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# Negative shocks and mass persecutions: evidence from the Black Death

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## Abstract

We study the Black Death pogroms to shed light on the factors determining when a minority group will face persecution. Negative shocks increase the likelihood that minorities are persecuted. But, as shocks become more severe, the persecution probability decreases if there are economic complementarities between majority and minority groups. The effects of shocks on persecutions are thus ambiguous. We compile city-level data on Black Death mortality and Jewish persecutions. At an aggregate level, scapegoating increases the probability of a persecution. However, cities which experienced higher plague mortality were less likely to persecute. Furthermore, for a given mortality shock, persecutions were more likely where people were more inclined to believe conspiracy theories that blamed the Jews for the plague and less likely where Jews played an important economic role.


**Keywords** Economics of mass killings · Inter-group conflict · Minorities · Persecutions · Scapegoating · Biases · Conspiracy theories · Complementarities · Pandemics · cities

**JEL Classifications** D74 · J15 · D84 · N33 · N43 · O1 · R1

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What factors make minorities vulnerable to persecution? We answer this question by studying Jewish communities during the Black Death, the greatest demographic shock in European history: about 40% of the population died between 1347 and 1352 (Benedictow 2005; Christakos et al. 2005). We use data on city-level Black Death mortality rates and Jewish persecution to show that the higher mortality was in a city, the less likely was a persecution to occur. We find that this effect was attenuated in cities where people were inclined to believe Jews had caused the plague and accentuated in places where Jews played an important economic role.

Understanding the causes of the Black Death persecutions is important in its own right. These have been described as "the most monumental of medieval Jewish persecutions" (Cohn 2007) and "precursor(s) of the Holocaust" (Goldhagen 2013). Studying this episode is also important because it sheds light on other episodes of intergroup conflict. Minorities remain targets of violence in many parts of the world. But why are minorities protected in some places but not in others?[1]

We consider a simple framework to analyze how a negative shock affects the incentive to persecute a minority community that provides important economic services but can also be held responsible for the shock. As the severity of the shock increases, we expect two potentially conflicting effects. First, we expect a *scapegoating* effect: If the minority is blamed for the shock, then the incentive to persecute them can increase with the severity of the shock. Second, there may be a *complementarities* effect: If the economic importance of the minority increases as the shock worsens, then there is a greater incentive to protect them.

We compile city-level data on the virulence and spread of the Black Death along with information on the locations and characteristics of Jewish communities and persecutions across Western Europe. These data cover both the Black Death period (1347–1352) and surrounding centuries (1100–1850). Among the 1869 cities in Western Europe, 363 had a Jewish community at the onset of the Black Death. We have estimates of Black Death mortality rates for 263 localities. Our main sample is the intersection of these two datasets, which consists of the 124 cities that had a Jewish community in 1347 and for which we know their mortality rates. About half of cities with a Jewish community suffered a persecution during the Black Death. However, cities with higher mortality rates were *less* likely to persecute their community.

We provide evidence that these results are causal. (i) The virulence of the plague was plausibly unrelated to factors related to persecutions. (ii) The parallel trends assumption is verified as, prior to 1347, there was no difference in persecutions between the cities most affected and those less affected by the Black Death. (iii) Results are robust to the inclusion of geographic and institutional controls, as well as controls for community size and past persecutions. (iv) We implement instrumental variables (IV) strategies premised on the fact that the Black Death entered Europe through the Sicilian port of Messina and that it was more virulent early on. Our first IV is market access to Messina conditional on market access to all cities, as it was the specific connectedness to Messina and not the connectedness to other important cities that mattered for plague virulence. The second IV relies on the number of months between the year-month the city was infected and October 1347 (the date of first contact), exploiting the randomness of the timing of infection.

We shed light on the pathways that explain both why the overall level of persecution was high and why cities with higher mortality rates persecuted less. We find that, conditional on the size of the mortality shock, Jews were more likely to be persecuted in towns where people were inclined to believe antisemitic allegations (the *scapegoating* effect). Starting with

[1] Many minority groups across the world still suffer violent persecutions today, with some recent examples of ethnic and religious cleaning in the Central African Republic, Myanmar, South Sudan, Sudan, Sri Lanka, and Syria.

the First Crusade (1096), persecutions were increasingly perpetrated against Jews. From the twelfth century onwards, Jews were accused of ritually murdering Christian children. During the Black Death, Jews were tortured into confessing to have caused the plague by poisoning wells. We find that the protective effect of high mortality was attenuated for towns closer to where such accusations were made. The protective effect was also weaker in cities first infected during Christmastide and Easter—when Christians historically blamed Jews for the death of Jesus—and stronger for Advent and Lent—when Christians were doing penance.

Conversely, Jews were less likely to be persecuted at higher mortality rates in cities where they could offer specialized economic services (the *complementarities* effect). Conditional on the size of the shock, we find a lower probability of persecution in larger and better connected cities and in cities where Jews were offering moneylending services or services to the trading sector. In addition, cities with a community grew relatively faster than cities without and cities that persecuted their community during the Black Death grew relatively slower in the following centuries. We provide evidence that the complementarities effect may have contributed to the lower persecution probability in high-mortality cities and consider a range of alternative explanations.

We contribute to the literature on the economic determinants of violence against minorities (Easterly et al. 2006; Montalvo and Reynal-Querol 2008; Caselli et al. 2015; Rogall and Yanagizawa-Drott 2016). Arbatli et al. (2019) show that genetic diversity, as determined predominantly tens of thousands of years ago, can explain the frequency, incidence, and onset of civil conflicts and interethnic violence. Esteban et al. (2015) find that genocides occur when the surplus from eliminating a minority group exceed the productivity losses from conflict.

We study the Black Death, a major economic and demographic shock that resulted in mass persecutions at the macro-level. We are not the first to examine the Black Death pogroms. In an important contribution, Voigtländer and Voth (2012) use data on the Black Death pogroms (but not on Black Death mortality rates) to explore the persistence of antisemitic cultural traits from the fourteenth century through to the twentieth century in Germany.

Recent research suggests that major economic shocks can generate hostility towards minorities (Mayda 2006; Dippel et al. 2015; Autor et al. 2016; Becker et al. 2017; Anderson et al. 2018). In medieval Europe, colder weather made antisemitic violence more likely (Anderson et al. 2017). Similarly, in Czarist Russia, Grosfeld et al. (2018) find that pogroms were more likely when economic crises intersected with political unrest. Finley and Koyama (2018) explore how political institutions shaped the Black Death pogroms in the Holy Roman Empire. We believe that this is the first paper to study how a major shock—the Black Death—interacted with patterns of economic complementarity and substitutability. Understanding this interaction is crucial since it is in the context of large-scale shocks such as the Black Death or World War II that the worst episodes of antisemitic violence took place.

The European economy on the eve of the Black Death was Malthusian (Ashraf and Galor 2011; Voigtländer and Voth 2013). Due to population growth in previous centuries, per capita income in much of fourteenth century Europe was close to subsistence. The Black Death was a tremendous shock to medieval economies and society. In the decades that followed, per capita incomes rose due to labor scarcity. But in the initial aftermath of the plague, trade and economic activity collapsed (Campbell 2016). Given this collapse, and the reliance of the medieval economy on trade and credit networks, Jewish communities played an important role in maintaining production and ensuring that trade and credit did not dry up. We argue that this can explain why, at the microlevel, the likelihood of a persecution decreased in the severity of the plague shock.

Recent scholarship has established that patterns of economic complementarities between groups shape the vulnerability of a minority. Jha (2013) finds that toleration of Muslims in majority Hindu Indian cities depends on the complementarity/substitutability of the services they provide.[2] Examining pre-and post-Reformation Germany, Becker and Pascali (2019) find that the Protestant Reformation induced a change in the geography of antisemitism. Where Protestantism meant that Jews were exposed to competition from Christian moneylending, this had the effect of *lowering* the cost of antisemitism as evidenced by both the increased frequency of persecutions and an increase in the publication of antisemitic literature. They find that the increase in antisemitism following the Reformation was larger in cities where trade was important before the Reformation. The differences that they find in post-Reformation Germany are associated with antisemitism in late nineteenth century Germany.

Europe during the Black Death provides an apt setting to explore the causes of persecutions against minority groups. We have many cities, in more than one country, with two well-identified groups. We exploit a massive, highly variable, and plausibly random shock. Moreover, we examine a historical setting in which there were numerous decentralized polities with weak state capacity and which thus resembles that observed in poor countries today, where minorities often face violence. We exploit the rich variation in initial conditions across cities and study how the shock-persecution relationship varies with economic and other social institutions, allowing us to study the effects of both scapegoating and complementarities.

## 1 Conceptual framework

Web Appendix Section 1 outlines a simple framework that describes how a shock can affect the probability that a minority group is persecuted. We consider a minority group and a majority group. The former contributes to economic production through specialized economic services such as moneylending, but is resented by the majority. The decision to persecute the minority depends on their net effect on the utility of a representative member of the majority community.

The Black Death is a shock that affects both income, which fell in the short-run because economic activity was severely disrupted, and utility directly through the deaths of relatives and friends.[3] By definition, in every town with a minority community prior to the shock, the net utility of having their presence was at least weakly positive. We therefore define an indifference condition at which the majority community is indifferent about retaining the minority community. Whether the shock could induce a persecution of the minority depends on how the magnitude of the shock affects this condition.

If the minority is held to be responsible for the shock, the larger the shock, then the more the majority community will resent their presence. But if the negative shock raises the marginal value of economic services provided by the minority community, then their value to the rest of society will be increasing in the severity of the shock. We label the former effect the *scpaegoating* effect and the latter effect the *complementarities* effect. We show that the majority group will persecute the minority community if the scapegoating effect is larger than the complementarities effect.

[2] Jha (2018) models the conditions under which trade can support peaceful coexistence between groups and those under which groups will be subject to violence.

[3] In the medium- and long-run, wages increased due to greater labor scarcity. But the immediate economic impact of the plague was negative (Campbell 2016).

A further prediction of this model is that if the relative size of the complementarity and scapegoating effects varies from city to city, then a given mortality shock could have differential effects. Moreover, for a given set of city characteristics, the size of the shock might give rise to differential effects. If the economic role of the minority becomes more important in the presence of a large shock, this can generate an inverted U-shaped relationship between Black Death mortality and the probability of a persecution.

Our framework considers the utility of a representative member of the majority community. However, the majority group of medieval towns consisted of different subgroups, for the sake of simplicity city officials and the populace (which in turn consisted of merchants, artisans, laborers and peasants from the nearby countryside). The populace may be more likely to believe theories casting blame on the minority group. City officials may be more likely to internalize the positive economic externalities coming from their presence. The minority community may disproportionately contribute to aggregate output, which increases tax revenues, and also provide special taxes. In that case, whether a persecution occurs depends on how the scapegoating and complementarities effects vary for both subgroups and the balance of power between them.

We proxy for the scapegoating and complementarities effects in our analysis using characteristics of the towns. We will proxy for the strength of the scapegoating effect using data such as how close a town was to the origin of rumors of well poisoning by Jews, whether the town had a history of persecuting Jews, and the timing of the arrival of the Plague (e.g. Easter). Similarly, we will proxy for the strength of the complementarities effect using measures such as whether Jews were lending money in the town and whether Jewish traders had access to more markets.

## 2 Data

This section presents our data (see the Web Appendix for more details and Web Appendix Table A.1 for summary statistics).

*Black Death mortality* Data on cumulative Black Death mortality for the period 1347–1352 come from Christakos et al. (2005, 117–122) who compile data on mortality rates based on information from a wide array of historical sources including ecclesiastical records, parish records, testaments, tax records, court rolls, chroniclers' reports, donations to the church, financial transactions, passing away of famous people, letters, edicts, guild records, hospital records, cemeteries and tombstones. Christakos et al. (2005) carefully examine each data point and arbitrate between conflicting estimates based on the best available information. We have checked these data by consulting numerous other sources including Ziegler (1969), Russell (1972), Gottfried (1983), and Benedictow (2005). Details on how the data was constructed and which mortality estimates were selected by Christakos et al. (2005) are provided in Web Appendix Section 4. These data yield estimates of mortality for 263 localities in 13 countries. Figure 1 depicts these 263 localities and their mortality rates.

For 166 of these 263 localities we have a percentage estimate of the mortality rate. For example, Venice had a mortality rate of 60%. In other cases the sources report more qualitative estimates: (i) For 49 towns Christakos et al. (2005) provide a literary description of mortality. We rank these descriptions based on the implied magnitude of the shock and assign each

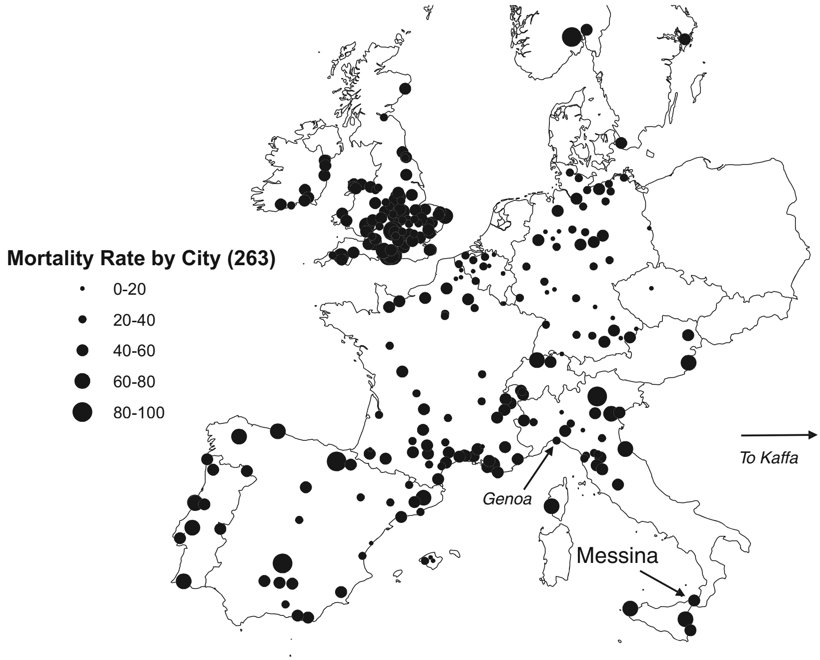


**Fig. 1** Cumulative Black Death mortality rates (%) in 1347–1352. *Notes* This map depicts Black Death mortality rates (%, 1347–1352) for 263 localities in 13 Western European countries: Austria, Belgium, the Czech Republic, France, Germany, Ireland, Italy, Norway, Portugal, Spain, Sweden, Switzerland and the United Kingdom. The main source for the mortality data is Christakos et al. (2005)

one of them a numeric mortality rate.[4] (ii) For 19 towns we know the mortality rate of the clergy. Christakos et al. (2005) provide evidence that clergy mortality was on average 8% higher than general mortality, so we divide the clergy mortality rates by 1.08 to obtain their mortality rate. (iii) For 29 towns we know the desertion rate, which includes both people who died and people who never came back. Following Christakos et al. (2005, 154–155), who show that the desertion rate is on average 1.2 times higher than the mortality rate, we divide desertion rates by 1.2 to obtain their mortality rate. Since the data generated for the towns of (i), (ii) and (iii) require various assumptions, we will show that our results are robust to using only numerical estimates.

*Jewish presence, Jewish persecution, and main sample* The *Encyclopedia Judaica* provides comprehensive coverage on Jewish life to determine which towns had Jewish communities (Berenbaum et al. 2007a). The *Encyclopedia Judaica* provides details on Jewish presence, economic activities, and persecutions. The length of entry varies: some are rich with historical information, others are shorter. An example entry is from Toulon:

> In the second half of the thirteenth century the Jews made up an appreciable proportion of the population of Toulon: at a general municipal assembly held in 1285, 11 of the 155 participants were Jews. […] The community came to a brutal end on the night of April 12/13, 1348 (Palm Sunday), when the Jewish street, "Carriera de la Juteria," was

[4] 5% for "spared" or "escaped", 10% for "partially spared" or "minimal", 20% for "low", 25% for "moderate", 50% for "high", 66% for "highly depopulated", and 80% if the town is "close to being depopulated" or "decimated".

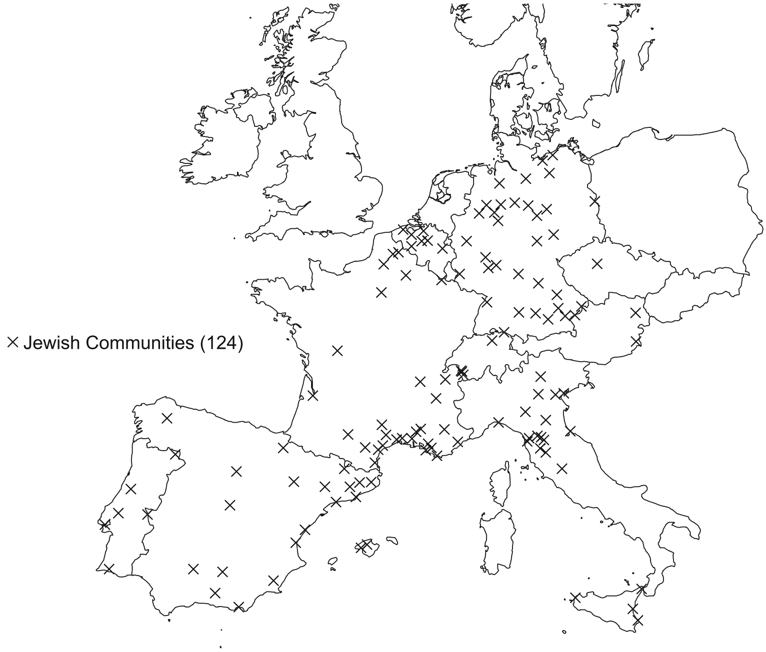


**Fig. 2** Main Sample of 124 Towns with a Jewish community and mortality data. *Notes* This map shows the 124 towns of our main sample, i.e. the towns with Jews present circa 1347 and for which we know the cumulative Black Death mortality rate (%, 1347–1352). These 124 towns belong to 9 countries using today's boundaries: Austria, Belgium, the Czech Republic, France, Germany, Italy, Portugal, Spain and Switzerland

> attacked, the houses pillaged, and 40 Jews slain; this attack was probably related to the Black Death persecutions. […] After this date, in addition to a few converted Jews, there were in Toulon only individual Jews who stayed for short periods.

