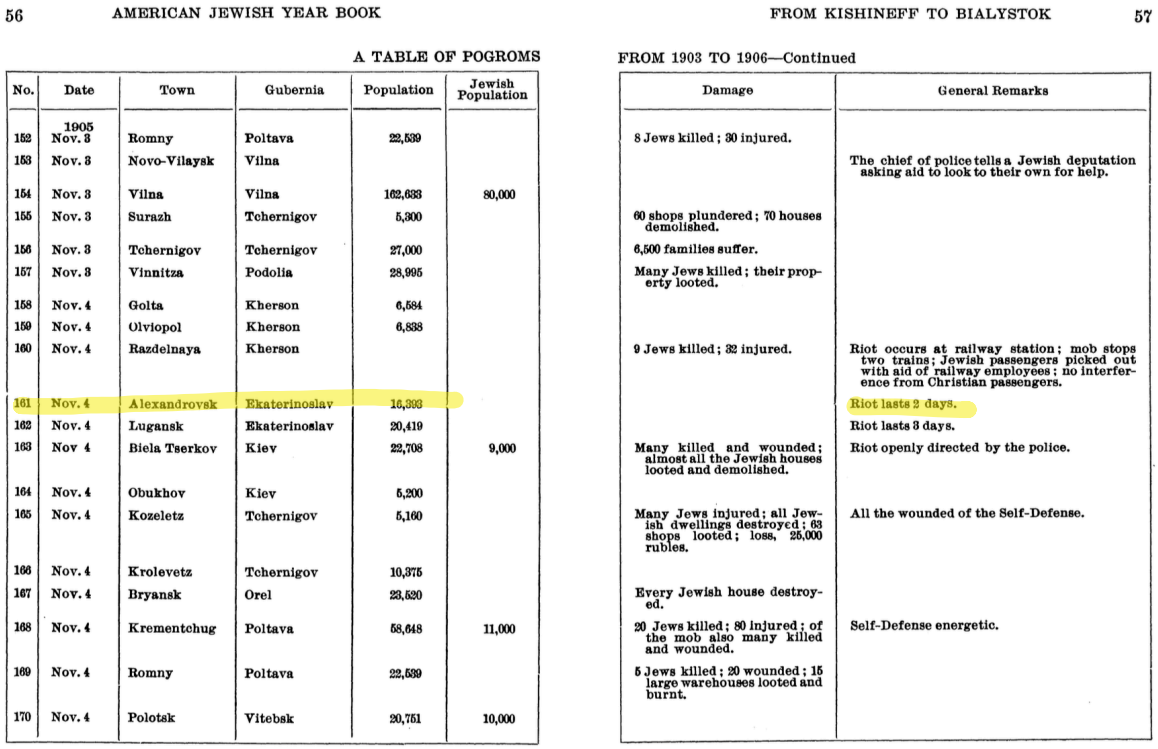
Figure A2: District-level population composition in the Pale of Settlement for ethnolinguistic (left) and religious (right) groups in 1897. "Orthodox" includes Old Believers.

Alexandrowsk

Gesamtbevölkerung (1897) 18 849, Juden 5267.

Der Pogrom von Alexandrowsk wurde von den dortigen reaktionären Organisationen, dem sogenannten Schwarzen Hundert, angestiftet und zum Teil durchgeführt. Zwar existierten diese Organisationen vor dem Pogrom nur de facto, ihre formale und legale Existenz begann erst später. Doch gehen wir kaum fehl, wenn wir annehmen, dass all die Elemente, aus denen diese reaktionären Gruppen bestehen, bereits vor dem Pogrom eine lose Organisation besessen haben. Darauf weist ihr planmässiges koordiniertes Handeln vor dem Ausbruch und während des Pogroms und die Fülle der stets denselben Charakter tragenden antisemitischen und reaktionären Flugblätter hin, die teils vor und meist nach dem Pogrom erschienen sind. Diese Flugblätter sind bald vom „Verband echt russischer Leute", bald vom „Monarchistischen Verband des 17. Oktober", bald vom „Russischen Volksbund" unterzeichnet. Der Ursprung ist aber wahrscheinlich stets der-

Figure A3: The beginning of a typical entry in Motzkin (1910), the primary source for the pogrom data in Spitzer (2021), and an example of the inputs in our LLM exercise.



56 AMERICAN JEWISH YEAR BOOK

A TABLE OF POGROMS

No.	Date	Town	Gubernia	Population	Jewish Population
	1905				
152	Nov. 3	Romny	Poltava	22,539	
153	Nov. 3	Novo-Vilaysk	Vilna		
154	Nov. 3	Vilna	Vilna	162,633	80,000
155	Nov. 3	Surazh	Tchernigov	5,300	
156	Nov. 3	Tchernigov	Tchernigov	27,000	
157	Nov. 3	Vinnitza	Podolia	28,995	
158	Nov. 4	Golta	Kherson	6,584	
159	Nov. 4	Olviopol	Kherson	6,838	
160	Nov. 4	Razdelnaya	Kherson		
161	Nov. 4	Alexandrovsk	Ekaterinoslav	16,393	
162	Nov. 4	Lugansk	Ekaterinoslav	20,419	
163	Nov 4	Biela Tserkov	Kiev	22,708	9,000
164	Nov. 4	Obukhov	Kiev	5,200	
165	Nov. 4	Kozeletz	Tchernigov	5,160	
166	Nov. 4	Krolevetz	Tchernigov	10,375	
167	Nov. 4	Bryansk	Orel	23,520	
168	Nov. 4	Krementchug	Poltava	58,648	11,000
169	Nov. 4	Romny	Poltava	22,539	
170	Nov. 4	Polotsk	Vitebsk	20,751	10,000