We supplement these data where possible with other sources including the *Jewish Encyclopedia* (Adler and Singer 1906). Among the 1869 Western European towns that reached 1000 inhabitants at one point between 800 and 1850 in the Bairoch (1988) database and/or belong to the Christakos et al. (2005) database of mortality rates, we have identified 363 towns in which Jews were present circa the onset of the Black Death in 1347. Of these 363 Jewish communities, 124 can be matched with locations to our database of mortality rates.

Our main sample thus consists of 124 towns with a Jewish community at the onset of the Black Death (circa 1347) and for which we know the Black Death mortality rate (%, 1347–1352). Figure 2 depicts these 124 towns. In addition, we also know for the full sample of 1869 towns which towns had a Jewish community in each year from 1100 to 1850. Our sample is representative: The 124 towns account for 66.7% of the total population of the 363 towns with a Jewish community at the onset of the Black Death, and 43% of the total population of the 1869 towns in 1300.

Data do not exist on the size of most Jewish communities. We have estimates of the Jewish share of the population for 30 out of the 124 towns in our dataset.

Our dependent variable is whether a community was persecuted. We focus on the Black Death period (1347–1352), but we have these data for each town with community from 1100 to 1850. Our definition of a persecution encompasses both a pogrom or an expulsion. For our main sample, and during the Black Death period only, we use additional information on the date of the persecution, the number of victims, whether the community was annihilated, whether Jews were burned, and whether a mob was involved in the persecution.

*Black Death spread* We use the data from Christakos et al. (2005) to obtain for 95 of the 124 towns the year and month of first infection. For the other 29 towns, we combine information for neighboring towns, maps of the epidemic spread, and extra sources to impute the year-month of first infection.[5] Information is sparser for the year-month of last infection.

*Controls* Geographic controls include mean growing season temperature in 1500–1600, elevation, soil suitability for cereal production and pastoral farming, dummies for whether the town is within 10 km from a coast or river, and longitude and latitude.[6]

To control for factors related to trade, we employ data on populations in 1300 from Jebwab et al. (2016), who combine data from both Bairoch (1988) and Chandler (1987). The last two sources represent attempts to collect information on populations for all towns with at least 1000 inhabitants. For the towns for which population is not available, city size must have been less than 1000; we thus arbitrarily assume that their population was 500. We also control for the presence of major and regular Roman roads (and their intersections) using the GIS data from McCormick et al. (2013), medieval trade routes (and their intersections) after digitizing a map from Shepherd (1923), and two dummies capturing the presence of medieval market fairs and membership in the Hanseatic league based on information from Dollinger (1970). We also calculate market access for every town to the 1869 towns of the full sample in 1300. Following Donaldson and Hornbeck (2016), market access for town $i$ is defined as $MA_i = \Sigma_j \frac{L_j}{\tau_{ij}^{\sigma}}$, with $L_j$ being the population of town $j \neq i$, $\tau_{ij}$ the travel time, i.e. network distance, between town $i$ and town $j$, and $\sigma = 3.8$, as in Donaldson (2018). We compute the least cost travel paths via four transportation modes—sea, river, road and walking—using the data from Boerner and Severgnini (2014) who estimate the speed at which the Black Death traveled via each mode of transportation.

Our human capital controls include a dummy for whether a town possessed a university (Bosker et al. 2013) and for whether a town was within 10 km from a Roman aqueduct (Talbert 2000). To control for institutions, we distinguish between towns that were located in monarchies and self-governing cities around 1300 (Bosker et al. 2013; Stasavage 2014). We control for whether the town was a state capital around 1300. We measure parliamentary activity during the fourteenth century using data from van Zanden et al. (2012) and control for whether a city was within 100 km of a battle that took place between 1300 and 1350.

[5] For example, for Landshut in Germany we learn from Benedictow (2005, 190) that the epidemic went from Mühldorf to the neighboring town of Landshut (50 km). From Christakos et al. (2005), we know that Mühldorf and Regensburg were first infected in June and July 1349, respectively. Since Landshut is about one-half of the way between Mühldorf and Regensburg, it must have been infected in June or July 1349, but most probably in June 1349.

[6] We use 10 km for the coast and river dummies and some controls described below, due to measurement error.

## 3 Historical setting

*The Black Death* The Black Death arrived in Europe in October 1347. Over the next few years it spread across the continent killing between 30 and 50% of the population.[7] Recent discoveries in plague pits have corroborated the hypothesis that the Black Death was Bubonic plague. The bacterium *Yersinia Pestis* was transmitted by the fleas of the black rat. Infected fleas suffer from a blocked esophagus. These "blocked" fleas are unable to sate themselves and continue to bite rats or humans, regurgitating the bacterium into the bite wound thereby infecting rats or humans. Within less than a week, the bacteria is transmitted from the bite to the lymph nodes producing buboes. Once infected, death occurred within 10 days with 70% probability.[8]

While the vector for bubonic plague is infected fleas, fleas cannot spread the disease far in the absence of hosts such as black rats. The spread of the plague was rapid and its trajectory was largely determined by chance. One important means of transmission depended on which ship became inhabited with infected fleas. It was largely coincidence that the plague spread first from Kaffa in the Black Sea to Messina in Sicily in October 1347 rather than elsewhere as the ships carrying the plague were originally bound to Genoa (but the ships could have gone to other Mediterranean ports). Figure 1 shows the locations of Messina, Kaffa, and Genoa. Similarly, it was coincidental that the plague spread first from Messina to Marseilles, rather than to, say, Barcelona, Lisbon, or Antwerp. From the various coasts where infected ships docked the plague then spread inland along rivers, roads, and paths, as boats and carts were inadvertently carrying infected fleas. The local spread of the plague thus also depended on the local populations of black rats. Since black rats are territorial, their numbers were not correlated with population density (Benedictow 2005). For example, similar death rates are recorded in urban and in rural areas (Herlihy 1965).

For epidemiological reasons, virulence was greater in towns affected earlier on (Christakos et al. 2005, 212–213). Initially, epidemics spread exponentially. Eventually, epidemics run out of victims, a factor that forces the disease to mutate so as to favor benign pathogens.[9] People may also develop immunities and pathogen mutation may increase individual memory immune responses due to "contacted individuals becoming infected only if they are exposed to strains that are significantly different from other strains in their memory repertoire" (Girvan et al. 2002). In other words, pathogen mutation and natural immunization eventually cause an epidemic to end.

All towns were affected by the Black Death. Once an outbreak began in a town, mortality increased rapidly before peaking 2–3 months after the date of first infection and then declining (Christakos et al. 2005, 212–213). Consistent with this, when we use available data on the year and month of first and last infection for 39 out of the 124 towns, the mean duration of

[7] Conventionally the overall death rate was estimated at 1/3. Recent studies suggest that the death rate was higher than this (see Benedictow 2005, 2010; Aberth 2009). For the 124 towns, the population-weighted average is 38.0%.

[8] See Benedictow (2005, 2010). The importance of blocked fleas as the main vector of transmission is currently under debate. Other vectors such as lice may also have been at work. The literature agrees, however, that person-to-person transmission was probably rare and cannot account for the diffusion of the plague (Campbell 2016, 235).

[9] According to Berngruber et al. (2013): "[…] selection for pathogen virulence and horizontal transmission is highest at the onset of an epidemic but decreases thereafter, as the epidemic depletes the pool of susceptible hosts […] In the early stage of an epidemic susceptible hosts are abundant and virulent pathogens that invest more into horizontal transmission should win the competition. Later on, the spread of the infection reduces the pool of susceptible hosts and may reverse the selection on virulence. This may favor benign pathogens […]."

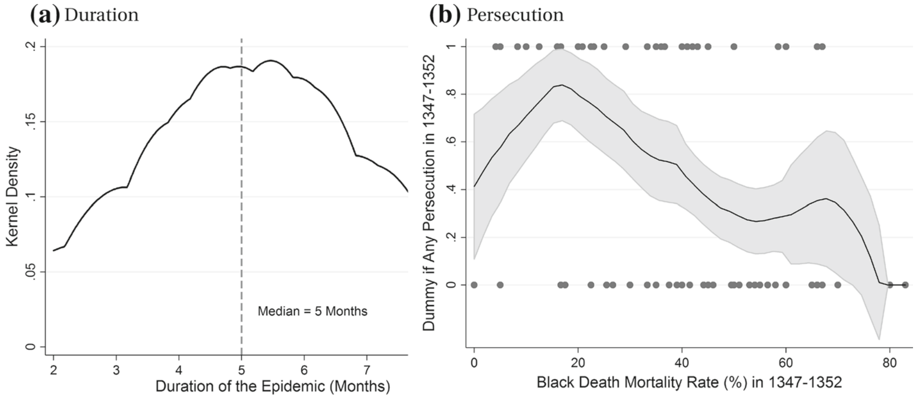


**Fig. 3** Distribution of the Duration of the Black Death, and relationship between cumulative Black Death mortality and the likelihood of a persecution in 1347–1352. *Notes:* Panel **a** Kernel distribution of the duration of the Black Death in each city—i.e. the time difference between the month of the first infection in the city and the month of the last infection in the city (information available for 39 out of the 124 towns of the main sample). The mean and median durations were 5 months. Panel **b** Main sample of 124 Jewish towns for which we have data on Black Death mortality there is a positive and then negative correlation between the likelihood of a persecution and the mortality rate in 1347–1352 (local polynomial smooth plot with a bandwidth of 5% points of mortality). See Web Appendix for more details on data sources

the plague was 5 months. Figure 3a shows the distribution of the duration of the epidemic for these.

Based on the historical and epidemiological facts described above, it is apparent that the spread, and thus the local virulence, of the plague had a significant random component. When studying variation in mortality rates across space, historical accounts have been unable to rationalize the patterns in the data (Ziegler 1969; Gottfried 1983; Theilmann and Cate 2007; Cohn and Alfani 2007). To illustrate, Venice had high mortality (60%) while Milan escaped comparatively unscathed (15% mortality). Highly urbanized Sicily suffered heavily from the plague. Equally urbanized Flanders, however, had relatively low death rates. Southern Europe and the Mediterranean were hit especially hard, but so were the British Isles and Scandinavia.[10] Likewise, Christakos et al. (2005, 150) explain that some scholars have "argued that Black Death hit harder the ports and large cities along trade routes" but that "the generalization is logically valid at a regional level at best" and that "examples and counterexamples abound, making it impossible to reach any definite conclusion." Consistent with this, Figure 4a illustrates the lack of a relationship between mortality rates and city population in 1300 (Y = 40.36*** − 0.55 X; Obs. = 124; $R^2 = 0.00$). Likewise, Fig. 4b shows that there is no relationship between mortality rates and city market access in 1300 (Y = 38.45*** + 0.85 X; Obs. = 124; $R^2 = 0.01$). Similarly, some scholars have argued that death rates from the plague were lower in mountainous regions, but mortality rates in mountainous Savoy were high whereas "despite Switzerland having the most rugged terrain in Europe, the Black Death reached almost every inhabited region of the country" (Christakos et al. 2005, 150).

One may nonetheless wonder to what extent the lack of observed relationship may be due to measurement error. However, random measurement error in dependent variables, here mortality, does not lead to bias, only to higher standard error. Even if we used the "true"

[10] Variation in sanitation does not explain this pattern. Gottfried (1983, 69) notes "it would be a mistake to attribute too much to sanitation" given the "failure of Venice's excellent sanitation to stem the deadly effect of the plague".

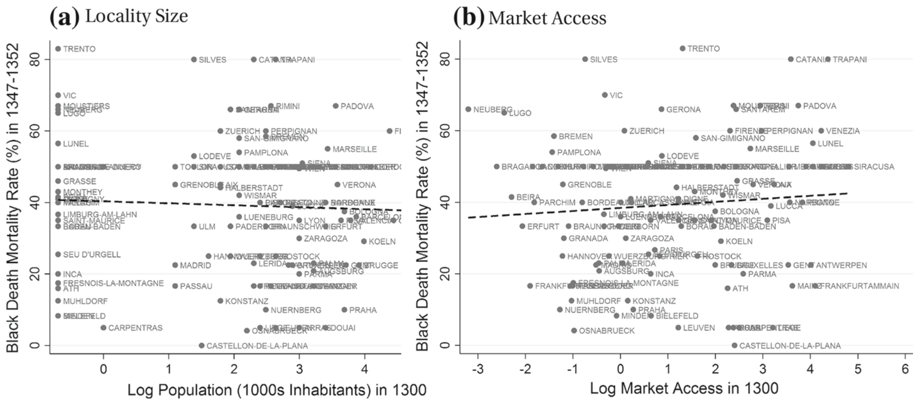


**Fig. 4** Cumulative Black Death mortality, locality size, and market access in 1300. *Notes* Panel **a** depicts the relationship between mortality rates (%, 1347–1352) and log population size in 1300 for our main sample of 124 Jewish towns (Y = 40.36*** − 0.55 X; Obs. = 124; $R^2 = 0.00$). Panel **b** Relationship between mortality rates (%, 1347–1352) and log market access to all 1869 towns in 1300 for our main sample of 124 Jewish towns (Y = 38.45*** +0.85 X; Obs. = 124; $R^2 = 0.01$). Market access for city $i$ is defined as $MA_i = \Sigma_j \frac{P_j}{D_{ij}^{\sigma}}$, with $P_j$ being the population of town $j \neq i$, $D_{ij}$ the travel time between city $i$ and city $j$, and $\sigma = 3.8$. To obtain the travel times, we compute the least cost travel paths via four transportation modes—by sea, by river, by road and by walk—with the transportation speeds from Boerner and Severgnini (2014). See Web Appendix for more details on data sources

mortality rates, the $R^2$ would still remain equal to 0.00–0.01. Random measurement error in the variable of interest, here market access, could then produce a downward bias. However, our measure of market access follows a specification and uses a distance elasticity of trade that is standard in the literature (Donaldson and Hornbeck 2016; Donaldson 2018).[11] Since market access could still be misestimated, we will show below that, conditional on other controls, there is no positive significant effect of other measures of transportation and trade networks, namely coastal access, rivers, roads, market fairs and trading routes.

Subsequent outbreaks of bubonic plague reoccurred in Europe for two and a half centuries following the Black Death. Epidemiologists and historians have long noted that virulence, spread, and associated mortality of the Black Death differed from the pattern associated with later outbreaks of bubonic plague (see discussion in Web Appendix Section 2). These plague reoccurrences were caused either by local plague reservoirs or the repeated reintroduction of the bacteria from Asia (Schmid et al. 2015); their impact is studied by Biraben (1975) and Siuda and Sunde (2017).[12]

Though later outbreaks of the bubonic plague could devastate a city or region, in general morality was significantly lower than in the initial pandemic (1348–1352). Aberth observes that "the virulence of these recurrences of plague seems to have gradually declined" (Aberth 2009, 37). Europe-wide studies of later plague outbreaks focus on the extensive margin of

[11] In addition, Web Appendix Table A.2 (see Web Appendix Section 9.) shows that there is no correlation when market access is computed using a lower elasticity than 3.8, whether 2 or 1, in particular to reflect the fact that only luxury goods tended to be traded over longer distances in the medieval period, or when relying on Euclidean distance instead of network distance since the speed parameters used for the four transportation modes could also be misestimated.

[12] Dittmar and Meisenzahl (2018) use plague reoccurrences as an instrument for local shocks to the political economy of cities in Germany during the Reformation.

the plague and not on the intensive margin for which comprehensive estimates only exist for the Black Death.[13]

The Black Death affected all segments of the population:, rulers and commoners, rich and poor, adults and children, men and women. Neither the medical profession nor authorities were able to respond effectively. Medical knowledge was rudimentary: Boccaccio (2005, 1371) wrote that "all the advice of physicians and all the power of medicine were profitless and unavailing". Individuals, regardless of wealth, were unable to protect themselves. Institutional measures of prevention were nonexistent: the practice of quarantine was not employed until 20 years later.[14]

In summary, only two general statements can be made about variation in mortality rates: (i) The disease was more virulent initially, so towns that were randomly infected earlier should have higher mortality rates; and (ii) The disease spread from Messina initially. Therefore, we expect towns that were better connected to Messina—but not necessarily better connected overall—to have higher mortality rates.

*The economic role played by Jewish communities* In parts of Europe, Jewish communities had settled in Roman times; elsewhere their settlement dated back to the Carolingian period (768–888) or the tenth–eleventh centuries and thus predates our study by centuries. Among the full sample of 1869 towns, 363 of them had a Jewish community in 1347, i.e. almost 20% of towns. Jews tended to live in larger cities. The 124 towns of our main sample account for 66.7% of the total population of the 363 towns with a Jewish community in 1347. The most important economic role played by Jews at the time of the Black Death was as moneylenders, due to their high levels of human capital (Botticini and Eckstein 2012) and the restrictions on lending money at interest issued by the Church (Koyama 2010). Jews were also doctors and merchants. The economic value of Jewish communities was clearly perceived at the time (Chazan 2010).[15]

*The Black Death persecutions* The Black Death led to economic collapse as fields lay fallow and trade networks were disrupted. Prices rose due to this negative supply shock and this resulted in an immediate fall in incomes (Munro 2004).[16] The outbreak was accompanied by outbreaks of sexual and religious excess and by persecutions (Ziegler 1969; Gottfried 1983; Aberth 2009). Figure 5a plots the persecutions in our dataset between 1100 and 1600. Figure 5b focuses on the Black Death era (1347–1352) for our main sample. 58 out of the 124 towns—47% of them—saw a persecution (53 pogroms and 13 expulsions). The majority of persecutions took place in 1348–1349.

Note that Jews were the only ethnic and religious minority in most towns. Thus, with the exception of Narbonne where beggars were accused of having poisoned the wells, Jews were the only minority group persecuted during the Black Death period.

[13] The outbreak of the plague in northern Italy in 1630 was unusually severe as is studied by Alfani (2013).

[14] The term quarantine was first used in Ragusa, part of the Venetian empire in 1377 Gensini et al. (2004, 257).

[15] Historians observe that "rulers concerned with attracting Jews offered promises of security and economic opportunity to Jewish settlers" (Chazan 2010, 103). This also appears in many entries of the *Encyclopedia Judaica*.