FROM KISHINEFF TO BIALYSTOK 57

FROM 1903 TO 1906—Continued

Damage	General Remarks
8 Jews killed; 30 injured.	
	The chief of police tells a Jewish deputation asking aid to look to their own for help.
60 shops plundered; 70 houses demolished.	
6,500 families suffer.	
Many Jews killed; their property looted.	
9 Jews killed; 32 injured.	Riot occurs at railway station; mob stops two trains; Jewish passengers picked out with aid of railway employees; no interference from Christian passengers.
	Riot lasts 2 days.
	Riot lasts 3 days.
Many killed and wounded; almost all the Jewish houses looted and demolished.	Riot openly directed by the police.
Many Jews injured; all Jewish dwellings destroyed; 63 shops looted; loss, 25,000 rubles.	All the wounded of the Self-Defense.
Every Jewish house destroyed.	
20 Jews killed; 80 injured; of the mob also many killed and wounded.	Self-Defense energetic.
5 Jews killed; 20 wounded; 15 large warehouses looted and burnt.	

Figure A4: Two typical pages in "From Kishineff to Bialystok" (*American Jewish Year Book*, 1906–07), the secondary source for the pogrom data in Spitzer (2021). The highlighted case is the pogrom narrated in the entry of Figure A3.

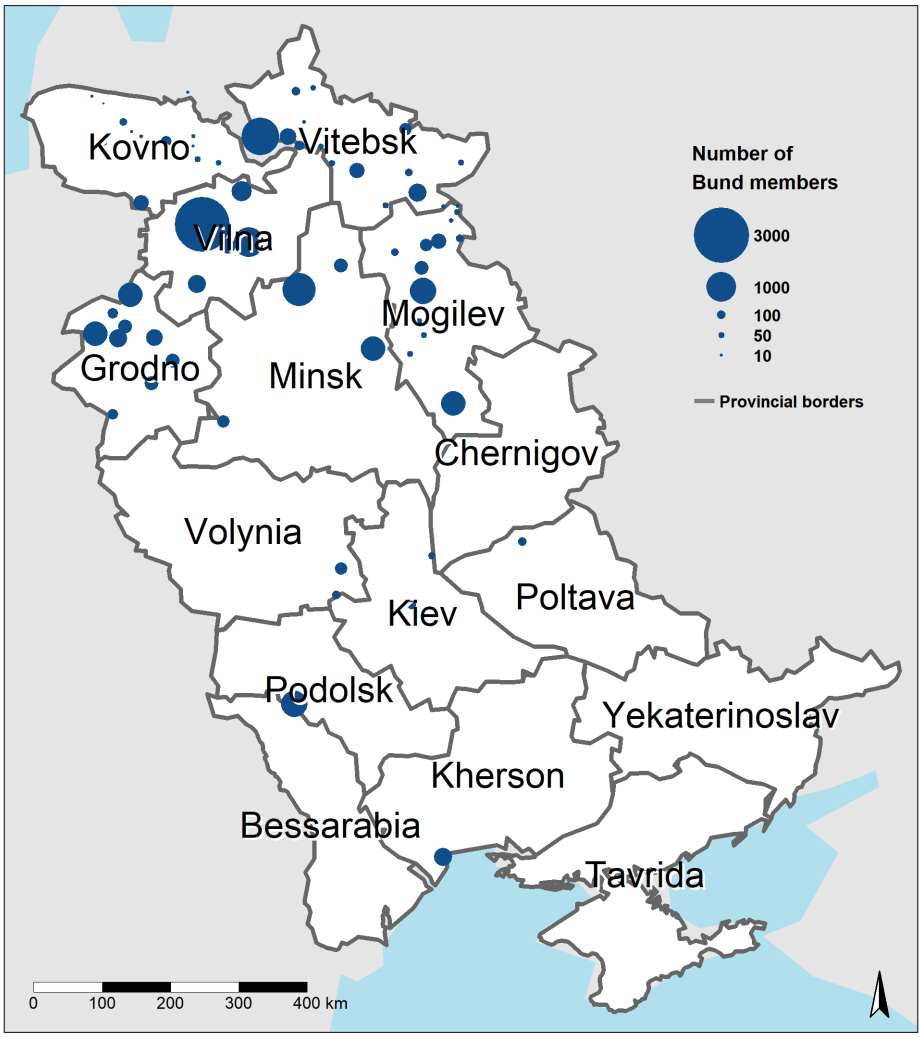


Figure A5: Bund branches, 1904. These data are drawn from the collection of Stepanskii, ed. (2010, pp. 391–392).