[16] It was only years later that the labor shortage effect emphasized by Pamuk (2007) and Voigtländer and Voth (2013) was realized in rising wages. In addition, property rights were feudal and, as such, most property was owned by elites. While the Black Death did improve the bargaining position of peasants (e.g. Haddock and Kiesling 2002; Acemoglu and Robinson 2012), it took decades for contractual obligations to be renegotiated.

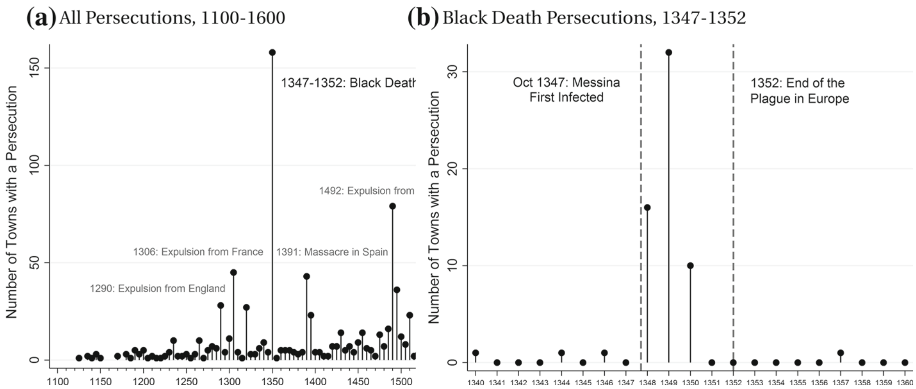


**Fig. 5** Total number of Jewish persecutions in 1100–1600 and 1347–1352. *Notes* Panel **(a)** depicts all persecutions recorded in the full sample of 1869 towns when the year is rounded to the nearest decade (year ending in 0) or mid-decade (year ending in 5). It shows that the Black Death period (1347–1352, rounded in 1350) witnessed the greatest number of persecutions in medieval European history (here, 1100–1600). Panel **(b)** focuses on the 124 towns of the main sample and on persecutions that took place within the Black Death period and 7 years before or after (here, 1340–1360). See Web Appendix for more details on data sources

## 4 The effect of Black Death mortality on Jewish persecutions

### 4.1 Specification and main results

We estimate a series of regressions based on:

$$P_{i,1347-52} = \alpha + \beta \mathrm{M}_{i,1347-52} + \varepsilon_i \ , \tag{1}$$

where $P_{i,1347-52}$ is a dummy equal to one if there is a persecution in town $i$ between 1347–1352, and $M_{i,1347-52}$ is the cumulative Black Death mortality rate (%) in town $i$ over the period 1347–1352.

As discussed in Sect. 3, the plague lasted on average 5 months in each town. Thus, mortality in 1347–1352 measures *cumulative* mortality over a period of a few months only (and not 5 years). Cumulative mortality was strongly correlated with *monthly* mortality, which increased quickly after the first infection before decreasing to 0. With a plague duration of 5 months, monthly mortality most likely peaked after 2 months. Most persecutions also took place during these 5 months. The cumulative mortality rate in 1347–1352 is thus a good proxy for the monthly mortality rate that people were facing at the time when a persecution occurred.

Row 1 of Table 1 presents our baseline result. The constant is 0.831*** which reflects the fact that, on average, there was a high probability of a persecution in 1347–1352. The effect of mortality on the persecution probability, however, is negative, at – 0.009***. This effect is strong: a one standard deviation increase in mortality (≈ 18.5% points) is associated with a 0.34 standard deviation reduction in the likelihood of a persecution (≈ 17.1% points). Figure 3b depicts this result non-parametrically. The persecution probability increases to about 0.8 when mortality reaches 16%, and then decreases to 0 as mortality increases to 80%.[17] Rows 2–4 then explore the long-run impact of the plague on persecutions. We find

[17] One town with a mortality rate of 0% did not persecute its community. However, since we use a bandwidth of 5% points of mortality for the local polynomial smooth plot, the mean persecution rate at the origin is 0.4.

**Table 1** Black Death mortality rates and Jewish persecutions, 1100–1600

| | | Mortality in 1347–1352 | | Constant | | Obs. | $R^2$ |
|---|---|---|---|---|---|---|---|
| *Dependent variable: dummy if any Jewish persecution in period* $[t-1; t]$ | | | | | | | |
| 1. | $[t-1; t] = [1347–1352]$ | – 0.009*** | [0.002] | 0.831*** | [0.104] | 124 | 0.12 |
| 2. | $[t-1; t] = [1353–1400]$ | – 0.004* | [0.002] | 0.404*** | [0.098] | 122 | 0.02 |
| 3. | $[t-1; t] = [1353–1500]$ | – 0.000 | [0.002] | 0.640*** | [0.099] | 124 | 0.00 |
| 4. | $[t-1; t] = [1353–1600]$ | 0.000 | [0.002] | 0.731*** | [0.088] | 127 | 0.00 |
| 5. | $[t-1; t] = [1341–1346]$ | 0.001 | [0.000] | – 0.004 | [0.004] | 122 | 0.01 |
| 6. | $[t-1; t] = [1321–1346]$ | – 0.001 | [0.001] | 0.144 ** | [0.068] | 126 | 0.01 |
| 7. | $[t-1; t] = [1300–1346]$ | – 0.001 | [0.002] | 0.255*** | [0.082] | 131 | 0.00 |
| 8. | $[t-1; t] = [1200–1346]$ | – 0.003 | [0.002] | 0.370*** | [0.090] | 132 | 0.02 |

This table shows the constant $\alpha_t$ and the effect $\beta_t$ of the Black Death mortality rate (%) in 1347–1352 on a dummy equal to one if there has been any persecution in various periods $[t-1; t]$, for the towns for which we have mortality data and in which we know that Jews were present in period $[t-1; t]$, without controls Robust SE's: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$. See Web Appendix for data sources

an effect that is half the size of our baseline coefficient in 1353–1400 (row 2) but no effect in the following centuries (rows 3–4).[18]

## 4.2 Investigating causality

Section 3 suggests that the intensity of the plague was not well explained by characteristics of the towns affected. We now provide further evidence that the impact of the Black Death was plausibly exogenous to other factors which may have affected the likelihood of a persecution during the period 1347–1352.

A downward bias is more problematic than an upward bias as we then overestimate the "protective" effect of the plague. The effect is biased downward if towns where persecutions would have occurred anyway during that period were also non-coincidentally affected by lower mortality rates. An upward bias is less of an issue since we then underestimate the protective effect. We discuss below several potential biases and identification strategies.

*Correlates of mortality* In Table 2, we show that mortality rates were uncorrelated with most town characteristics that could also have caused persecutions in 1347–1352. We define our town level characteristics according to whether they proxy for geography (1), trade and human capital (2) or institutions (3). While several variables are significant in columns (1)-(3), latitude and longitude are the only variables significantly associated with mortality once we include all the controls altogether (see (4), – 1.90** and 1.09** respectively). The plague indeed spread from the South-East and was initially more virulent due to epidemiological reasons. Measures of transportation and trade networks do not have positive significant effects.[19]

[18] Our finding is entirely consistent with that of Voigtländer and Voth (2012) who find that antisemitic attitudes can persist over centuries. Our results simply suggest that the direct "protective effect" of Black Death mortality on the likelihood of a persecution had dissipated several decades after the shock itself.

[19] The $R^2$ terms are not exactly equal to 0 when studying the correlation between mortality and trade and human capital (see (2), 0.12) or institutions (see (3), 0.15). This is because some of the variables are also correlated with latitude and longitude. For example, Roman roads were more dense in the South.

**Table 2** Town characteristics and Black Death mortality rates

| Dependent variable | Black Death Mortality rate (%, 1347–1352) | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | | (2) | | (3) | | (4) | |
| Average temperature 1500–1600 (d) | – 0.23 | [0.68] | | | | | 0.26 | [0.83] |
| Elevation (m) | – 0.01 | [0.01] | | | | | – 0.00 | [0.01] |
| Cereal suitability index | – 2.28* | [1.28] | | | | | – 2.52 | [1.52] |
| Pastoral suitability index | – 4.58 | [5.25] | | | | | 0.87 | [6.49] |
| Coast 10 Km dummy | – 6.67 | [4.98] | | | | | – 8.94 | [5.82] |
| Rivers 10 Km dummy | – 3.15 | [3.08] | | | | | – 4.26 | [3.85] |
| Latitude (d) | – 2.39*** | [0.58] | | | | | – 1.90** | [0.79] |
| Longitude (d) | 0.62** | [0.30] | | | | | 1.09** | [0.42] |
| Log town population in 1300 | | | – 1.41 | [1.23] | | | – 1.56 | [1.57] |
| Maj.Roman Rd (MRR) 10 Km dummy | | | 0.49 | [8.48] | | | – 4.74 | [6.30] |
| MRR intersection 10 Km dummy | | | 9.67* | [5.71] | | | 8.62 | [5.65] |
| Any Roman Rd (ARR) 10 Km dummy | | | 6.78 | [9.23] | | | 10.39 | [7.57] |
| ARR intersection 10 Km dummy | | | – 5.54 | [5.53] | | | – 2.15 | [5.49] |
| Medieval route (MR) 10 Km dummy | | | 1.07 | [4.42] | | | – 1.73 | [4.06] |
| MR intersection 10 Km dummy | | | – 3.04 | [5.14] | | | – 3.94 | [5.41] |
| Market and fair dummy | | | – 5.14 | [4.38] | | | – 0.97 | [5.16] |
| Hanseatic league dummy | | | – 1.03 | [6.30] | | | 7.20 | [6.88] |
| Log market access in 1300 | | | 0.51 | [1.00] | | | – 0.07 | [1.06] |
| Aqueduct 10 Km dummy | | | 3.19 | [4.22] | | | – 0.33 | [4.66] |
| University dummy | | | 3.88 | [6.47] | | | 4.43 | [7.02] |
| Monarchy in 1300 dummy | | | | | 3.18 | [5.44] | 6.85 | [5.51] |
| State capital in 1300 dummy | | | | | 7.06 | [6.43] | 2.01 | [7.45] |
| Parliamentary activity in 1300–1400 | | | | | 5.08 | [4.66] | – 0.32 | [4.59] |
| Log distance to nearest parliament | | | | | 4.49** | [1.95] | 0.59 | [2.09] |
| Self-governing city in 1300 dummy | | | | | – 5.30 | [4.04] | 2.04 | [4.38] |
| Battle w/i 100 Km in 1300–1350 dummy | | | | | – 3.59 | [3.85] | – 6.48 | [4.31] |
| Obs.; $R^2$ | 124; 0.27 | | 124; 0.12 | | 124; 0.15 | | 124; 0.36 | |

Each column is a separate regression. The variables in (1), (2) and (3) proxy for physical geography, trade and human capital, and institutions, respectively

Robust SE's: $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. See Web Appendix for data sources

*Parallel trends* Rows 5–8 in Table 1 show that mortality is not associated with persecution probability in the period prior to the Black Death, whether we use: 1341–1346 (6 years), 1321–1346 (25 years), 1300–1346 or 1200–1346. Parallel trends suggest fixed effects regressions would give us similar results. Because the Black Death lasted 6 years, we create 10 6-year periods $t$ both before 1347 (1297–1302, …, 1341–1346) and after 1352 (1353–1358, …, 1398–1403). For 172 towns $i$ that had Jews at one point in 1297–1403 and for which we know the mortality rate, we regress a dummy if there was a persecution in town $i$ in period $t$ on mortality in the same town and period, for the town-periods where Jews were present. Including town fixed effects and period fixed effects, and posit mortality is nil outside 1347–1352, the baseline effect remains unchanged.[20]

Another way to examine parallel trends is to study whether cities above and below mean or median mortality (39.3 and 40.5%, respectively) had different persecution rates before the Black Death. Since we focus on a period of 6 years (1347–1352), we divide the 999–1346 period into 58 periods of 6 years and plot in Web Appendix Figures 5(a)-5(b) the average persecution rates for both cities "above" and cities "below" the mean/median. This confirms that there were not differential pre-trends in persecution between the two groups.

*Size of Jewish community* The fact that we capture Jewish presence via a dummy could bias our estimates. If mortality is lower in towns with larger communities and the probability of a persecution is higher in towns with larger communities, simply because they are larger, then this creates a downward bias. Conversely, if mortality is higher in towns with larger communities and persecutions are less likely in such towns because larger communities are better able to defend themselves against potential persecutions, this also creates a downward bias.

Jews may have been less exposed if they had better hygiene practices or lived in isolated areas within towns. Towns with more Jews should then have had lower aggregate mortality rates. At the same time, Jewish areas were often overcrowded as they had to live in the few streets reserved for them. If this was the case, towns with more Jews could have had higher mortality rates.

These biases are unlikely to be significant for the following reasons. First, it is unlikely that Jews experienced differential mortality. The plague was mostly bubonic, which limited the role that characteristics of the community could play.[21] Second, Jews comprised a small share of the population, minimizing the effect of any differential mortality between Jews and non-Jews on town mortality. For 30 towns for which we have information on the size of their community, the median population share of Jews was 6.3%. Third, Table 3 shows that results hold if we add proxies for the size of the community and the organization of the town (rows 2–6). Row 1 reproduces the baseline results (− 0.009***). Row 2 drops the towns for which we know that Jews accounted for more than 5% of the population. Row 3 adds dummies for whether the town had a Jewish cemetery/quarter/synagogue circa 1347. Communities with a Jewish cemetery and a synagogue were larger, whereas towns with a Jewish quarter were segregated. Row 4 controls for the first year Jews were present and the

[20] See Web Appendix Table A.3 and Web Appendix Section 9). This result holds even if we drop the 10 post-1352 periods since there were plague reoccurrences after the Black Death and consistent data does not exist on the specific mortality rate associated with each reoccurrence.

[21] For example, in Marseille, the mortality rate of the Jewish population was 50% during the Black Death versus 55% for the whole city (Sibon 2011). In Avignon, it is reported that "the Jewish population was extremely hard hit by the plague" (Deaux 1957, 103–104). The argument that the Jewish practice of ritual bathing would lead to lower mortality rates is not well grounded as ritual baths often used stagnant water (see van Straten 2007, 47) and, contrary to common mythology, bathing was common among the Christian populations of medieval Europe.

**Table 3** Mortality rates and persecutions, investigation of causality

| | | Mortality 1347–1352 | | Constant | | Obs. | $R^2$ |
|---|---|---|---|---|---|---|---|
| *Dependent variable: dummy if any Jewish persecution in 1347–1352* | | | | | | | |
| 1. | Baseline (Row 1 of Table 1) | −0.009*** | [0.002] | 0.831*** | [0.104] | 124 | 0.12 |
| 2. | Drop if Jews ≥ 5% of town population | −0.008*** | [0.002] | 0.779*** | [0.113] | 107 | 0.09 |
| 3. | Controls for Jewish cemetery, quarter and synagogue | −0.009*** | [0.002] | 0.808*** | [0.117] | 124 | 0.15 |
| 4. | Controls for years of first entry and last reentry | −0.009*** | [0.002] | 0.814*** | [0.197] | 124 | 0.13 |
| 5. | Control for Jewish centrality index | −0.009*** | [0.002] | 0.757*** | [0.197] | 124 | 0.14 |
| 6. | Keep if year of last entry is after 1290 | −0.007* | [0.004] | 0.823*** | [0.179] | 36 | 0.07 |
| 7. | Drop if known number of victims | −0.009*** | [0.002] | 0.775*** | [0.111] | 115 | 0.11 |
| 8. | Dummy if persecution in 1321–1346 | −0.009*** | [0.002] | 0.812*** | [0.107] | 124 | 0.12 |
| 9. | Dummy if persecution in 1300–1346 | −0.009*** | [0.002] | 0.816*** | [0.108] | 124 | 0.12 |
| 10. | Control for number of persecutions in 1321–1346 | −0.009*** | [0.002] | 0.812*** | [0.107] | 124 | 0.12 |
| 11. | Control for number of persecutions in 1300–1346 | −0.009*** | [0.002] | 0.836*** | [0.106] | 124 | 0.12 |
| 12. | Drop if Jewish presence inferred from persecution | −0.008*** | [0.002] | 0.767*** | [0.108] | 114 | 0.10 |
| 13. | Drop top and bottom 25% in mortality | −0.015** | [0.007] | 1.049*** | [0.295] | 71 | 0.06 |
| 14. | Drop if natural baths or response | −0.009*** | [0.002] | 0.812*** | [0.106] | 121 | 0.11 |
| 15. | All controls of Table 2 | −0.005* | [0.003] | 1.373 | [1.344] | 124 | 0.43 |
| 16. | All controls Excl. Lat., Long. and Temperature | −0.006** | [0.002] | 1.148*** | [0.308] | 124 | 0.41 |

Robust SE's: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. See Web Appendix for data sources last year of reentry in the town before 1347 as towns that had Jews for a long time may have had a larger community. Row 5 controls for a Jewish centrality index that measures to what extent the town is surrounded by other towns with a community. This indicates whether Jews are well-established in the region.[22] Row 6 restricts the sample to towns where Jews settled after 1290, the year Jews were expelled from England, since their resulting community size might be more exogenous. The effect is weaker as sample size decreases to 36.

Similarly, Web Appendix Table A.5 shows results hold if we focus on towns where the community was likely large (small): (i) towns with (without) a Jewish cemetery/quarter/synagogue; (ii) towns where the year of first/last entry is below (above) the median year in the sample; (iii) and towns with a Jewish centrality index above (below) the median in the sample.

In addition, Web Appendix Table A.6 shows that mortality was not correlated with these proxies for the size of the community, thus minimizing this concern. Likewise, for 172 cities with mortality data and that do not belong to the British Isles or Scandinavia, we find no correlation between mortality and whether there was a Jewish community in the town. We exclude these regions because there was a blanket ban on the presence of Jews for their whole territory.

A second, related, question is whether our mortality rates fail to include the deaths due to persecutions, thus mechanically causing an upward bias. However, an upward bias is less concerning for us. Moreover, Jews generally comprised a small population share of the towns. In row 7, we drop 8 towns for which we know that a significant number of Jews were persecuted.[23]

Third, towns with more persecutions in the past may have had a smaller community by 1347. Furthermore, towns with a history of persecutions may have had more antisemitism, which would then raise the Black Death persecution rate. Conversely, towns with more persecutions in the past may have been less likely to persecute their community during the Black Death if there were fewer Jews left to persecute. In rows 8–11 we control for previous persecutions using a dummy indicating any persecution or the number of years with a persecution, whether one generation (1321–1346, $\approx$ 25 years) or two generations (1300–1346, $\approx$ 50 years) before. Row 12 then shows that results hold if we drop 10 cities for which information on Jewish presence circa 1347 was directly inferred from information on the occurrence of a persecution.

*Presence of Jewish community* We focus on towns with a Jewish community since, by construction, there cannot be a persecution without a community. However, if Jewish presence is better measured in towns with high mortality and low persecution, or low mortality and high persecution, our results may be spurious. In our sample, there is only one town where Jews entered after 1347. Web Appendix Table A.5 (see Web Appendix Section 9.) shows that results hold if we drop that city and rely only on towns with (i) a Jewish cemetery/quarter/synagogue, (ii) whose year of first entry/last reentry in the town was below the median in the sample, and (iii) whose Jewish centrality index was above the median.

22 For town $i$, and other towns $j \in J$ (363 towns with a Jewish community circa 1347) or $j \in A$ (all 1869 towns), the Jewish centrality index is equal to $\sum_{j \in J} D_{ij}^{-\sigma} \div \sum_{j \in A} D_{ij}^{-\sigma} * 100$ with $D_{ij}$ the travel time between city $i$ and city $j$. If all surrounding towns have a Jewish community, it will be close to 100, and 0 otherwise.

23 We do not know how many Jews were persecuted in other towns. However, the fact that we could not find the number of victims for these other persecutions suggests that the numbers were not as high as for the 8 towns dropped as persecutions involving more victims should have been better documented than persecutions with fewer victims.

*Outliers* In row 13, we drop the towns with the 25% highest and the 25% lowest mortality rates to ensure that our results are not driven by outliers that may have had high, or low, mortality rates for specific reasons. In general, no community was prepared to deal with the Black Death. It was attributed to the "vengeance of God" or the "'conjunction of certain stars and planets" (Horrox 1994, 48–49). Therefore, there was little variation in a town's ability to deal with the plague. Historians report that some towns had either natural baths (Nuremberg, Baden-Baden) or tried to take action in response to the plague (Venice). Results hold when we drop these towns (row 14).

*Controls* A downward bias is possible if any of the characteristics of Table 2 increases (decreases) mortality and decreases (increases) the probability of a persecution. For example, if being on a trade route was positively correlated with mortality and negatively correlated with a town's propensity to persecute, this would be a source of downwards bias. Alternatively, if being located in rugged terrain made a town less susceptible to the plague but more likely to persecute Jews, this would be a source of upwards bias. Table 2 has shown, however, that mortality is not significantly correlated with most characteristics. Only latitude and longitude have any relationship since the plague came from the South-East and was more virulent initially. In row 15, we include all our control variables in the regression. However, with only 124 observations, including 27 controls reduces the degrees of freedom. In addition, as mortality is correlated with latitude and longitude, there is a risk of overcontrolling when adding all controls. The size of our coefficient remains significant though smaller in size (− 0.005*). When not controlling for latitude and longitude (and temperature whose correlation with latitude is 0.73), the point estimate is − 0.006** (row 16). Note that we still obtain significantly negative effects when adding dummies for the cultural and linguistic areas and when we include a dummy for the Holy Roman Empire, where fragmented political authority may have influenced the probability of a persecution (Finley and Koyama 2018) (see Web Appendix Table A.4).

*IV1: proximity to Messina* Market access to Messina should predict plague virulence, since the plague was more virulent initially. We construct an instrumental variable based on log market access to Messina (see Fig. 6a), *conditional* on a town's log market access to all 1869 towns (see Fig. 6b). Market access to Messina $m$ for town $i$ is defined as $MA_{im} = \Sigma(L_m \div \tau_{im}^{\sigma})$, with $L_m$ being the population of Messina in 1300, $\tau_{im}$ the computed travel time between town $i$ and Messina, and $\sigma = 3.8$.[24] Controlling for market access to all 1869 towns captures the fact that some towns were better connected overall, and may have thus been more likely to receive infected rats/fleas. Hence, we exploit the fact that it was the specific connectedness to Messina, and not connectedness overall, that mattered for mortality.[25]

The exclusion restriction is satisfied as long as network proximity to Messina is uncorrelated with factors that directly influenced the persecution probability. Sicily was an urbanized and commercialized part of Europe but its economy resembled that of its Mediterranean neighbors (Epstein 1992). Sicily did not treat its Jewish population differently from the rest of Europe (Backman 1995, 148–150). Messina was not a particularly important city. For example, it was only the 45th largest city in our full sample in 1300. There was no reason for distance to Messina to be correlated with Jewish community characteristics. Jews settled in Europe long before the Black Death, and with the exception of the British Isles and Scandinavia, most states had Jewish communities at the onset of the Black Death. It is thus

24 Note that we log market access because it is not bounded whereas mortality and persecution probability are.

25 Note that the correlation between the two market access measures is indeed lower than 1, at 0.51, since Northern European towns also had access to large towns other than Messina.

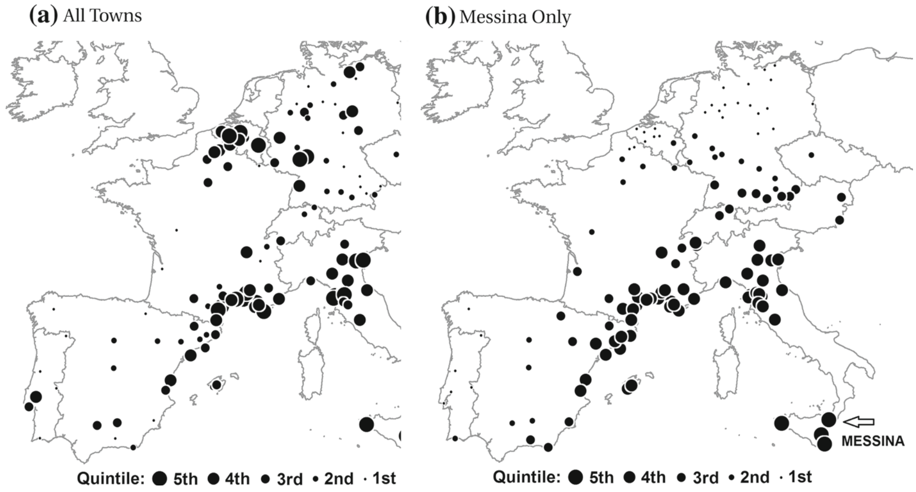


**Fig. 6** Market access to all towns versus market access to Messina only, 1300. *Notes* Panel **(a)** shows for the 124 towns of the main sample their log market access to all towns in 1300. Panel **(b)** shows for the same 124 towns their log market access to Messina in 1300. See notes under Fig. 4b for details on how market access is calculated. We use as an instrument log market access to Messina, *conditional* on log market access to all 1869 towns in Western Europe. See Web Appendix for more details on data sources

reasonable to believe that network proximity to Messina should not be directly correlated with persecution probability.

As shown in row 2 of Table 4, the first stage of this instrument is strong (F: 31.0, coeff.: 4.42***). The second stage yields a coefficient estimate that is negative and similar in magnitude to our baseline estimate of row 1 (− 0.016***, not significantly different from − 0.009***). To ensure that our instrument is not picking up additional unobservables we control for latitude and longitude and latitude and longitude squared (row 3). The first stage estimate is weaker (F: 4.3, coeff.: 3.46**) but we still obtain a negative coefficient that is not significantly different from − 0.009*** (Web Appx. Table A.7 shows the full 1st-stage regressions). In row 7, we show the reduced-form effect of the IV—log market access to Messina in 1300—on persecution probability, but then verify that the IV is unrelated with persecution probability in the preceding generation(s) (1321–1346 and 1300–1346, see rows 9 and 11). In Web Appendix Table A.8, we also verify that this IV is not correlated with the log growth rate of town population in 1200–1300, conditional on log market access to all towns and log initial population in 1200 (to control for mean reversion). In Web Appendix Table A.9, we then show that the IV effects hold if we simultaneously control for all proxies for Jewish community size and past persecutions (cemetery, quarter, synagogue, years of first and last entry, Jewish centrality index, and dummies if persecution in 1321–1346 and 1300–1346).

Despite the fact that we control for market access to all towns, one might worry that market access to Messina is correlated with market access to other cities, and that better connectedness to these is correlated with persecution probability. Web Appendix Table A.9 shows results hold if we control for log market access to (i) Genoa and (ii) cities in the Middle-East and North Africa (MENA). Kaffa was a trading colony established by Genoa on the Black Sea (Epstein 1996, 143). The ships carrying the plague from Kaffa were bound to Genoa, and stopped in Messina because sailors were sick. By controlling for market access

**Table 4** Mortality rates and persecutions, IV strategies

| | ("F" = First stage Kleibergen-Paap F; "1st" = First stage effect) | Mortality 1347–1352 | | Constant | | Obs. | $R^2$ |
|---|---|---|---|---|---|---|---|
| *Dependent variable: dummy if any Jewish persecution in 1347–1352* | | | | | | | |
| 1. | Baseline (Row 1 of Table 1) | −0.009*** | [0.002] | 0.831*** | [0.104] | 124 | 0.12 |
| 2. | IV1: Log MA Messina, Ctrl for Log MA (F: 31.0; 1st: 4.42***) | −0.016*** | [0.005] | 1.134*** | [0.184] | 123 | 0.51 |
| 3. | IV1 + Lat., Long. and their Sq. (F: 4.3; 1st: 3.46**) | −0.023* | [0.014] | 8.356* | [4.606] | 123 | 0.44 |
| 4. | IV2: #Months b/w Oct 1347 and 1st Inf. (F: 33.3; 1st: −0.87***) | −0.028*** | [0.006] | 1.567*** | [0.240] | 124 | 0.28 |
| 5. | IV2 + Lat., Long. and their Sq. (F: 7.3; 1st: −0.83***) | −0.029** | [0.015] | 6.116 | [5.522] | 124 | 0.29 |
| 6. | IV1 + IV2 + Lat., Long. and Sq. (F: 4.4) | −0.019* | [0.011] | 8.652** | [4.239] | 123 | 0.51 |
| 7. | Reduced-form of Log MA to Messina, Ctrl for Log MA | −0.071*** | [0.002] | −0.192 | [0.200] | 123 | 0.09 |
| 8. | Reduced-form of #Months b/w Oct 1347 and 1st Inf. | 0.024*** | [0.004] | 0.113 | [0.069] | 124 | 0.23 |
| *Dependent variable: dummy if any Jewish persecution in 1321–1346:* | | | | | | | |
| 9. | Reduced-form of Log MA to Messina, Ctrl for Log MA | −0.001 | [0.007] | 0.061 | [0.076] | 121 | 0.00 |
| 10. | Reduced-form of #Months b/w Oct 1347 and 1st Inf. | −0.002 | [0.002] | 0.108** | [0.047] | 122 | 0.01 |
| *Dependent variable: dummy if any Jewish persecution in 1300–1346:* | | | | | | | |
| 11. | Reduced-form of Log MA to Messina, Ctrl for Log MA | −0.003 | [0.013] | 0.103 | [0.136] | 121 | 0.01 |
| 12. | Reduced-Form of #Months b/w Oct 1347 and 1st Inf. | −0.004 | [0.004] | 0.207*** | [0.007] | 122 | 0.02 |

"F" = First stage Kleibergen-Paap F. "1st" = First stage effect

Rows 2–3 instrument mortality by log market access (MA) to Messina, controlling for log MA to all 1869 towns (IV1). Rows 4–5 instrument mortality by the number of months between Oct. 1347 and the month of first infection in the town (IV2). See Web Appendix Table A.7 for the full 1st-stage regressions. Rows 9–12: Reduced-form effects of the IVs in 1347–1352, 1321–1346 and 1300–1346, respectively

Robust SE's: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$. See Web Appendix for data sources

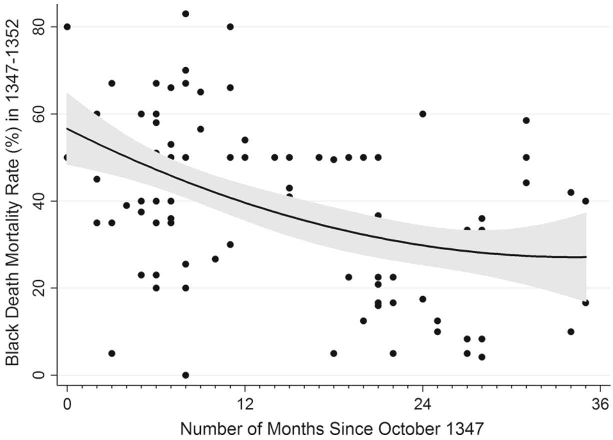


**Fig. 7** Timing of the onset of the Black Death and Black Death mortality. *Notes* This figure shows for the sample of 124 towns the relationship between cumulative Black Death mortality rates (%) in 1347–1352 and the timing of the onset of the Black Death in the town. Number of months is measured since October 1347, the date Messina—the port of entry of the Black Death in Europe—was infected. Towns infected earlier had higher mortality rates (Y = 52.01*** − 0.87*** X; $R^2 = 0.22$). See Web Appendix for more details on data sources

West of Italy was a wealthy area of Europe then, and we exploit the fact that the ships stopped in Messina and not a different city.[26] We control for market access to the MENA using information on the five largest MENA cities in 1300 according to Chandler (1987). Lastly, a more realistic measure of market access would account for variation in local tariffs and currencies. Counting these is an impossible task as the vast majority of these are not recorded by historians. In any case, these local tariffs were likely endogenous to local town characteristics, so including them in our analysis could introduce additional bias.[27] Instead, Web Appendix Table A.9 shows that results hold if we use an ahistorical measure of market access as our instrument, more precisely the log Euclidean distance to Messina, conditional on average log Euclidean distance to all 1869 towns.[28]

*IV2: month of first infection* A related source of exogenous variation in mortality is timing of first infection, not just because of proximity to Messina, but also because there was randomness in the local patterns of the plague, depending on where infected rats/fleas went. Figure 7 plots mortality rates against the date that the town was first infected (n. of months since October 1347). Towns infected later, indeed, had lower mortality rates. Using the number of months since October 1347 as an IV, the coefficient is − 0.028*** (row 4 of Table 4, F: 33.3, 1st-stage coeff.: − 0.87***). This is precisely estimated, however, this estimate is also significantly larger than our OLS coefficient (− 0.009). In row 5, we also control for latitude and longitude and latitude and longitude squared in order to rely more on the random component of the spread of the plague. The F-stat decreases to 7.3 (1st-stage coeff.: − 0.83***, see Web Appendix. Table A.7 for the full 1st-stage regressions), and the coefficient is less precisely estimated (not significantly different from − 0.009***). In row 8, we show the reduced-form effect of the IV on persecution probability, but then verify that

[26] The correlation between log market access to Messina and log market access to Genoa is lower than 1, at 0.69.

[27] We also use $\sigma = 3.8$ (from Donaldson (2018)), which implies a high cost of distance on trade.

[28] Results also hold if market access to all towns exclude Messina (Web Appendix Table A.7A.9).

the IV is unrelated with persecution probability in the preceding generation (s) (1321–1346 and 1300–1346, see rows 10 and 12).[29] We combine IV1 and IV2 in row 6. The effect is − 0.019* (F-stat of 4.4, not significantly different from − 0.009).

However, note that the year–month of first infection is imputed for 29 towns. This could create measurement error which, if endogenous, could question the exclusion restriction. In Web Appendix Table A.10, we verify that cities for which we impute the dates are not different in terms of mortality, persecution probability, and centrality (since we sometimes impute their date of infection using the information from neighboring towns). Besides, Web Appendix Table A.11 shows that the IV2 results hold when using non-imputed dates for 95 towns only.

*Plague reoccurrences* The plague reoccurred intermittently over the next centuries. Using data from Schmid et al. (2015) based on Biraben (1975), we study the relationship between plague reoccurrence and the persecution of Jews. We focus on 415 towns with Jews at one point in 1353–1598. Since the Black Death lasted 6 years, we create 41 6-year periods $t$ (1353–1358, …, 1594–1598). We then regress a dummy if there was a persecution in town $i$ in period $t$ on the number of years with a plague outbreak within 5 km, or 100 km, from the town during the period, for the town-periods where Jews were present. We include town fixed effects and period fixed effects. Note that we also use outbreaks within 100 km because Biraben recorded plague outbreaks for larger cities only (Roosen and Curtis 2018), hence the need to rely on local buffers around each city. There is also no data on the mortality rate associated with each outbreak, so our measure is very imperfect. Nonetheless, Web Appendix Table A.12 shows there is a negative effect on plague reoccurrences on persecution probability (− 0.005*** when using 100 km; − 0.006 ($p$ value: 0.23) when using 5 km). However, given the limitations of this analysis and the fact that plague reoccurrences are possibly endogenous, we do not pursue it further.

*Distribution of rats* The Black Death was transmitted by the fleas of the black rat (*Rattus rattus*). However, their populations began to decline after the introduction of the brown rat (*Rattus norvegicus*) in the eighteenth century (Christakos et al. 2005), which coincided with the end of plague reoccurrences in Europe. Note that little is known about the geographical distribution of black rats at the time of the Black Death, and we cannot infer it from the distribution of brown rats today (for which little data also exist). We thus do not use rats for identification purposes.

*Summary* Our analysis of town characteristics, parallel trends, Jewish community size, outliers, controls, and IV strategies suggest there is a plausibly causal negative effect of mortality on persecution probability. The identification strategies suggest effects that range between about − 0.006 and − 0.019, but are not significantly different from our baseline OLS effect of − 0.009. In the rest of the analysis we will thus employ the intermediary OLS estimates as our baseline.

## 4.3 Robustness

*Preventive persecutions* Historians note that some communities heard accusations that the Jews were poisoning the wells and killed their Jews prior to any outbreak of plague (Ziegler 1969, 103). These persecutions raise several problems for our analysis. First, our hypothesis is that people and authorities observe other people are dying and then decide to persecute

[29] Web Appendix Table A.8 shows that this IV is not correlated with town population growth in 1200–1300.

their community. If most of the persecutions in our sample were preventive persecutions, our results would be coincidental. Second, preventive persecutions were likely to occur in the later years of the Black Death (1347–1352). But places infected later also had lower mortality rates.