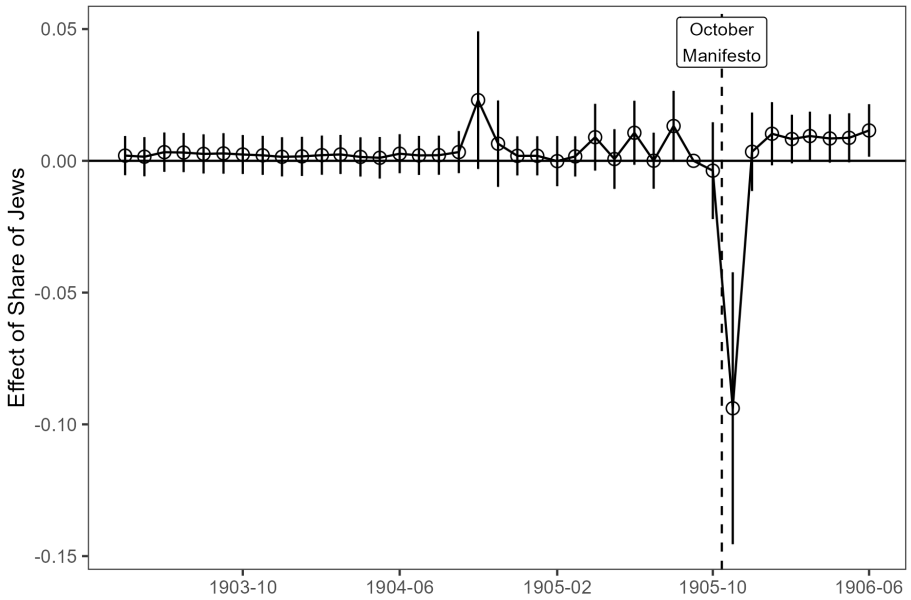


Figure A6: Event Study: Share of Jews, Extended Sample (April 1903–June 1906).

Additional tables

Table A1: Summary Statistics

Variable	N	Mean	SD	Min	Max
Main Variables					
Pogrom	35620	0.005	0.074	0	1
Temperature deviation	35620	-0.018	0.938	-2.640	3.799
Precipitation deviation	35620	0.291	1.176	-1.805	7.562
Share of Jews	1370	0.451	0.255	0.005	1.000
Share of Jews × (1 - Share of Jews)	1370	0.182	0.065	0.000	0.250
Urban	1370	0.156	0.363	0	1
Mestechko	1370	0.610	0.488	0	1
Log population	1370	7.798	1.066	6.107	12.909
Variables Specific to Certain Tables					
Distance to railroad	1370	0.019	0.017	0.000	0.104
Partial mobilization	35620	0.074	0.261	0	1
Travel time to provincial capital	1370	0.017	0.012	0.000	0.073
Travel time to provincial capital > median	1370	0.511	0.500	0	1
Log industrial workers per capita	1370	0.009	0.036	0.000	0.793
Log industrial output per capita	1370	0.918	1.582	0.000	7.355
Pogrom in 1881–1882	1370	0.023	0.151	0	1
Bund branch	1370	0.050	0.219	0	1
Log Bund members per 1000 people	1370	0.131	0.605	0.000	4.833
Log distance to Bund branch	1370	3.989	1.227	0.000	6.263
Police office	1370	0.377	0.485	0	1
Distance to troops	1370	31.181	20.847	0.000	114.512
Share of Jews among creditors	149	0.609	0.319	0.000	1.000
Share of Jews among grain traders	149	0.906	0.157	0.176	1.000
Variables Used in Appendix Tables					
Share of Jews with 0s imputed from Other	1443	0.431	0.263	0.004	1.000
Religious fractionalization	1370	0.411	0.148	0.000	0.718
Religious polarization	1370	0.727	0.232	0.000	0.998
Share of Jews × Share Orthodox	1370	0.123	0.087	0.000	0.249
Selo	1370	0.104	0.305	0	1
Rye price	45	0.687	0.082	0.530	0.910
Wheat price	45	0.828	0.059	0.690	0.970