Table 5 considers these issues. Baseline results are shown in row 1. We first add a dummy for towns which were first hit by the plague after September 1348 as they are the towns liable to preemptively persecute their Jews (row 2). Indeed, September 1348 is the month when Jews were tortured at the Castle of Chillon in Switzerland and forced to admit to poisoning wells which, in turn, started the rumor that Jews had caused the disease (Horrox 1994, 68). In the analysis below the 10 possible preventive persecutions in our sample all took place after September 1348.

Second, we control for the log distance to Chillon and its interaction with the post-September 1348 dummy in order to control for the effects of the diffusion of the rumor from Chillon after that month (row 3). Next, we add a control for the log distance to the first ten towns to be warned by letter of a Jewish conspiracy and its interaction with the post-September 1348 dummy in order to control for the effects of the diffusion of the rumor (row 4). We then include a control for the log distance to the Rhine and its interaction with the post-September 1348 dummy as the rumors also diffused along the Rhine (row 5). Lastly, in row 6 we control for the log distance to the path of the Flagellant movement and its interaction with the post-September 1348 dummy as they are alleged to have massacred Jews in their attempts to ward off the plague (Nohl 1924). None of these factors significantly affect our estimates (which decrease but retain their precision).[30]

Third, we investigate if there are persecutions that preceded the plague itself. Although our analysis is for the year 1347–1352, Fig. 7 shows that all 124 towns were first infected within 3 years of October 1347, so before September 1350. All persecutions also took place during that period (see Fig. 5b). More detailed analysis then reveals that there are five towns where it is likely (N = 3) or possible (N = 2) that a persecution preceded the plague. In the three "likely" towns, it appears that the persecutions took place in 1349, but the Black Death did not hit before 1350. Though it is possible that these were not preventive persecutions if the date of infection (or persecution) was measured with error. In the two "possible" towns, we cannot be sure if the persecution took place either during *or* in the month preceding the month of first infection. Given these uncertainties, we keep these five towns in our sample. We nonetheless verify in rows 7 and 8 that results are unchanged when dropping them. In addition, if they were truly preventive persecution, their "true" mortality rate should have been 0 at the time of the decision to persecute. However, replacing their mortality rate by 0% in the regression does not change the estimated coeffcient (see row 9, − 0.009***). Therefore, whether we include these towns or not is inconsequential. Finally, we expect preventive persecutions to have occurred later. We find that the effect is weaker but still significant if we include three time fixed effects for whether the town was first infected in the first, second or third year after October 1347 (see row 10). However, if we limit our analysis to the 95 towns for which the date of first infection was not imputed, the effect remains as strong as the baseline effect (see row 11).

*Alternative outcomes* In Table 6, we explore whether there were differences in the intensity of persecution. Our results are stronger for pogroms than for expulsions (− 0.007*** in row 2 vs. − 0.004** in row 3, significantly different at 5%). The fact that the effect is larger for pogroms may reflect the fact that pogroms involved the killing of Jews. Expulsions, by contrast, were more orderly events and often involved the Jews being expelled from a city

30 Results hold if we use network distances instead of Euclidean distances (results available upon request).

**Table 5** Mortality rates and preventive persecutions

| | | Mortality 1347–1352 | | Constant | | Obs. | $R^2$ |
|---|---|---|---|---|---|---|---|
| *Dependent variable: dummy if any Jewish persecution in 1347–1352* | | | | | | | |
| 1. | Baseline (Row 1 of Table 1) | −0.009*** | [0.002] | 0.831*** | [0.104] | 124 | 0.12 |
| 2. | Add dummy if year-month of infection ≥ Sept. 1348 | −0.007*** | [0.002] | 0.601*** | [0.138] | 124 | 0.17 |
| 3. | Row 2 + Log Dist. to chillon + dummy × Log Dist. | −0.007*** | [0.002] | −0.251 | [0.899] | 124 | 0.23 |
| 4. | Row 2 + Log Dist. to letters + dummy × Log Dist. | −0.005** | [0.003] | 0.720 | [0.410] | 124 | 0.26 |
| 5. | Row 2 + Log Dist. to rhine + dummy × Log Dist. | −0.005** | [0.003] | −0.972 | [0.773] | 124 | 0.25 |
| 6. | Row 2 + Log Dist. to flagellants + dummy × Log Dist. | −0.005** | [0.002] | 0.137 | [0.139] | 124 | 0.30 |
| 7. | Drop if likely preventive based on year-month | −0.009*** | [0.002] | 0.806*** | [0.107] | 121 | 0.11 |
| 8. | Row 7 + drop if possibly preventive based on year-month | −0.009*** | [0.002] | 0.788*** | [0.109] | 119 | 0.10 |
| 9. | Replace mortality = 0% if likely or possibly preventive | −0.009*** | [0.002] | 0.829*** | [0.09] | 124 | 0.14 |
| 10. | Fixed effects if first infection in 1st, 2nd or 3rd year | −0.005** | [0.002] | 0.513*** | [0.139] | 124 | 0.21 |
| 11. | Row 10 using non-imputed dates of infection only | −0.009*** | [0.002] | 0.725*** | [0.155] | 95 | 0.30 |

Row 2: We add a dummy if the town was infected from Sept. 1348. Rows 3–6: We also control for the log Euclidean distance to Chillon, towns that first received letters warning them of the conspiracy, the Rhine, and the path of the flagellants, and their interaction with the post-Sept. 1348 dummy. Rows 7–9: We drop towns where it is likely/possible that the persecution preceded the Black Death, or replace their mortality rate by 0%. Rows 10–11: Adding three fixed effects for whether the town was first infected in the first year, second year or third year after October 1347

Robust SE's: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. See Web Appendix for data sources

**Table 6** Mortality and persecutions, alternative outcomes

| Dependent variable: dummy if … | | Mortality 1347–1352 | | Constant | | Obs. (sample) | $R^2$ |
|---|---|---|---|---|---|---|---|
| 1. | Baseline: persecution (N = 58) | – 0.009*** | [0.002] | 0.831*** | [0.104] | 124 (all) | 0.12 |
| 2. | Pogrom (N = 53) | – 0.007*** | [0.002] | 0.720*** | [0.105] | 124 (all) | 0.08 |
| 3. | Expulsion (N = 13) | – 0.004** | [0.001] | 0.244*** | [0.077] | 124 (all) | 0.05 |
| 4. | Expulsion or annihilation (N = 32) | – 0.009** | [0.004] | 0.837*** | [0.137] | 58 (persecution) | 0.08 |
| 5. | Annihilation (N = 19) | – 0.008* | [0.004] | 0.694*** | [0.166] | 45 (pogrom only) | 0.07 |
| 6. | Burning (N = 8) | – 0.003 | [0.002] | 0.178 | [0.108] | 45 (pogrom only) | 0.02 |
| 7. | Mob involved (N = 11) | – 0.010*** | [0.003] | 0.556*** | [0.160] | 45 (pogrom only) | 0.15 |
| 8. | Annihilation, burning or mob (N = 28) | – 0.010** | [0.004] | 0.844*** | [0.157] | 45 (pogrom only) | 0.11 |
| 9. | Persecution + successful prevention (N = 61) | – 0.009*** | [0.002] | 0.857*** | [0.103] | 124 (all) | 0.12 |
| 10. | Persecution − failed prevention (N = 50) | – 0.009*** | [0.002] | 0.751*** | [0.103] | 124 (all) | 0.11 |
| 11. | Any Attempt to Prevent (N = 11) | 0.003 | [0.003] | 0.075 | [0.086] | 61 (row 9) | 0.02 |
| 12. | Drop Top 25% in mortality (Mort. ≥ 50%) | – 0.009*** | [0.003] | 0.835*** | [0.128] | 101 (all) | 0.08 |
| 13. | Keep if Pop. 1300 < Median (11,000) | – 0.007** | [0.003] | 0.844*** | [0.142] | 61 (all) | 0.09 |
| 14. | Keep if Pop. 1300 ≥ Median (11,000) | – 0.013*** | [0.003] | 0.872*** | [0.158] | 63 (all) | 0.19 |
| 15. | Keep if residual Pop. < Median (5600) | – 0.007** | [0.003] | 0.812*** | [0.155] | 62 (all) | 0.08 |
| 16. | Keep if residual Pop. ≥ Median (5600) | – 0.015*** | [0.004] | 0.959*** | [0.156] | 62 (all) | 0.24 |

Row 4: The annihilation dummy is equal to one if all Jews are killed. Row 6: The burning dummy is equal to one if at least one Jew is burned. Row 7: The mob dummy is equal to one if the persecution is initiated by a mob. Rows 9–10: We know if the local authority tried to prevent a persecution, and succeeded in doing so. Rows 13–16: We restrict the sample to towns for which the population in 1300 or the estimated residual population in the immediate aftermath of the Black Death was below or above the median in the sample of 124 towns. Robust SE's: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$. See Web Appendix for data sources

into the nearby countryside from which they could return. In this case, if a higher mortality rate increases the economic value of Jews, a town may prefer to expel its community instead of killing its members.

Among the 58 towns with a persecution we investigate in row 4 if the persecution led to Jews being expelled or all Jews being killed ("Annihilation"). We find a negative effect of mortality on the probability of an exit. This implies that if the mortality rate is high the populace and/or the authorities prefer to kill a few Jews rather than losing its entire Jewish community. This is in line with our baseline result that towns persecute Jews "less" at higher mortality rates.

Among the 45 towns with a pogrom, we study if the pogrom was particularly violent, for example whether the community was annihilated (row 5), whether some Jews were burned alive (row 6), or whether the pogrom was perpetrated by a mob (row 7). We find that a higher mortality rate led to a lower "intensity" of the pogrom, as it reduces the likelihood of an annihilation (row 5, −0.008*) and a mob being involved (row 7, −0.010***). We find a negative, but not significant, effect for burning. Lastly, pogroms were "less" violent when mortality was high (row 8, −0.010**).

Lastly, we know if the local authorities attempted to prevent a persecution. Row 9 shows that results hold if we consider as a persecution any persecution that was successful or unsuccessful. Row 10 shows results hold if we consider as a persecution only the persecutions where no prevention attempt was made. In row 11, among the 61 towns with a successful or unsuccessful prevention, we show that there is a positive but not significant correlation between a dummy equal to one if there has been any attempt to prevent the persecution and mortality.

*External validity* There could be concerns that our sample size is small. In row 1 of Table 7, we use as regression weights population in 1300. By doing so, less weight is placed upon small towns. The effect becomes stronger. Given that our 124 towns are capturing 66.7% of the total population of the 363 towns with a Jewish community, having more towns would not change results (unless they are very large). We also use several, albeit imperfect, methods to obtain mortality estimates for more towns. First, the effect remains similar if we use the mortality rate of the closest neighboring town with mortality data if this town is within 100 km (row 2, N = 307). Second, we create estimates of mortality based on spatial extrapolation.[31] In row 3, we use these extrapolated estimates to show that our analysis holds for the full sample of 363 communities. Third, Christakos et al. (2005) report mortality rates for selected provinces (e.g., "Languedoc"). When we attribute to each town whose mortality is missing the rate of its province, the effect is similar (row 4, N = 159).[32]

Since the 124 towns represent 66.7% of the total population of the 363 towns with a Jewish community, one could object that our sample is biased towards large towns among the group of towns with a community. Kernel distributions of 1300 populations and Kolmogorov–Smirnov tests confirm that our 124 towns are, on average, larger than the 363 towns. If complementarities, scapegoating and other effects vary with population size, our estimated

[31] We create a two-dimensional surface of predicted plague mortality using an inverse distance weighted function of known mortality rates for the full sample of 263 towns with mortality data. For every point on the surface a predicted mortality rate is then generated using the closest 15 cities within an approximately 1000 km radius circle around the point. Details can be found in Web Appendix Section 5 (map of extrapolated mortality rates shown in Web Appendix. Fig. A.3).

[32] One problem here is that several towns now use the same provincial estimate [e.g., Aragon (7 towns) and Bohemia (5)]. Our results are then robust to dropping towns located within different modern country borders (Web Appx. Table A.13): France, Germany, Italy, Portugal and Spain. Other countries only contribute 7 towns or fewer.

**Table 7** Mortality and persecutions, measurement and specification

| | | Mortality 1347–1352 | | Constant | | Obs. | $R^2$ |
|---|---|---|---|---|---|---|---|
| *Dependent variable: dummy if any Jewish persecution in 1347–1352* | | | | | | | |
| 1. | Use town population in 1300 as regression weights | −0.011*** | [0.003] | 0.749*** | [0.181] | 124 | 0.14 |
| 2. | Use mortality of nearest avail. town within 100 km | −0.007*** | [0.001] | 0.690*** | [0.076] | 307 | 0.07 |
| 3. | Mortality extrapolated from 263 towns | −0.015*** | [0.002] | 0.963*** | [0.075] | 363 | 0.19 |
| 4. | Use provincial mortality from Christakos et al. (2005) | −0.008*** | [0.002] | 0.722*** | [0.090] | 159 | 0.08 |
| 5. | Controlling for log town population 1300 | −0.009*** | [0.002] | 0.921*** | [0.116] | 124 | 0.14 |
| 6. | Dropping bottom and top 25% in population 1300 | −0.008*** | [0.003] | 0.812*** | [0.142] | 63 | 0.11 |
| 7. | Dropping top 10% in population 1300 | −0.009*** | [0.002] | 0.808*** | [0.110] | 111 | 0.10 |
| 8. | Use information from Encyclopedia Judaica only | −0.013*** | [0.002] | 0.961*** | [0.107] | 94 | 0.25 |
| 9. | Drop if Unsure about Jewish presence or persecution | −0.012*** | [0.002] | 0.957*** | [0.109] | 92 | 0.20 |
| 10. | Use all towns with mortality data | −0.007*** | [0.001] | 0.533*** | [0.070] | 263 | 0.10 |
| 11. | Drop description-based mortality data | −0.011*** | [0.003] | 0.927*** | [0.110] | 97 | 0.15 |
| 12. | Drop desertion-based mortality data | −0.006** | [0.003] | 0.624*** | [0.131] | 99 | 0.05 |
| 13. | Drop number-based mortality data | −0.010*** | [0.003] | 0.921*** | [0.138] | 52 | 0.16 |
| 14. | Use only number-based mortality data | −0.007** | [0.003] | 0.704*** | [0.159] | 72 | 0.06 |
| 15. | Keep top and bottom 25% in mortality | −0.008*** | [0.003] | 0.855*** | [0.116] | 38 | 0.12 |
| 16. | Conley standard errors (100 km) | −0.009*** | [0.002] | 0.831*** | [0.112] | 124 | 0.12 |
| 17. | Probit model (marginal effect: −0.009***) | −0.025*** | [0.007] | 0.887*** | [0.304] | 124 | 0.09 |

Robust SE's: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. See Web Appendix for data sources effect may differ from the *average* effect. We thus show results hold if we: (i) control for log population in 1300 (row 5); (ii) drop the towns in the top and bottom 25% in population size (row 6); and (iii) drop the towns in the top 10% in population size (row 7), as doing so is enough to ensure that our sample is not significantly different in size from the sample of 363 towns (not shown).

*Measurement concerns* Classical measurement error in the persecution dummy should, if anything, increase standard error, thus making our estimated effect less precise as the true protective effect. However, our results remain precise, since the baseline effect is significant at 1%. Additionally, in row 8 we report our estimates using a sample based solely on the *Encyclopedia Judaica*. In row 9 we drop 12 towns where we cannot be entirely certain that there was a community intact in 1347 and 20 towns where we cannot be entirely certain about the fate of the community during the period.[33] We analyze all 263 cities for which we have mortality estimates (row 10). We implement this test because it is possible that some of the towns that we classify as not having Jews actually had Jews in 1347. Results hold overall.

Another related concern are non-classical measurement errors. If the community is small, and mortality high, there may not be any Jew left to persecute. We then misclassify as having Jews cities that did not have Jews when deciding to persecute, particularly so for high-mortality cities. This creates a negative relationship between mortality and persecution probability. However, while Jews accounted for a small share of city population (mean of 7.5%, for the 30 towns for which we have such data), towns were not that small on average (mean of 18,000 inhabitants for the 124 towns), so the size of their community was not negligible (mean of 1060 inhabitants for the 30 towns). Even with an extreme mortality rate, there would have been Jews left to persecute. Second, the mean duration of the plague in each town was 5 months, and not all people died at once, so there would certainly have been Jews alive the first months. Third, Fig. 3b showed that the persecution probability almost linearly decreases for mortality rates higher than 20%. Results are not driven by high-mortality outliers. Row 12 of Table 6 confirms this by dropping those cities in the top 25% of most affected cities (mortality above 50%).[34]

Perhaps at high mortality rates there is no one left to report a persecution, so that persecution rates are underestimated? However, the *Encyclopedia Judaica* does not only rely on documents provided by communities themselves, but also on official city records, personal diaries, cemetery records, etc. Second, most towns were sufficiently large, and their estimated residual population in the immediate wake of the Black Death remained large (mean of 11,000 inhabitants). Even in the top 25% of most affected cities, the residual population remained large (mean of 6500 inhabitants). Third, persecution probability decreases almost linearly with mortality and results hold when dropping or keeping the top 25% of most affected cities.

Classical measurement error in mortality should, if anything, bias the estimated effect towards 0. In that case, our estimated effect is not as strong as the true protective effect. Our IV results could be consistent with that since they produce protective effects that are stronger, i.e. more negative, than the OLS effect. However, measurement error could also be non-classical, hence the need to examine how the effects vary depending on the sources used to obtain the mortality rates. We find nonetheless that the effect does not systematically differ from the

[33] The 12 towns include 8 French towns for which information is sparse following the expulsions of 1306–1322, and 3 Tuscan towns in which Jews settled in the fourteenth century, but for which we cannot be sure of the year. The 20 towns include towns for which sources mentioned a persecution, but without providing corroborating details about it.

[34] Web Appendix Table A.5 shows results hold if we restrict the sample to towns where the community was likely to be large enough for the plague *not* to annihilate the community, i.e. towns with a Jewish synagogue/quarter/cemetery or towns where Jews have been living for a long time.

baseline effect when: (i) we drop mortality estimates based on literary descriptions (row 11 of Table 7); (ii) we drop estimates based on desertion rates (row 12); (iii) we drop estimates based on numerical estimates (row 13); and (iv) we only use numerical based estimates (row 14). Another approach is to focus on those cities that are either in the bottom 25% of least affected cities or in the top 25% of most affected cities, since measurement errors in mortality rates are more consequential when comparing towns with relatively similar estimated rates, for example towns close to 50% (row 15).

*Specification checks* In row 16 we employ Conley standard errors (threshold of 100 km) to account for spatial auto-correlation. In row 17 we use a probit regression model instead of a linear regression model. The estimated marginal effect is the same as the baseline effect (− 0.009***).

## 5 Discussion of the mechanisms

One approach to explore the mechanisms is to compare towns with *different* mortality rates, and observe how the *de facto* preference for diversity and the economic valuation of having Jews around differ across these towns. Obviously, no data exist on the level of antisemitism, the economic value of the services provided by Jews, and the cost of committing a persecution for each town *during* the months in which the town was infected. We only observe actions. However, since actions reveal preferences, we can use our conceptual framework, econometric results, and qualitative evidence, to discuss which patterns of the scapegoating and complementarities effects could make sense in our context. We follow this approach in Sect. 5.1.

Another approach is to compare towns with *similar* mortality rates but different characteristics that may affect the relative strength of the scapegoating and complementarities effects. This approach, which we follow in Sect. 5.2, allows us to quantify the importance of the effects.

### 5.1 Qualitative evidence on the mechanisms

To explain the patterns in the data, a plausible explanation is that: (i) at low levels of mortality, the scapegoating effect is strong, so that the persecution probability is high (relative to the pre-plague case); and (ii) at high levels of mortality, the scapegoating effect decreases and/or is offset by a complementarities effect that reduces the likelihood of a persecution when mortality increases.

*Scapegoating: blaming Jews* The Black Death persecutions provide a classic example of ideological scapegoating. The Jews were blamed for spreading the disease (Cohn 2007; Aberth 2009). In particular, chroniclers' accounts suggest that it was the sheer scale and devastating impact of the Black Death that made contemporaries feel that it was either the wrath of God or part of a grand conspiracy and the responsibility of the antichrist or his followers (Nohl 1924; Moore 1987; Horrox 1994). In line with the scapegoating theory, this suggests that the probability of persecuting the Jews was likely to increase with plague virulence (Glick 2002, 2005).

*Scapegoating: updating priors* An alternative explanation is that the scapegoating effect was initially increasing in mortality, but as mortality increased further it declined as non-Jews revised their prior that Jews were to blame. For example, in September 1348, Pope Clement

VI issued a Bull contradicting the libel against the Jews (Chazan 2010, 153–154), observing that "it cannot be true that the Jews [...] are the cause or occasion of the plague, because throughout many parts of the world the same plague, by the hidden judgement of God, has afflicted and afflicts the Jews themselves" (Horrox 1994, 222).

However, by then, 16 persecutions had already taken place in our data, including persecutions around Avignon. The Papal Bull also did not prevent the 42 persecutions in our data after September 1348. One reason for this was that the authority of Papacy had been weakened by the relocation to Avignon in 1309 and the perception that the Pope was the "puppet" of the French king (Mollat 1963; Renouard 1970). Bishops and Archbishops had independent discretionary authority due the limitations of communication technologies. The low clergy, moreover, often held more antisemitic views (Cohen 1982). It is therefore unlikely that the Bull had influence beyond Avignon. Furthermore, the theory of "motivated reasoning" suggests that once individuals have decided to blame a group for a misfortune, they are resistant to countervailing evidence (Kunda 1990). The results are thus unlikely to be driven by Christians updating their priors.

*Scapegoating: cost of committing a persecution* We could also imagine that Christian townsfolk were less able to organize themselves to persecute the Jews at higher mortality rates. Indeed, if many were ill (or dead), or had to take care of the sick, they may have not been able to commit a persecution. Note, however, that most persecutions in our sample were pogroms and compared to expulsions, which were orderly and organized, pogroms required almost no organization.

First, Jews were easily identifiable, often lived in specific areas and were forbidden to bear arms (see Baron 1967). Second, persecutions could be carried out by spontaneous mobs of villagers and townsfolk, local people armed only with axes, knives, pitchforks or wooden sticks (Aberth 2009).[35] Third, the plague lasted on average 5 months, long enough for a pogrom to be organized.

In addition, if organizational costs were high enough to prevent persecutions at high mortality rates, this should especially affect our results when comparing places with low and high mortality rates. However, we showed that the protective effect was stronger when dropping the towns with the 25% highest and the 25% lowest mortality rates (i.e. mortality rates below 25% or above 50%), so places with presumably low and high organizational costs (Table 3, row 13). Likewise, our results hold when dropping only the 25% highest mortality rates (i.e., mortality rates above 50%), since the high organizational costs should affect high-mortality towns (Table 6, row 12 ). With a mortality rate of 50%, as long as the community has at least two residents, there should be one Jew left to persecute. However, in our sample of 29 towns for which we know the size of the community, the mean Jewish population is 1060, and the five smallest communities have 22, 57, 128, 132 and 150 individuals. It is thus very unlikely that no Jews were left to persecute.

Additionally, a high mortality rate is only a problem if there are few townsfolk left to commit the persecution. We show that results hold if we restrict our sample to towns with a population in 1300 below or above the median (Table 6, rows 13–14). Likewise, results hold if we restrict our sample to towns with a low or high population in the immediate aftermath of the plague. More precisely, we estimate this "residual" population as the population of the town in 1300 multiplied by (100—the mortality rate of the town)/100, and then show results hold if we only use towns with a residual population below or above the median (Table 6,

[35] In Ulm we are told that on Jan. 30, 1349, a mob "stormed" the Jewish quarter; in Magdeburg, the "citizens and peasants of the vicinity fell upon the Judendorf, pillaged it, and burned many Jews in their houses".

rows 15–16). Similarly, it is also not the case that a higher mortality rate would reduce the chances of there being a Jewish community at all, or anyone at all to report a persecution.

*Complementarities* The role Jews played as moneylenders in medieval Europe is documented by numerous historians (Roth 1961; Baron 1967; Dorin 2015) and economists (Botticini and Eckstein 2012; Pascali 2016; Becker and Pascali 2019). The literature suggests that towns internalized the economic benefits brought by Jews. Rulers "anticipated Jewish contribution to the economy" (Chazan 2010, 102).[36] Elites had "reasons to want to curb …[antisemitic] violence, and at times they succeeded …The Jews were a major source of credit for them in a society that forbade Christians charging realistic interest on loans" (Byrne 2004, 84–85).[37] As Becker and Pascali (2019, 1773) conclude for pre-Reformation Germany "Jewish moneylending played an important role for German rulers and citizens".

For the Black Death period, historians note that the "economic importance" of certain communities enabled them to weather persecutory storms. An important example is Regensburg where "moneylending to secular and ecclesiastic princes, in addition to the merchants of the city, was conducted on a large scale" and where the Jewish community survived the Black Death persecutions and the city reached "new heights of prosperity by offering asylum to rich refugees" (Wasserman 2007, 188).

Similar motivations explain why "[t]he patrician leaders of Strasbourg, Basel and Freiburg agreed that the Jews should be protected from popular demands if this was at all possible" (Rowan 1984, 25). Strasbourg is a particularly well documented case, a city in which the importance of Jews as merchants and moneylenders to the local economy is attested in the historical record (Ephraim 1922, 157). As a result, the city council was initially determined to protect the Jews and argued that the Jews should not be killed on hearsay evidence. This arouse the enmity of the artisans and nobility, as well as guild members who viewed the Jews as competitors and when the city council was overthrown, one of the first acts of the new government was to destroy the Jewish community. Similarly, Guild members in Basel, Ertfurt, Nuremberg, and Würzburg are mentioned as playing a leading role in having Jews killed.

Frequent tax breaks and other encouragements to resettle in the aftermath of a pogrom provide further evidence that contemporaries recognized the important economic role played by Jews. [38]

This qualitative evidence is consistent with a positive complementarities effect whereby Jews were more than proportionally needed when mortality increases. An alternative pattern, in which the Jewish contribution to economic activity is valued less as mortality increases,

[36] In the eleventh century the Bishop of Speyer argued that "the glory of our town would be augmented a thousandfold if I were to bring Jews" (quoted in Chazan 2010, 101). Similarly, peasants in Tuscany petitioned for the admission of Jewish lenders to make credit more abundant (Botticini 2000). Rowan observes that "Mass demonstrations against Jews tended to be feared by patrician governments, since the Jews were still useful to them. The destruction of the Jews would have meant the loss of a major source of tax money which would have had to be made up by increasing the burden on some other social group" (Rowan 1984, 23).

[37] We do not view this as incompatible with the argument of Cohn (2007) that elites often preempted or organized massacres of Jews. Given popular anger against the Jews, there were strong incentives for local authorities to take control and to organize the massacres (either to control them or perhaps to ensure they did not lead to wider social disorder).

[38] Jews returned to Nuremberg soon after the plague persecutions (Berenbaum et al. 2007b). In Aragon, where the impact of the plague was severe, King Pedro strove to protect the Jews and in the wake of the plague "was determined to restore the Jewish *aljamas* to a healthy economic state". He barred creditors from bringing lawsuits against Jews or collecting debts against them for a year while individuals who moved into settle land left empty due to the mortality associated with the plague were forced to inherit the debts owed to the Jews (Shirk 1981, 363).

is possible. Perhaps in the presence of high death rates, people and rulers become more present-biased and cease to value the long-term externality of a Jewish community. In this case the incentive to persecute would increase. However, given that Jews were likely to have been more than proportionally blamed when mortality increased, the lower persecution probability observed at higher mortality rates can best be explained by a complementarities effect increasing with mortality. In other words, the incentive to persecute was dominated by the incentive to protect them at higher mortality rates.[39]

### 5.2 Quantitative evidence on the mechanisms

To study these channels quantitatively, we interact mortality with various pre-Black Death town characteristics and look at how the effects of mortality on the probability of persecution were accentuated or attenuated depending on these. We estimate a series of regressions based on:

$$P_{i,1347-52} = \alpha + \beta \mathrm{M}_{i,1347-52} + \delta \mathrm{M}_{i,1347-52} * \mathbf{X}_i + \gamma \mathbf{X}_i + \epsilon_i \ , \qquad (2)$$

where $P_{i,1347-52}$ is a dummy equal to one if there is a persecution in town $i$ in 1347–1352, $M_{i,1347-52}$ is the cumulative mortality rate (%) in town $i$ over the period 1347–1352, and $\mathbf{X}_i$ is a dummy variable proxying for a pre-plague town characteristic. $\delta$ is our main coefficient of interest and captures whether the protective effect of higher mortality was accentuated or attenuated in towns with this characteristic. As we control for the mortality rate, we are comparing towns with the same mortality rate, but different pre-shock characteristics. Note that we are also controlling for the individual effect of the town characteristic on the persecution dummy.

Tables 8 and 9 report the effect of mortality ($\beta$), the effect of its interaction with the dummy variable proxying for the characteristic ($\delta$), and the combination of these effects ($\beta + \delta$), to see if the interacted effect is strong enough to offset the protective effect of mortality. Note that the dummies that start as "Close to …" are equal to one for the towns that are in the bottom 10% of the distance to the described location. In addition, the interacted effect of mortality and the dummy is not necessarily causal, as the characteristic may be correlated with other characteristics.[40]

### 5.3 Evidence on scapegoating

In Table 8 we examine the effects of scapegoating on the probability of persecution.

#### 5.3.1 The well poisoning libel

*The origin and spread of the libel* Figure 8a shows Chillon Castle, the place of origin of the well-poisoning libel, and the Rhine river, along which the rumor of a conspiracy spread. Proximity to Chillon (row 1, 0.016*) wipes out the protective effect of high mortality. The effects of proximity to the 10 towns that were first warned of the alleged conspiracy also

39 Many authorities such as in Cologne and Zurich tried to protect their communities because of their economic value (Schwarzfuchs and Kaufmann 2007; Berenbaum and Carlebach 2007).

40 Web Appendix Table A.18 shows that the interacted effects logically decrease when using higher percentiles, whether 15, 20, 25 or 50% (see Web Appendix Section 13). Interacted effects are thus larger the closer to the studied characteristic.

**Table 8** Mortality and persecutions, evidence on scapegoating

| | Effect of: Rows 1–18: Dummy Equal to 1 if: | Mortality Rate ($\beta$) | | Mortality x Dummy ($\delta$) | | Sum ($\beta + \delta$) | | Dummy ($\gamma$) | | $R^2$ |
|---|---|---|---|---|---|---|---|---|---|---|
| *Dependent variable: dummy if any Jewish persecution in 1347–1352* | | | | | | | | | | |
| 1. | Close to Chillon castle | −0.010*** | [0.002] | 0.016* | [0.009] | 0.006 | [0.009] | −0.44 | [0.38] | 0.14 |
| 2. | Close to towns warned by letter | −0.010*** | [0.002] | 0.017* | [0.009] | 0.007 | [0.009] | −0.33 | [0.37] | 0.18 |
| 3. | Close to Rhine river | −0.009*** | [0.002] | 0.017** | [0.007] | 0.008 | [0.007] | −0.09 | [0.27] | 0.20 |
| 4. | Close to seat of papacy 1347 | −0.010*** | [0.002] | 0.018*** | [0.006] | 0.008 | [0.005] | −0.94*** | [0.26] | 0.14 |
| 5. | Seat of bishop/archbishop 1347 | −0.006* | [0.003] | −0.007 | [0.005] | −0.013*** | [0.003] | 0.16 | [0.21] | 0.14 |
| 6. | Very recent entry ($\leq$ 5 Years) | −0.011*** | [0.002] | 0.015*** | [0.005] | 0.005 | [0.004] | −0.16 | [0.26] | 0.20 |
| 7. | Cemetery/quarter/synagogue | −0.009*** | [0.003] | 0.000 | [0.005] | −0.009** | [0.004] | 0.02 | [0.21] | 0.12 |
| 8. | Ashkenazi settlement 13C | −0.011*** | [0.002] | 0.015** | [0.007] | 0.004 | [0.007] | −0.29 | [0.24] | 0.16 |
| 9. | Very recent persecution in 1340–46 | −0.010*** | [0.002] | 0.039* | [0.021] | 0.029 | [0.021] | −1.82 | [1.42] | 0.14 |
| 10. | Close to pogrom 1st Crusade 1096 | −0.009*** | [0.002] | 0.016** | [0.007] | 0.007 | [0.007] | −0.221 | [0.28] | 0.14 |
| 11. | Close to ritual murder 13C | −0.010*** | [0.002] | 0.016** | [0.008] | 0.006 | [0.007] | −0.33 | [0.29] | 0.15 |
| 12. | Close to host desec. 1st half 14C | −0.010*** | [0.002] | 0.011** | [0.005] | 0.001 | [0.005] | −0.01 | [0.24] | 0.19 |
| 13. | First infected Dec (Advent) | −0.009*** | [0.002] | −0.022** | [0.009] | −0.031*** | [0.013] | 0.76* | [0.42] | 0.13 |
| 14. | First infected Jan (christmastide) | −0.011*** | [0.002] | 0.016** | [0.007] | 0.005 | [0.007] | −0.53* | [0.30] | 0.16 |
| 15. | First infected Feb–Mar (lent) | −0.008*** | [0.002] | −0.014*** | [0.003] | −0.022*** | [0.002] | 0.44** | [0.18] | 0.14 |
| 16. | First infected Apr–May (easter) | −0.011*** | [0.002] | 0.011* | [0.006] | 0.000 | [0.006] | −0.49* | [0.27] | 0.14 |
| 17. | First infected Oct (2nd planting) | −0.009*** | [0.003] | 0.011*** | [0.004] | 0.002 | [0.003] | −0.89*** | [0.15] | 0.16 |
| 18. | Climate shock 1347–1350 | −0.008*** | [0.003] | 0.008*** | [0.003] | 0.000 | [0.000] | −0.82*** | [0.11] | 0.14 |

This table shows for the main sample of 124 Jewish towns the respective effects of the mortality rate ($\beta$) and the respective interacted effects ($\delta$) of the mortality rate times a dummy variable shown in each row on a dummy equal to one if there has been any Jewish persecution in 1347–1352. It also shows the combination of the effect of the mortality rate ($\beta$) and the interacted effect of the mortality rate and the dummy ($\delta$)

Robust SE's: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$. See Web Appendix for data sources