Table A2: Correlates of Pogrom Incidence, Pre–November 1905 (Jewish Share)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share of Jews	0.0397*	0.0355*	0.0353*	0.0683***	0.0692***	0.0700***	0.0693***	0.0692***
	(0.0208)	(0.0214)	(0.0214)	(0.0237)	(0.0237)	(0.0237)	(0.0250)	(0.0250)
Log population		0.0759***	0.0753***	0.0647***	0.0650***	0.0637***	0.0637***	0.0638***
		(0.0097)	(0.0097)	(0.0100)	(0.0100)	(0.0097)	(0.0097)	(0.0097)
Temperature deviation			−0.0004	−0.0008	−0.0009	−0.0015	−0.0017	−0.0017
			(0.0035)	(0.0035)	(0.0034)	(0.0034)	(0.0034)	(0.0034)
Precipitation deviation			−0.0031*	−0.0037**	−0.0032*	−0.0033*	−0.0033*	−0.0033*
			(0.0019)	(0.0018)	(0.0018)	(0.0019)	(0.0019)	(0.0019)
Urban				0.0455*	0.0435*	0.0433*	0.0411	0.0409
				(0.0262)	(0.0262)	(0.0262)	(0.0263)	(0.0264)
Mestechko				−0.0545***	−0.0553***	−0.0566***	−0.0582***	−0.0583***
				(0.0140)	(0.0140)	(0.0142)	(0.0143)	(0.0143)
W × pogroms					0.0120	0.0097	0.0097	0.0098
					(0.0076)	(0.0079)	(0.0079)	(0.0081)
Distance to railroad						1.3955	1.4210*	1.4206*
						(0.8485)	(0.8472)	(0.8471)
Travel time to provincial capital						−2.5220**	−2.5547**	−2.5570**
						(1.2621)	(1.2589)	(1.2597)
Share Catholic							−0.0038	−0.0036
							(0.0272)	(0.0272)
Share Protestant							−0.0957**	−0.0952**
							(0.0414)	(0.0412)
Log industrial workers per capita								−0.0190
								(0.0951)
Observations	1,370	1,370	1,370	1,370	1,370	1,370	1,370	1,370
R^2	0.002	0.159	0.16	0.184	0.187	0.189	0.19	0.19
Adj R^2	0.002	0.149	0.149	0.172	0.174	0.176	0.175	0.174
Province fixed effects		✓	✓	✓	✓	✓	✓	✓

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.10$. The estimated magnitude of the relationship between Share of Jews and pogrom incidence is quite small: for Columns 4–8, an increase in Jewish share of one standard deviation is associated with an increase in pogrom incidence of approximately 8 percent of the output standard deviation (1.7 percentage points). See text for additional details and variable definitions. In each column, 1,370 observations, with a mean outcome of 0.046.

Table A3: Correlates of Pogrom Incidence, Pre–November 1905 (Polarization)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share of Jews	0.2697***	−0.1521	−0.1502	−0.0710	−0.0594	−0.0623	−0.0447	−0.0447
× (1 - Share of Jews)	(0.0776)	(0.0930)	(0.0926)	(0.0993)	(0.0962)	(0.0966)	(0.0968)	(0.0968)
Log population		0.0802***	0.0797***	0.0654***	0.0655***	0.0641***	0.0633***	0.0636***
		(0.0105)	(0.0104)	(0.0103)	(0.0103)	(0.0100)	(0.0099)	(0.0099)
Temperature deviation			−0.0004	−0.0009	−0.0010	−0.0016	−0.0017	−0.0017
			(0.0036)	(0.0035)	(0.0034)	(0.0034)	(0.0034)	(0.0034)
Precipitation deviation			−0.0031	−0.0037**	−0.0032*	−0.0033*	−0.0034*	−0.0034*
			(0.0019)	(0.0019)	(0.0018)	(0.0019)	(0.0019)	(0.0019)
Urban				0.0574**	0.0552**	0.0553**	0.0527*	0.0521*
				(0.0268)	(0.0267)	(0.0267)	(0.0269)	(0.0269)
Mestechko				−0.0370***	−0.0382***	−0.0389***	−0.0425***	−0.0427***
				(0.0137)	(0.0136)	(0.0140)	(0.0143)	(0.0143)
W × pogroms					0.0116	0.0092	0.0092	0.0093
					(0.0076)	(0.0080)	(0.0079)	(0.0080)
Distance to railroad						1.2856	1.3813	1.3806
						(0.8528)	(0.8482)	(0.8483)
Travel time to provincial capital						−2.4162*	−2.5738**	−2.5784**
						(1.2733)	(1.2604)	(1.2610)
Share Catholic							−0.0353	−0.0347
							(0.0252)	(0.0252)
Share Protestant							−0.0927**	−0.0917**
							(0.0422)	(0.0420)
Log industrial workers per capita								−0.0385
								(0.0971)
Observations	1,370	1,370	1,370	1,370	1,370	1,370	1,370	1,370
R^2	0.007	0.159	0.16	0.179	0.182	0.184	0.185	0.185
Adj R^2	0.006	0.149	0.149	0.166	0.169	0.17	0.17	0.169
Province fixed effects		✓	✓	✓	✓	✓	✓	✓

***$p < 0.01$; **$p < 0.05$; *$p < 0.10$. In each column, 1,370 observations, with a mean outcome of 0.046. See text for additional details and variable definitions.

Table A4: Share of Jews, Polarization, and Pogrom Incidence

	(1)	(2)	(3)	(4)	(5)
Share of Jews × Post–Oct 1905	−0.0152***	−0.0091**	−0.0082**	−0.0090**	−0.0081**
	(0.0036)	(0.0042)	(0.0040)	(0.0042)	(0.0040)
Share of Jews × (1 - Share of Jews) × Post–Oct 1905	0.0028	0.0005	−0.0002	−0.0145	−0.0143
	(0.0128)	(0.0167)	(0.0162)	(0.0178)	(0.0170)
Temperature deviation		0.0034***	0.0023**	0.0043***	0.0033***
		(0.0011)	(0.0011)	(0.0011)	(0.0011)
Precipitation deviation		0.0012	0.0005	0.0000	0.0001
		(0.0008)	(0.0007)	(0.0004)	(0.0004)
W × pogroms			0.0388***		0.0233**
			(0.0066)		(0.0101)
Urban × Post–Oct 1905		0.0175***	0.0173***	0.0072	0.0069
		(0.0056)	(0.0055)	(0.0054)	(0.0052)
Mestechko × Post–Oct 1905		0.0056*	0.0051*	0.0023	0.0016
		(0.0032)	(0.0031)	(0.0033)	(0.0031)
Log Population × Post–Oct 1905				0.0053***	0.0052***
				(0.0020)	(0.0020)
Temperature deviation × Post–Oct 1905				−0.0026	−0.0032
				(0.0031)	(0.0027)
Precipitation deviation × Post–Oct 1905				0.0038	0.0001
				(0.0025)	(0.0022)
W × pogroms × Post–Oct 1905					0.0241*
					(0.0135)
Settlement fixed effects	✓	✓	✓	✓	✓
Month fixed effects	✓	✓	✓	✓	✓
Province fixed effects × Post–Oct 1905		✓	✓	✓	✓

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.10$. These models include both Jewish population share and polarization to check whether the former is simply proxying for (part of the variation in) the latter. The results suggest that this is not the case: the treatment effects with both Jewish population share and polarization are very similar to those in the baseline model. In each column, 1,370 settlements and 35,620 observations, with a mean outcome of 0.0055.

Table A5: Second-Degree Polynomial in Share of Jews

	(1)	(2)	(3)	(4)	(5)
Share of Jews × Post–Oct 1905	−0.0124	−0.0086	−0.0084	−0.0235	−0.0223
	(0.0142)	(0.0193)	(0.0187)	(0.0204)	(0.0195)
(Share of Jews)^2 × Post–Oct 1905	−0.0028	−0.0005	0.0002	0.0145	0.0143
	(0.0128)	(0.0167)	(0.0162)	(0.0178)	(0.0170)
Temperature deviation		0.0034***	0.0023**	0.0043***	0.0033***
		(0.0011)	(0.0011)	(0.0011)	(0.0011)
Precipitation deviation		0.0012	0.0005	0.0000	0.0001
		(0.0008)	(0.0007)	(0.0004)	(0.0004)
W × pogroms			0.0388***		0.0233**
			(0.0066)		(0.0101)
Urban × Post–Oct 1905		0.0175***	0.0173***	0.0072	0.0069
		(0.0056)	(0.0055)	(0.0054)	(0.0052)
Mestechko × Post–Oct 1905		0.0056*	0.0051*	0.0023	0.0016
		(0.0032)	(0.0031)	(0.0033)	(0.0031)
Log Population × Post–Oct 1905				0.0053***	0.0052***
				(0.0020)	(0.0020)
Temperature deviation × Post–Oct 1905				−0.0026	−0.0032
				(0.0031)	(0.0027)
Precipitation deviation × Post–Oct 1905				0.0038	0.0001
				(0.0025)	(0.0022)
W × pogroms × Post–Oct 1905					0.0241*
					(0.0135)
Settlement fixed effects	✓	✓	✓	✓	✓
Month fixed effects	✓	✓	✓	✓	✓
Province fixed effect		✓	✓	✓	✓

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.10$. These models include a second-degree polynomial in Jewish share to allow for the possibility that pogrom incidence is nonmonotonically related to Jewish share through some functional form other than "polarization." The results provide no evidence for that conjecture. In each column, 1,370 settlements and 35,620 observations, with a mean outcome of 0.0055.

Table A6: Collapsed Pre- and Post-Periods

	(1)	(2)	(3)	(4)	(5)
Share of Jews × Post–Oct 1905	−0.1454***	−0.0975***	−0.0975***	−0.0988***	−0.0977***
	(0.0158)	(0.0238)	(0.0239)	(0.0249)	(0.0249)
Post–Oct 1905	0.1020***	0.0580***	0.0581***	0.0738***	0.0765***
	(0.0052)	(0.0045)	(0.0045)	(0.0045)	(0.0045)
Temperature deviation		−0.0465**	−0.0465**	−0.0335	−0.0364
		(0.0212)	(0.0212)	(0.0251)	(0.0254)
Precipitation deviation		0.0067	0.0066	−0.0137	−0.0119
		(0.0176)	(0.0177)	(0.0226)	(0.0230)
W × pogroms			0.0002		0.0036
			(0.0045)		(0.0083)
Urban × Post–Oct 1905		−0.0005	−0.0005	0.0115	0.0107
		(0.0260)	(0.0260)	(0.0321)	(0.0322)
Mestechko × Post–Oct 1905		0.0305*	0.0305*	0.0351	0.0350
		(0.0159)	(0.0159)	(0.0220)	(0.0220)
Log Population × Post–Oct 1905				−0.0053	−0.0052
				(0.0047)	(0.0048)
Temperature deviation × Post–Oct 1905				−0.0269	−0.0250
				(0.0214)	(0.0214)
Precipitation deviation × Post–Oct 1905				0.0415	0.0406
				(0.0296)	(0.0306)
W × pogroms × Post–Oct 1905					−0.0070
					(0.0109)
Settlement fixed effects	✓	✓	✓	✓	✓
Province fixed effects × Post–Oct 1905		✓	✓	✓	✓

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.10$. These models collapse the monthly panel dataset to two time periods: up to and including October 1905 and after October 1905. The estimated treatment effect is little affected. In each column, 1,370 settlements and 2,740 observations, with a mean outcome of 0.0642.