**Table 9** Mortality and persecutions, evidence on complementarities

| | Effect of: Rows 1–18: dummy equal to 1 if: | Mortality Rate ($\beta$) | | Mortality x Dummy ($\delta$) | | Sum ($\beta + \delta$) | | Dummy ($\gamma$) | | $R^2$ |
|---|---|---|---|---|---|---|---|---|---|---|
| *Dependent variable: dummy if any Jewish persecution in 1347–1352* | | | | | | | | | | |
| 1. | Top 10% town pop. 1300 | – 0.009*** | [0.002] | – 0.013** | [0.006] | – 0.021*** | [0.006] | 0.43 | [0.33] | 0.13 |
| 2. | Top 10% predicted pop. 1353 | – 0.009*** | [0.002] | – 0.012*** | [0.004] | – 0.021*** | [0.003] | 0.43* | [0.23] | 0.13 |
| 3. | Belongs to kingdom 1300 | – 0.010*** | [0.003] | 0.005 | [0.005] | – 0.005 | [0.003] | – 0.42* | [0.21] | 0.16 |
| 4. | Belongs to large kingdom 1300 | – 0.011*** | [0.002] | 0.008* | [0.005] | – 0.003 | [0.004] | – 0.49** | [0.22] | 0.16 |
| 5. | Jewish moneylending | – 0.006** | [0.003] | – 0.008** | [0.004] | – 0.014*** | [0.003] | 0.60*** | [0.17] | 0.21 |
| 6. | Jewish moneylending (≤ 13C) | – 0.006** | [0.003] | – 0.010** | [0.004] | – 0.016*** | [0.003] | 0.65*** | [0.17] | 0.22 |
| 7. | Jewish moneylending (restrictive) | – 0.006** | [0.003] | – 0.008** | [0.004] | – 0.014*** | [0.003] | 0.52*** | [0.17] | 0.17 |
| 8. | Close to major financial centers | – 0.008*** | [0.002] | 0.008*** | [0.002] | 0.000 | [0.000] | – 0.84*** | [0.11] | 0.19 |
| 9. | Top 10% market access 1300 | – 0.008*** | [0.003] | – 0.010*** | [0.004] | – 0.018*** | [0.115] | 0.45** | [0.17] | 0.13 |
| 10. | Coast 10 Km | – 0.009*** | [0.003] | – 0.001 | [0.007] | – 0.010 | [0.006] | 0.05 | [0.35] | 0.12 |
| 11. | North-Baltic coast 10 Km | – 0.009*** | [0.003] | 0.009*** | [0.002] | 0.000 | [0.999] | 0.19 | [0.11] | 0.14 |
| 12. | Atlantic coast 10 Km | – 0.009*** | [0.003] | 0.009*** | [0.002] | 0.000 | [0.999] | – 0.83*** | [0.10] | 0.13 |
| 13. | Mediterranean coast 10 Km | – 0.009*** | [0.003] | 0.002 | [0.007] | – 0.007 | [0.006] | – 0.12 | [0.38] | 0.12 |
| 14. | Medieval route intersect. 10 Km | – 0.009*** | [0.002] | – 0.009* | [0.005] | – 0.018*** | [0.005] | 0.28 | [0.22] | 0.13 |
| 15. | Maj. Roman road intersect. 10 Km | – 0.004 | [0.004] | – 0.007 | [0.005] | – 0.010*** | [0.003] | 0.07 | [0.21] | 0.16 |
| 16. | Rivers 10 Km | – 0.008*** | [0.003] | – 0.004 | [0.005] | – 0.011*** | [0.004] | 0.20 | [0.20] | 0.12 |
| 17. | Top 10% Jewish centrality index | – 0.008*** | [0.002] | – 0.010** | [0.004] | – 0.018*** | [0.004] | 0.59*** | [0.15] | 0.15 |
| 18. | Market fairs | – 0.009*** | [0.002] | – 0.001 | [0.007] | – 0.011 | [0.006] | 0.02 | [0.27] | 0.12 |
| 19. | Hanseatic League | – 0.009*** | [0.002] | 0.005 | [0.005] | – 0.004 | [0.004] | 0.21 | [0.14] | 0.17 |
| 20. | Hanseatic League capitals | – 0.009*** | [0.002] | 0.009*** | [0.002] | 0.000 | [0.999] | 0.18* | [0.10] | 0.13 |

This table shows for the main sample of 124 Jewish towns the respective effects of the mortality rate ($\beta$) and the respective interacted effects ($\delta$) of the mortality rate times a dummy variable shown in each row on a dummy equal to one if there has been any Jewish persecution in 1347–1352. It also shows the combination of the effect of the mortality rate ($\beta$) and the interacted effect of the mortality rate and the dummy ($\delta$)

Robust SE's: *$p < 0.10$, **p< 0.05, ***$p < 0.01$. See Web Appendix for data sources

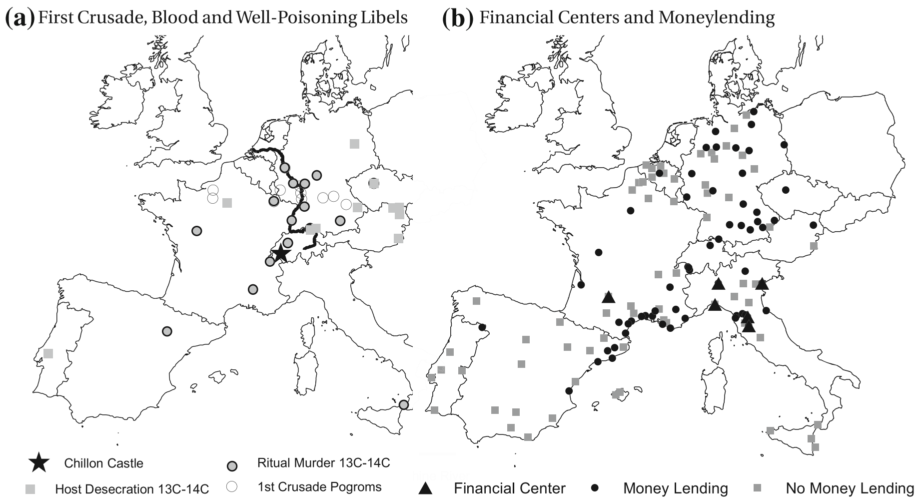


**Fig. 8** Mapping of first crusade, blood and well-poisoning libels, and financial competition. *Notes* Panel **(a)** shows the locations of the 12 pogroms that took place during the First Crusade (1096), the locations of the 19 charges of ritual murder and the 11 charges of host desecration in the thirteenth and fourteenth centuries, Chillon (the place of origin of the well-poisoning libel), and the Rhine river (along which the rumor of a Jewish conspiracy spread). Panel **(b)** shows the main financial centers of Western Europe (Cahors, Florence, Genoa, Milan, Sienna and Venice) and the 60 towns where Jews were lending money ca. 1347. See Web Appendix for more details on data sources

attenuate any protective effects and increase the probability of a persecution when mortality was high (row 2, 0.017*). Proximity to the Rhine river also reduces the protective effective of mortality (rows 3, 0.017**).[41]

*Attempts to counteract the libel* We explore the extent to which the Pope was able to counteract the libel. We find that close to Avignon there was no protective effect of mortality (row 4, 0.018***). This likely reflects the fact that in many cases the plague arrived and Jews were massacred prior to the Papal Bull. Looking at cities that were the seats of bishops or archbishops, we find some evidence that the scapegoating effect was weakened, but not significantly so (row 5, −0.007). This weak effect is consistent with the limited influence that the Pope had on local decisions.

*Jewish community characteristics* We find that in towns where Jews were recent migrants (less than 5 years), the protective effect of higher mortality was attenuated (row 6, 0.015***). Speculatively, this may be because people made a direct association between the recent arrival of Jews and the plague.[42] We consider whether the presence of a Jewish cemetery/quarter/synagogue had an effect on persecutions, as this might indicate that the community was well established. In that case, we would expect the protective effect to be accentuated. The protective effect would be attenuated if non-Jews resent the fact that Jews

[41] Historians have traditionally held the flagellants responsible for massacring Jews. Yet, we find no evidence that the path of the flagellant movement was associated with the mortality-persecution relationship (see Web Appx. Table A.14). We find, however, that the protective effect was accentuated very close to Narbonne (i.e., for towns in the bottom 5% of the Euclidean distance to it). There “beggars and mendicants”, and not Jews, were accused of having poisoned the wells, which could explain the lower effect of mortality on persecution probability (see Web Appx. Table A.14).

[42] We do not find the same effects when considering older entries (see Web Appendix Table A.14).

have their own structures.[43] We find, however, no effects for these community characteristics (row 7). We also find no differential effect for each characteristic considered individually, even when controlling for community size via several proxies (years of first and last entry, centrality index, dummies if persecution in 1321–1346 and 1300–1346) (see Web Appendix Table A.14).[44].

*Ashkenazi communities* While all Jewish communities share a common genetic legacy that can be traced back to the Levant, there are differences between communities. Ashkenazi Jews share a number of genetic markers and were characterized by a low rate of admixture for the past 80 generations, suggesting that they remained a genetically distinct population (Behar et al. 2010). In contrast, Askhenazi (as well as Sephardic) Jews share more genetic markers in common with southern European populations than they do with northern Europeans (Seldin et al. 2006). Therefore, Ashkenazi Jewish communities were likely to have been culturally more distant from their Christian neighbors than was the case for Sephardic Jews in southern Europe. Looking at conflict in the modern period, Arbatli et al. (2019) find that genetic diversity within a country predicts violence and conflict. Consistent with this in row 8, we find that mortality conferred less of a protective effect on Ashkenazi communities than it did on Sephardic communities (0.015**).

### 5.3.2 Latent antisemitism

*Past pogroms* When we include an interaction with a dummy for if there had been a persecution in the 6 years (1340–1346) prior to the Black Death period (which also lasted 6 years), we find a positive and significant effect (row 9, 0.039*). As we report in Web Appendix Table A.14, this effect is weaker if the past persecution dummy is defined for a past period of 25 years in 1321–1346 (0.012, only significant at 15%), and nil if it is defined for a past period of about 50 years in 1300–1346. This suggests that a recent history of persecutions, i.e. of antisemitic violence committed by ones' parents, made a difference and diminished the "protective effect". However, one could also argue that recent persecutions made the community smaller at the time of the Black Death, so that the stronger protective effect actually reflects the reduced ability of the community to protect itself. That is why we now turn to more historical measures of antisemitism.

*Crusades* The First Crusade (1096) saw a marked intensification of antisemitism (Cohen 1957, 77). The call for a crusade to recapture Jerusalem from the Muslims elicited tremendous popular religious passion and anger against the enemies of Christ who included both the Muslims and the Jews (Eidelberg 1977; Chazan 1987). Large numbers of knights and peasants gathered in Cologne and Worms and marched through Germany on their way to Jerusalem and on their way attacked Jewish communities (see Fig. 8a for the locations of these pogroms).[45]

[43] The existence of a Jewish quarter meant that Jews were separated from Christians. This may have prevented non-Jews from seeing that Jews were also dying. However, it might also have simply reflected the larger size of a community.

[44] There is no existing data on the layout of towns back then, so we do not know the location of the quarter relative to the main areas of the town then. The only other characteristic for which we have data is walled density (N = 56), but we find no differential effect across towns above and below median density (see Web Appendix Table A.14).

[45] Schwarzfuchs (2007, 311) writes: "The sight of the wealthy Rhenish communities acted as an incentive to the crusaders, who decided to punish 'the murderers of Christ' wherever they passed".

We find that in towns close to a First Crusade-era pogrom, the protective effect of mortality is wiped out (row 10, 0.016**).[46]

*Ritual murder* The rise of virulent antisemitic tropes over time led to an intensification of animosity in the middle ages (Trachtenberg 1943; Rubin 2004). Blood libel accusations began in England in the twelfth century and spread through Europe (Stacey 1998). The libel involved the story that an organized group of Jews had murdered Christian children in order to obtain blood for the Passover (Ben-Sasson et al. 2007, 74). The myth of the blood libel was spread by preachers and reinforced when Jews were tortured into confessing to the alleged crime. Figure 8a shows the locations of ritual murder charges in the thirteenth and early fourteenth centuries. When we interact the mortality rate with a dummy for towns close to the alleged location of a ritual murder in the thirteenth century, the protective effect of higher mortality is attenuated (row 11, 0.016**).

*Host desecration* As the belief that the consecrated eucharist contained the body of Christ became more important in the twelfth and thirteenth centuries, stories of Jews desecrating the host began to circulate in Europe (Rubin 1992, 2004).[47] By desecrating the host, the Jews were imagined to be reenacting the suffering of Christ. Such charges often inspired acts of antisemitic violence. Figure 8a shows the locations of host desecration charges in the thirteenth and early fourteenth centuries. We find that accusations of host desecration in the first half of the fourteenth century (but not earlier) eliminate any protective effect of mortality (rows 12, 0.011**).[48]

### 5.3.3 The liturgical and agricultural calendars

*Advent* Advent, the period before Christmas, is the beginning of the liturgical year. During that period, Christians are encouraged to do penance to prepare for the coming of Christ. Consistent with these motivations, in towns where December was the month of first infection, we find that the protective effect of Black Death mortality was reinforced (row 13, – 0.022**).

*Christmastide* In contrast, the period from Christmas Day (December 25th), when the birth of Jesus is commemorated, until the Feast of the Presentation of the Lord (February 2nd) was Christmastide. According Matthew's Gospel (2:1–23), after the birth of Jesus, magi visited Herod the Great, the King of Jews, to inquire about the location of the "one having been born king of the Jews". Herod, who felt threatened by this newborn, ordered the death of all male babies, an event which came to be known as the Massacre of the Innocents. The commemoration of the massacre was historically connected with the Feast of the Epiphany, on

46 We find similar results if we use the main path of the First Crusade instead (see Web Appendix Table A.14). However, we do not find any effect if we use the places most associated with the main leaders of the First Crusade (ditto). The null effect is unsurprising since these are the locations from where the First Crusade was initially called for and organized and many Crusader leaders were not involved in pogroms which often involved local people. We also collected data on the pogroms that took place during the Crusades of 1147 and 1189 but the later Crusades were not characterized by large-scale antisemitic violence (Stacey 1999.).

47 Stacey (1998, 13) writes: "we begin to see Jews taking on a new role, as enemies not only of the body of Christ on the cross and of the body of Christ in the Church, but also as the enemies of the body of Christ in the eucharist. This is the notion that lies behind the host desecration charge—that Jews would torture […] consecrated eucharistic hosts".

48 The non-significant results for ritual murders in the first half of the fourteenth century and for host desecration in the thirteenth century are shown in Web Appendix Table A.14.

January 6th. During the Feast, Christians celebrated the martyrdom of the Innocents (Archer 1984, 47).[49]

Jews were historically viewed as "Christ-killers". The charge of deicide was at the heart of much antisemitism up until modern times (Freudmann 1994; Friedländer 1997; Donaldson 2010). This made Christmas a period of heightened tensions between Christians and Jews. Jews sometimes had to pay special taxes on Christmas Day.[50] Consistent with this, in towns where January was the month of infection, higher mortality conferred no protective effect (row 14, 0.016**).

*Lent* Lent, which usually starts in February or the first week of March (February 16th in 1348), was a 40-day period of restraint when Christians are supposed to fast and do penance. Consistent with this, in towns where February or March was the month of first infection, higher mortality conferred a stronger than usual protective effect (row 15, −0.014***).

*Easter* Another period with heightened antisemitism was the period between Easter, in April according to the Julian calendar, and the Ascension of Christ (40 days later). Easter celebrations such as the reenactments of the passion of Christ made the alleged role of the Jews in the death of Christ more salient. During Easter Christ was expected to rise and to triumph over his enemies.[51]

This was reflected in local institutions. Jews were prohibited from going forth in public on the 3 days before Easter (see Chanes 2004, 86). In some cities Jews paid a special tax on Easter to atone for the crime of killing Christ (e.g. Gottheil and Kahn 1906). This institution sometimes involved public humiliation.[52] As such, there were many cases of pogroms occurring during Easter.[53] Freudmann (1994, 300) notes that "the annual paschal pogrom became a Christian rite of spring". Consistent with this, in towns where April or May was the month of first infection, higher morality conferred no protective effect (row 16, 0.011*).[54]

Overall, these results are consistent with the *scapegoating* effect.

49 Archer observes that "At a period in which the individual child was not looked upon with particular tenderness, parental sentiment found a communal channel in the idolization of the Christ child and of the Innocents, the baby boys butchered in the first attempt to kill Jesus by Herod, the Jewish King" (Archer 1984, 47).

50 This was the case in Trier where Jews gave six pounds of pepper and silks for his clothing to the archbishop and two pounds of pepper to the chamberlain. In Öttingen, each Jew over the age of twelve had to pay a gulden to the emperor on Christmas day (Adler and Singer 1906).

51 A favorite Easter sermon from St. Augustine provided a fictitious antisemitic account of Jesus's death: "The Jews held him; the Jews insulted him; the Jews bound him; they crowned him with thorns, dishonored him by spitting on him; they scourged him; they heaped abuse on him …" (Freudmann 1994, 300). Just as hostility to Jews at Christmas-time manifested itself with the association of Jews with Herod, so at Easter they were closely associated with Judas.

52 In Toulouse there was a tradition whereby the Jewish community had to choose a member of the community every year to be publicly slapped in the face on Good Friday. In the middle ages this ritual was waived on the condition that the Jews pay a special fee to the town (Blumenkranz et al. 2007).

53 The tensions around the month of April were further heightened by the fact that Passover typically coincides with Easter and Jews were accused of ritually murdering and sacrificing Christian children as part of this festival (Rubin 2004). Numerous studies suggest that this was a factor during the Black Death. The Jewish community of Toulon were massacred on 12–13 April 1348, at the same time as the plague reached the city (Cremieux 1930).

54 We investigate the effect for each month individually, by regressing the persecution dummy on the mortality rate, 12 month of infection dummies, and the 12 interactions of these dummies with the mortality rate. We then test whether the effect of each month is significantly different from our baseline effect of −0.009. Web Appendix Figure A.7 (see Web Appendix Section 13) confirms that the protective effect of mortality was accentuated in February, March and December, and attenuated in January, May (significant at 15%) and October. There is no clear effect for April now (Easter took place in late April in 1348).

*Agricultural calendar* An alternative interpretation of some of our seasonal results is based on the agricultural calendar as April and October were the main months of planting and April-May were the lean months when farmers had to borrow money from Jewish moneylenders to buy new seeds and sometimes food that would last them until the next harvest (July).[55] These farmers had to repay their loan six months later around October and sometimes extending an existing loan from the same Jewish moneylenders to buy seeds for the winter planting season.[56] The smaller protective effect of higher mortality in April–May (row 16, 0.011*) and October (row 17, 0.011***) could be consistent with the desire to not have to repay these loans.

*Climate shocks* Finally, using climate data from Anderson et al. (2017), we identify cities that have been affected by a temperature shock, here defined as cities whose mean temperature in a year is one standard deviation higher or lower than the long-run average of mean annual temperature in 1300–1499. Row 18 shows that the protective effect of higher mortality was significantly reduced for cities affected by a shock during our main period (1347–1350). Note that these shocks were all cold spells, i.e. negative temperature shocks. Since no significant effects are found for earlier shocks (see Web Appendix Table A.14), the contemporary effect could be consistent with the desire to not to repay agricultural loans in times of social disruption.

### 5.4 Evidence on complementarities

The economic complementarities hypothesis is that when plague mortality is high, the value of Jewish communities in terms of economic skills and contribution to tax revenue increases. We label this hypothesis (i). One alternative to the complementarities effect is that rather than generating a specific incentive to protect Jewish communities, higher plague mortality simply made all labor, and hence all human lives, more economically valuable. We label this hypothesis (ii).

In Table 9 we examine the effects of proxies for the role of complementarities on the probability of persecution. We also investigate the temporal effects of Jewish presence and Jewish persecutions on city growth in Table 10. Overall, we interpret our results as evidence that cities particularly valued Jews' skills, in line with (i) rather than (ii).