Table A7: Polarization and Fractionalization with Disaggregated Non-Jews

	(1)	(2)	(3)	(4)
Share of Jews × Post–Oct 1905		−0.0081*		−0.0076*
		(0.0043)		(0.0043)
Religious fractionalization × Post–Oct 1905	0.0005	−0.0029		
	(0.0066)	(0.0075)		
Religious polarization × Post–Oct 1905			0.0024	0.0004
			(0.0040)	(0.0046)
Temperature deviation	0.0033***	0.0033***	0.0033***	0.0033***
	(0.0011)	(0.0011)	(0.0011)	(0.0011)
Precipitation deviation	0.0001	0.0001	0.0001	0.0001
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
W × pogroms	0.0233**	0.0233**	0.0233**	0.0233**
	(0.0101)	(0.0101)	(0.0101)	(0.0101)
Urban × Post–Oct 1905	0.0052	0.0064	0.0049	0.0061
	(0.0050)	(0.0052)	(0.0050)	(0.0052)
Mestechko × Post–Oct 1905	−0.0007	0.0011	−0.0011	0.0008
	(0.0025)	(0.0030)	(0.0026)	(0.0031)
Log Population × Post–Oct 1905	0.0050**	0.0051**	0.0049**	0.0049**
	(0.0020)	(0.0020)	(0.0020)	(0.0020)
Temperature deviation × Post–Oct 1905	−0.0030	−0.0031	−0.0030	−0.0031
	(0.0027)	(0.0027)	(0.0027)	(0.0027)
Precipitation deviation × Post–Oct 1905	0.0002	0.0002	0.0002	0.0002
	(0.0022)	(0.0022)	(0.0022)	(0.0022)
W × pogroms × Post–Oct 1905	0.0241*	0.0241*	0.0241*	0.0241*
	(0.0135)	(0.0135)	(0.0135)	(0.0135)
Settlement fixed effects	✓	✓	✓	✓
Month fixed effects	✓	✓	✓	✓
Province fixed effects × Post–Oct 1905	✓	✓	✓	✓

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.10$. These models operationalize fractionalization and polarization (distinct concepts when there are more than two groups) from population shares of six religious groups: Jewish, Orthodox, Orthodox Old Believer, Protestant, Catholic, Muslim, and "Other." Variation in these variables is unrelated to the increase in pogroms after the Manifesto, and the treatment effect with Jewish share is essentially unaffected. In each column, 1,370 settlements and 35,620 observations, with a mean outcome of 0.0055.

Table A8: Polarization Calculated as Share of Jews × Share Orthodox

	(1)	(2)	(3)	(4)	(5)
Share of Jews × Share Orthodox	0.0407***	0.0179	0.0152	0.0074	0.0058
× Post–Oct 1905	(0.0105)	(0.0128)	(0.0120)	(0.0134)	(0.0124)
Temperature deviation		0.0034***	0.0023**	0.0043***	0.0033***
		(0.0011)	(0.0011)	(0.0011)	(0.0011)
Precipitation deviation		0.0012	0.0005	0.0000	0.0001
		(0.0008)	(0.0007)	(0.0004)	(0.0004)
W × pogroms			0.0388***		0.0233**
			(0.0066)		(0.0101)
Urban × Post–Oct 1905		0.0153***	0.0153***	0.0052	0.0050
		(0.0052)	(0.0050)	(0.0051)	(0.0049)
Mestechko × Post–Oct 1905		0.0028	0.0026	−0.0005	−0.0010
		(0.0023)	(0.0022)	(0.0026)	(0.0024)
Log Population × Post–Oct 1905				0.0050**	0.0049**
				(0.0020)	(0.0020)
Temperature deviation × Post–Oct 1905				−0.0024	−0.0030
				(0.0031)	(0.0027)
Precipitation deviation × Post–Oct 1905				0.0039	0.0002
				(0.0025)	(0.0022)
W × pogroms × Post–Oct 1905					0.0241*
					(0.0135)
Settlement fixed effects	✓	✓	✓	✓	✓
Month fixed effects	✓	✓	✓	✓	✓
Province fixed effects × Post–Oct 1905		✓	✓	✓	✓

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.10$. These models operationalize "polarization" as Jewish population share × Orthodox population share, excluding other religious groups from the calculation, under the assumption that Orthodox Christians were most affected by the Russian Orthodox Tsar's loss of authority. As in our baseline models, there is no robust association between ethno-religious polarization and pogrom incidence in our sample period. In each column, 1,370 settlements and 35,620 observations, with a mean outcome of 0.0055.

Table A9: Jewish Share Imputed from "Other"

	(1)	(2)	(3)	(4)	(5)
Share of Jews × Post–Oct 1905	−0.0152***	−0.0092**	−0.0083**	−0.0085**	−0.0076**
	(0.0035)	(0.0038)	(0.0037)	(0.0038)	(0.0037)
Temperature deviation		0.0036***	0.0026**	0.0046***	0.0037***
		(0.0011)	(0.0010)	(0.0012)	(0.0011)
Precipitation deviation		0.0011	0.0004	−0.0001	−0.0000
		(0.0008)	(0.0007)	(0.0004)	(0.0004)
W × pogroms			0.0390***		0.0228**
			(0.0065)		(0.0097)
Urban × Post–Oct 1905		0.0177***	0.0176***	0.0071	0.0071
		(0.0053)	(0.0052)	(0.0053)	(0.0051)
Mestechko × Post–Oct 1905		0.0060**	0.0058**	0.0020	0.0018
		(0.0025)	(0.0024)	(0.0027)	(0.0026)
Log Population × Post–Oct 1905				0.0050***	0.0049***
				(0.0019)	(0.0019)
Temperature deviation × Post–Oct 1905				−0.0031	−0.0036
				(0.0030)	(0.0027)
Precipitation deviation × Post–Oct 1905				0.0038	0.0001
				(0.0025)	(0.0021)
W × pogroms × Post–Oct 1905					0.0252*
					(0.0131)
Settlement fixed effects	✓	✓	✓	✓	✓
Month fixed effects	✓	✓	✓	✓	✓
Province fixed effects × Post–Oct 1905		✓	✓	✓	✓

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.10$. The sample for the regressions in this table includes 73 settlements in which Jewish population share is imputed as the share of "Other" religious groups. The estimated treatment effect is little affected. In each column, 1,443 settlements and 37,518 observations, with a mean outcome of 0.0055.

Table A10: Rye and Wheat Prices as Proxy for Weather Shocks

	(1)	(2)	(3)	(4)	(5)	(6)
Share of Jews × Post–Oct 1905	−0.0093**	−0.0090**	−0.0077**	−0.0093**	−0.0090**	−0.0077**
	(0.0040)	(0.0039)	(0.0038)	(0.0040)	(0.0040)	(0.0038)
Rye price	0.0332	0.0049	0.0088			
	(0.0371)	(0.0205)	(0.0186)			
Wheat price				0.1352***	−0.0043	0.0025
				(0.0507)	(0.0320)	(0.0286)
W x pogroms			0.0236**			0.0236**
			(0.0100)			(0.0100)
Urban × Post–Oct 1905	0.0175***	0.0062	0.0062	0.0175***	0.0062	0.0062
	(0.0053)	(0.0053)	(0.0051)	(0.0053)	(0.0053)	(0.0051)
Mestechko × Post–Oct 1905	0.0055**	0.0014	0.0009	0.0055**	0.0014	0.0009
	(0.0026)	(0.0029)	(0.0028)	(0.0026)	(0.0029)	(0.0028)
Log Population × Post-October 1905		0.0051***	0.0050**		0.0051***	0.0050**
		(0.0020)	(0.0019)		(0.0020)	(0.0019)
Rye price × Post-October 1905		0.1016	−0.0428			
		(0.1228)	(0.1073)			
Wheat price × Post-October 1905					0.2019***	0.0475
					(0.0778)	(0.0691)
W x pogroms × Post-October 1905			0.0240*			0.0232*
			(0.0135)			(0.0136)
Settlement fixed effects	✓	✓	✓	✓	✓	✓
Month fixed effects	✓	✓	✓	✓	✓	✓
Province fixed effects × Post–Oct 1905	✓	✓	✓	✓	✓	✓

***$p < 0.01$; **$p < 0.05$; *$p < 0.10$. These regressions substitute rye and wheat prices, defined at the provincial level, for the temperature and precipitation deviations used in the baseline models. The estimated treatment effect is little affected. Source: Mironov, Boris Nikolaevich, 1985, *Khlebnye tseny v Rossii za dva stoletiia (XVIII–XIX v.)*, Leningrad: Nauka, Leningradskoye otdeleniie. In each column, 1,370 settlements and 35,620 observations, with a mean outcome of 0.0055.

Table A11: Rural Settlements With and Without Churches

	(1)	(2)	(3)	(4)
Share of Jews × Post–Oct 1905	−0.0092**	−0.0086**	−0.0092**	−0.0085**
	(0.0040)	(0.0038)	(0.0039)	(0.0038)
Temperature deviation	0.0034***	0.0023**	0.0043***	0.0033***
	(0.0011)	(0.0011)	(0.0011)	(0.0011)
Precipitation deviation	0.0012	0.0005	0.0000	0.0001
	(0.0008)	(0.0007)	(0.0004)	(0.0004)
W × pogroms		0.0388***		0.0233**
		(0.0066)		(0.0101)
Urban × Post–Oct 1905	0.0174***	0.0167***	0.0053	0.0045
	(0.0056)	(0.0054)	(0.0058)	(0.0055)
Mestechko × Post–Oct 1905	0.0054*	0.0045	0.0004	−0.0007
	(0.0030)	(0.0029)	(0.0035)	(0.0033)
Selo × Post–Oct 1905	−0.0005	−0.0015	−0.0026	−0.0035
	(0.0033)	(0.0031)	(0.0034)	(0.0032)
Log Population × Post–Oct 1905			0.0052***	0.0051***
			(0.0020)	(0.0020)
Temperature deviation × Post–Oct 1905			−0.0026	−0.0032
			(0.0031)	(0.0027)
Precipitation deviation × Post–Oct 1905			0.0038	0.0002
			(0.0025)	(0.0022)
W × pogroms × Post–Oct 1905				0.0241*
				(0.0135)
Settlement fixed effects	✓	✓	✓	✓
Month fixed effects	✓	✓	✓	✓
Province fixed effect × Post–Oct 1905	✓	✓	✓	✓

***$p < 0.01$; **$p < 0.05$; *$p < 0.10$. These models include an interaction between *Selo* (church village) and post–October 1905, leaving *Derevniia* (village) × post–October 1905 as the excluded category. The estimated treatment effect is very similar to that in the baseline models. In each column, 1,370 settlements and 35,620 observations, with a mean outcome of 0.0055.

Table A12: Sensitivity to Conley Distance Cutoff

	(1)	(2)	(3)	(4)	(5)
Share of Jews × Post-Oct 1905	−0.0151	−0.0091	−0.0082	−0.0086	−0.0077
25 km	(0.0034)***	(0.0037)**	(0.0036)**	(0.0037)**	(0.0036)**
50 km	(0.0037)***	(0.0039)**	(0.0038)**	(0.0039)**	(0.0038)**
75 km	(0.0039)***	(0.0040)**	(0.0038)**	(0.0040)**	(0.0038)**
100 km	(0.0041)***	(0.0042)**	(0.0040)**	(0.0042)**	(0.0039)**
150 km	(0.0046)***	(0.0046)**	(0.0043)*	(0.0046)*	(0.0042)*
Settlement fixed effects	✓	✓	✓	✓	✓
Month fixed effects	✓	✓	✓	✓	✓
Province FE × Post-Oct 1905		✓	✓	✓	✓

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. Column numbers refer to specifications in Table 2. Each column reports Conley standard errors and associated significance levels for the estimated treatment effect using five alternative distance cutoffs.

Table A13: Heterogeneity by Settlement Type

	Urban	Mestechko	Rural	Urban	Mestechko	Rural	Urban	Mestechko	Rural
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Share of Jews × Post–Oct 1905	−0.0636***	−0.0090**	−0.0062	−0.0234	0.0007	−0.0106**	−0.0169	0.0006	−0.0086*
	(0.0226)	(0.0044)	(0.0038)	(0.0359)	(0.0042)	(0.0051)	(0.0345)	(0.0040)	(0.0050)
Temperature deviation				0.0111***	0.0020*	0.0011	0.0097***	0.0012	0.0003
				(0.0036)	(0.0012)	(0.0010)	(0.0035)	(0.0012)	(0.0010)
Precipitation deviation				0.0008	−0.0004	0.0006	0.0004	−0.0004	0.0007
				(0.0022)	(0.0003)	(0.0008)	(0.0022)	(0.0003)	(0.0007)
W × pogroms							0.0469*	0.0155**	0.0227
							(0.0253)	(0.0075)	(0.0146)
Log Population × Post–Oct 1905				0.0073	0.0034**	0.0084**	0.0075	0.0034**	0.0078**
				(0.0057)	(0.0017)	(0.0037)	(0.0056)	(0.0016)	(0.0035)
Temperature deviation × Post–Oct 1905				−0.0156*	0.0022	−0.0020	−0.0165**	0.0011	−0.0011
				(0.0084)	(0.0025)	(0.0038)	(0.0077)	(0.0024)	(0.0035)
Precipitation deviation × Post–Oct 1905				0.0101	0.0052**	0.0050	0.0016	0.0022	0.0039
				(0.0086)	(0.0021)	(0.0036)	(0.0080)	(0.0018)	(0.0033)
W × pogroms × Post–Oct 1905							0.0236	0.0189*	0.0208
							(0.0342)	(0.0111)	(0.0199)
"'latex Observations	5,564	21,736	8,320	5,564	21,736	8,320	5,564	21,736	8,320
Settlements	214	836	320	214	836	320	214	836	320
Mean outcome	0.0225	0.0023	0.0023	0.0225	0.0023	0.0023	0.0225	0.0023	0.0023
Settlement fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Province fixed effects × Post–Oct 1905				✓	✓	✓	✓	✓	✓

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.10$. The regressions in this table are estimated separately on the sample of urban settlements, mestechki, and rural settlements, respectively.