#### 5.4.1 City size and political entity size

*City size* Hypothesis (ii) should be most salient in smaller cities, since it was in these cities that the shock of the Black Death was likely to have the greatest impact on economic activity and tax revenue. Hence, if the effects that we find simply reflected the increased value of all workers (rather than the specific economic benefits associated with Jewish communities)

55 In ancien regime France, the passage of time was defined by "agro-liturgical calendars" (Moriceau 2010). These calendars provide information about the agricultural cycles of the medieval period. The agricultural cycle depends on whether one considers a town in the North or the South of Europe. In our sample, the mean latitude and longitude are 45.7 and 6.0, close to Chambéry. In France, December and February were "idle" months, January was the month fields had to be plowed for the spring sowing, March–April and October were the months of planting, April–May were the "lean" months, and July and October were the months of grain and grape harvesting.

56 Ramirez (2010, 64) writes for the region of Savoy in which Chambéry is located (translated): "Most of the loans [given by Jews] take place in the Spring and the Fall […] during the planting season and during the lean season."

**Table 10** Jewish presence, Black Death persecutions, and town growth

Dependent variable: log town population in year $t$

| *Panel A: effect of Jewish presence in period* $[t-1;t]$ *dummy* | Coeff. | SE | Obs. | Adj. $R^2$ |
|---|---|---|---|---|
| 1. Baseline effect of Jewish presence dummy | 0.32*** | [0.04] | 16,821 | 0.73 |
| 2. Row 1 + including the lag of log town population in $t-1$ | 0.23*** | [0.02] | 16,821 | 0.79 |
| 3. Individual effect of Jewish presence dummy | 0.11* | [0.06] | 16,821 | 0.73 |
| Individual effect of Jewish presence share | 0.27*** | [0.07] | | |
| 4. *Entries:* effect if Jews absent in previous period $[t\text{-}2;t-1]$ | 0.35*** | [0.05] | 16,821 | 0.74 |
| *Exits:* effect if Jews present in previous period $[t\text{-}2;t-1]$ | 0.12** | [0.05] | | |
| 5. Effect of Jewish presence dummy before fourteenth century | 0.30*** | [0.05] | 16,821 | 0.73 |
| Effect of Jewish presence dummy after fourteenth century | 0.39*** | [0.06] | | |

| *Panel B: effect of Black Death persecutions in 1347–1352 after fourteenth century* | Coeff. | SE | Obs. | |
|---|---|---|---|---|
| 1. Baseline effect of persecution in 1347–1352 dummy after fourteenth century | −0.21* | [0.11] | 16,821 | 0.73 |
| 2. Effect of pogrom in 1347–1352 dummy after fourteenth century | −0.31*** | [0.12] | 16,821 | 0.73 |
| Effect of expulsion in 1347–1352 dummy after fourteenth century | 0.24 | [0.16] | | |

All regressions include town FE and year FE, and extrapolated mortality interacted with year FE. *Panel A:* Effect on log town pop. in year $t$ of a dummy if Jews were present in $[t-1;t]$. Row 2: We control for log town pop. in $t-1$. Row 3: We add the share of years in which Jews were present in $[t-1;t]$. Row 4: We add a dummy for whether Jews were present in $[t-2;t-1]$, and interact it with our variable of interest. Row 5: We add a dummy for whether the year $t$ if strictly after the fourteenth century, so $t > 1400$ (and $[t-1;t] >$ [1300;1400]), and interact it with our variable of interest. *Panel B:* The dependent variable is log town pop. in $t$. We interact a dummy if there has been a persecution/pogrom/expulsion during the Black Death with a dummy for whether the year $t$ if strictly after the fourteenth century, so $t > 1400$ (and $[t-1;t] >$ [1300;1400]) Robust SE's clustered at the town level: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$. See Web Appendix for data sources

then these effects should be most pronounced in smaller cities (hypothesis (ii)). If, however, larger cities have economic sectors that disproportionately rely on Jews' skills (hypothesis (i)), we expect larger cities to disproportionately protect Jews.

To test these two hypotheses, we focus on city size in 1300 but also impute predicted city size circa 1353 using city size in 1300 and Black Death mortality rates. If hypothesis (ii) prevails, then cities shrinking because of the shock are more likely to protect Jews. In rows 1 and 2 of Table 9, we show that towns in the top 90% of population size circa 1300 or predicted population size circa 1353 experience lower persecution probability for a given mortality rate (−0.013*** and −0.012***, respectively), consistent with (i) rather (ii). In Web Appendix Table A.19, we verify that this protective effect logically decreases if we use cities in the top 75%, 50%, 25% or 10% of size circa 1300 or predicted size circa 1353.

*Political entity size* Another possibility is that cities that were independent or part of smaller polities were disproportionately reliant on their own populations for tax revenue and for the provision of public goods. The economies and public finances of these cities would be especially hard hit in the wake of losing a significant share of their population and hence more dependent on the contributions of the local Jewish community. This suggests that higher-mortality cities are more likely to engage in persecutions when they are part of a kingdom, especially a larger kingdom, in which the provision of public goods would be more likely to be determined at a higher level than that of the city. In our sample of 124 cities, 54 of them belonged to 10 “kingdoms” as defined by Nussli (2011), such as the Kingdom of Aragon, the Kingdom of Bohemia or the Kingdom of Portugal. Row 3 shows that being part of a kingdom reduces the protective effect, but this effect is not significant (0.005). However, three kingdoms (Majorca, Sicily-Trinacria and Granada) were particularly small (less than 30,000 sq km) and two of them were islands. If we use a large kingdom dummy that does not include these kingdoms, the protective effect is significantly reduced (row 4, 0.008*).[57]

### 5.4.2 Moneylending and financial competition

*Moneylending* In line with (ii), if Jews have skills that increase the total income of the town, either their own income or the income of non-Jews, we expect towns that rely on these skills to disproportionately protect Jews. One specific skill that Jews had during the Middle Ages was moneylending.

We collect data on whether Jews are mentioned as moneylenders in the *Encyclopedia Judaica*, the *Jewish Encyclopedia* and additional sources. Based on the first two sources only, moneylending is mentioned for 41 towns before the fourteenth century in our sample. Using additional sources, this increases to 51 towns, which we use as our main sample of moneylending towns (Fig. 8b depicts these towns). Where Jews are mentioned as lending money, we find that when mortality is high, the protective effect is reinforced (row 5, − 0.008**). This is suggestive evidence that in towns where Jews were moneylenders, they may have become more valuable when the plague was more destructive.[58]

We find no relationship between plague intensity and persecution probability and other economic characteristics of Jewish communities such as whether Jews worked as craftsmen, traders or as doctors, or are reported as having paid special taxes to the ruler (Web Appendix Table A.14).[59]

Is the interacted effect of mortality and moneylending causal? We have established the exogeneity of mortality. Jewish moneylending in cities in our sample generally predated the Black Death by a century or more: in 42 out of 51 towns, Jewish moneylending predates the fourteenth century. We drop towns where Jewish moneylending was a more recent phenomenon (of the first half of the fourteenth century) and where the decision to permit Jewish moneylenders might have been related to local conditions (row 6).

57 In Web Appendix Table A.19, we show how the protective effect decreases as the large kingdom dummy progressively excludes more smaller kingdoms.

58 Nevertheless, as we showed above, conditional on Jews lending money to non-Jews, they were more likely to be persecuted when the arrival of the plague coincided with the period when their debts were due to be repaid.

59 This is unsurprising as the literature agrees that the importance of Jewish merchants had waned by the fourteenth century. Roth (1961) argues that guilds pushed Jews out of trade and manufacturing into moneylending. However, Botticini and Eckstein (2012, 238) argue that guilds cannot explain the shift towards moneylending because by the time they became powerful: “the Jews in western Europe had entered and then become specialized and prominent in moneylending for at least two centuries”. Instead Botticini and Eckstein (2012) argue that Jews shifted away from trade due to their comparative advantage in moneylending. For our purposes the cause of this shift is irrelevant.

We also consider factors that might have given rise to Jewish moneylending. The most important of these was economic development after 1100. Due to prohibitions on usury and higher levels of Jewish human capital, Jews came to specialize as moneylenders (Botticini and Eckstein 2012). In Web Appendix Table A.16, we show that the interacted effect of moneylending is robust to controlling for a host of proxies for economic development (see Web Appendix Section 13).

Another hypothesis we consider is that the antisemitism generated by the First Crusade helped to push Jews out of trade and hence into moneylending.[60] We test whether moneylending is correlated with: (i) First Crusade pogroms; (ii) the path of the First Crusade; and (iii) the places associated with its main leaders. We find no effects (see Web Appendix Section 13 and Web Appendix Table A.15 for details).[61]

More generally, the interacted effect of mortality and moneylending holds when controlling for the individual effect and the interacted effect with mortality of each other characteristic in Tables 8 and 9 (and Web Appendix Table A.14). Finally, in row 7, we obtain similar results if we only define as moneylending towns the 41 towns mentioned as such in the *Encyclopedia Judaica* or the *Jewish Encyclopedia*, since there could be measurement error in the moneylending dummy.

*Financial competition.* Furthermore, we find that the protective effect of mortality is weakened for towns closer to major Christian financial centers. We construct a measure of access to financial markets based on distance to major financial centers at the time: Cahors, Florence, Genoa, Milan, Sienna and Venice (Fig. 8b).[62] For cities close to a financial center, the protective effect was attenuated when mortality was high (row 8, 0.008***). In regions with Christian financial cities, Jews were potential substitutes to Christian moneylenders and thus made potentially more vulnerable. The symmetry of the effects between Jewish moneylending and Christian finance increases our confidence in the results on Jewish lending.[63]

Finally, note that persecution probability increases to about 0.8 when mortality reaches 16%, and then decreases to 0 as mortality increases to 80% (see Fig. 3b). This suggests that the complementarities effect became strong around that turning point. We verify in Web Appendix Table A.17 that the city characteristics shown in Table 2, as well as whether a city had Jewish moneylending, do not differ between cities above and below that turning point. The complementarities effects are not an artifact of compositional changes.

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60 This hypothesis is advanced in Stein (2007, 442) who notes that the First Crusade had a major "impact on the status and livelihood of the Jews in France, Germany, and England drove them out of trade through the lack of security arising from the inimical attitude of society in general".

61 Note that since the First Crusade pogroms reduced the protective effect of mortality (see row 10 of Table 8), not simultaneously controlling for the interaction of the first crusade and mortality should, if anything, make us under-estimate the protective effect of moneylending.

62 Florence was the most important financial center in fourteenth century Europe (de Roover 1963). Genoa, Milan, Sienna and Venice were also important financial and banking centers (Mueller 1997). Cahors in southern France was a city known for its Christian moneylenders (Noonan 1957). Cahorsins are explicitly mentioned alongside "Lombards" (Milan is the capital of Lombardy) as direct substitutes for Jewish lenders (see, e.g. the entry for "Switzerland"' in the *Encyclopedia Judaica*). Dorin (2013, 2016) discusses the extent to which the decision to expel Jews were intertwined with attitudes to Christian usury.

63 While we directly control for the individual effect of financial centers on persecution, the interacted effect could nonetheless be endogenous. However, as we discuss in Web Appendix Section 13, the prominence of Christian moneylenders or banks in these cities reflects long-standing historical factors specific to each city, and hence is less likely to be endogenous than is the location of Jewish moneylending.

### 5.4.3 Trade

Jews traditionally played an important role in trade (Botticini and Eckstein 2012). This role had become less significant by the fourteenth century. But they still operated in the Mediterranean trade and in local, land-based, trade. In this section, we explore how various indicators of Jewish involvement in trade interacted with plague mortality in determining the vulnerability of different communities.

*Log market access* We find that cities in the top 10% of the market access distributions in 1300 were more likely to persecute Jews on average (row 9 of Table 9). Note that the largest cities and the cities with a higher market access were not necessarily the same, since the coefficient of correlation between the two is 0.29.

*Sea-based versus land-based trade* The presence of Jews might have been more important for land-based trade rather than sea-based trade. Credit was vital for trade in medieval Europe as it was highly costly to move bullion over long distances. Merchants relied on credit to fund trade (see Spufford 1988). From the twelfth century, merchants began to innovate contractual forms to evade the prohibition on usury, many of which facilitated borrowing money for seaborne trade (Koyama 2010). The sea-loan, or *foenun nauticum*, allowed lenders to earn a return above the principle to compensate for the risks involved in sea voyage (Hoover 1926). Thus, the relative importance of the financial intermediation provided by the Jews would have been lower in cities engaged in long-distance seaborne trade. In these cities, merchants had sophisticated financial contracts that would enable them to lend and borrow without reliance on Jewish traders. This was not yet the case for inland trade as local merchants relied on Jewish moneylenders for credit.

Distance to the coast is another proxy for access to trade routes. We find no differential effects for towns along the coast (row 10). However, this masks the differential effects associated with the North-Baltic coast (row 11) and the Atlantic coast (row 12) in contrast to the Mediterranean coast (row 13). The protective effect of higher mortality is *reduced* for towns along the North-Baltic and Atlantic coasts. The specific non-effect for the Mediterranean coast further corroborates our hypothesis. While Jews did not participate in North-Baltic and Atlantic maritime trade, they remained active in the Mediterranean.[64]

Likewise, the protective effect of higher mortality is accentuated for towns at the intersection of two medieval routes (row 14, −0.009*). We also find a reinforced effect for towns at the intersection of two major Roman roads or a river (row 15, −0.007, and row 16, −0.004, but not significant).

*Trade networks* We find evidence of a stronger protective effect in Jewish communities that were better connected to other communities, using our Jewish centrality index (row 17, −0.010**). Next, we find no effect for market fairs, in which Jews were involved but did not have a specific comparative advantage (row 18, −0.001).[65] The protective effect of higher mortality is then reduced, but not significantly, for towns that were part of the Hanseatic League—a league of cities that dominated trade in the Baltic region and in which Jews were not involved at all (row 19, 0.005). However, the positive interacted effect becomes significant for towns that were capitals of the League (row 20, 0.009***). The effects suggest that where we have evidence that Jews were more involved in trade, the larger the protective effect.

[64] The Jewish Encyclopedia notes that while Jews were mostly concentrated in moneylending after 1200, they could be found involved in trade in cities like Marseille and cities in Castile and Portugal where they continued to engage in commerce with the Muslims states of North Africa (Gottheil et al. 1906).

[65] Jew merchants, for instance, frequented the fairs of Cologne, Frankfurt, and Friedberg (Reviv et al. 2007, 82).

### 5.4.4 Economic consequences of persecutions

To provide further evidence for the complementarities effect, we study the correlation between population growth and the presence of Jews in, or their disappearance from, the town. If Jews' skills are particularly useful to the economy of cities, we expect their presence in/disappearance from a town to be correlated with the relative growth/decline of that town.

*Jewish presence and growth* We have data on Jewish presence and population for 1869 towns $\times$ 9 periods from 1100 to 1850, hence 16,821 observations.[66] For town $i$ in year $t$, we regress log population ($log\ L$) in $t$ on a dummy equal to one if Jews are present at any point ($J$) in $[t-1; t]$:

$$\log L_{i,t} = \kappa J_{i,[t-1;t]} + \beta_t M_{i,1347-52} + \xi_i + \lambda_t + \mu_{i,t} \ . \tag{3}$$

We include town fixed effects ($\xi_i$) and year fixed effects ($\lambda_t$), so the identifying variation comes from towns for which the Jewish presence dummy $J$ changes, either because Jews enter the town or leave the town following a persecution (Jews rarely leave a town on their own). $\kappa$ thus measures the effect of changes in Jewish presence. Standard errors are clustered at the town level.

Note that we control for the Black Death mortality rate ($M$) interacted with year fixed effects. Indeed, we show that mortality affected persecution probability, whereas mortality may have had long-term effects on city growth (Jebwab et al. 2016), hence the need to control for it. Also, we use the extrapolated mortality rates to avoid losing observations (see Sect. 4.3). Lastly, these effects should be interpreted with caution as changes in Jewish presence are likely endogenous. Other factors could cause both Jews to enter/leave a town and the relative growth/decline of that town.[67]

In row 1 of Table 10, Panel A, we show that towns with a Jewish community were growing 32% faster (over a period of about one century). The point estimates decrease, but remain high and significant at 23%, when we add the lag of log population in $t-1$ (row 2) in order to control for mean reversion. This effect is comparable to the magnitude found by the potato Nunn and Qian (2011) and about half the size of the effect of the printing press found by Dittmar (2011).[68] In row 3, we show that the effect of the Jewish presence dummy is reduced when we add the share of years in which Jews were present in $[t-1; t]$ and whose effect is large (0.27***). In row 4, we add a dummy for whether Jews were present at any point in the previous period [$t$-2;$t-1$] and interact it with our main variable of interest, the Jewish presence dummy in [$t-1$;$t-1$]. In doing so, we estimate separate effects for *entries* and *exits* (primarily because of a persecution). The effect of Jewish presence is stronger for entries (0.35***, row 4) than for exits (0.12***, so $-$0.12*** if Jews leave, significantly different at 1%). In row 5, we add a dummy for whether the year $t$ is strictly after the fourteenth century, so $t > 1400$ (and $[t-1;t] > [1300;1400]$) and interact it with our main variable of interest. In this way we can estimate separate effects for the eras before/during and after the Black Death. We find that the effect of Jewish presence was not significantly different between the two eras (row 5, 0.30*** vs. 0.39***).[69]

[66] Periods: 1100–1200, 1200–1300, 1300–1400, 1400–1500, 1500–1600, 1600–1700, 1700–1750, 1750–1800 and 1800–1850.

[67] Note that mortality is not a valid instrument for persecutions since it may affect future city growth.

[68] Specifically, it is comparable to a 1/3 improvement in potato suitability. In a separate paper, Johnson and Koyama (2017) use several identification strategies to further investigate the role of Jews in European city growth in 1400–1850, and find similar growth effects of 30% per century.

[69] Web Appendix Table A.20 shows baseline results hold if we use: (i) two lags of log population, to control for past growth trends, (ii) drop towns not in the Bairoch sample; and (iii) do not replace by 500 inhabitants the

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We can also explore whether cities that massacred their Jewish communities suffered a growth penalty. Such a growth penalty is suggested by contemporary sources which report cities resettling Jewish communities after the Black Death, specifically to replenish tax coffers and to rebuild the economy (e.g. Ephraim 1922). For the same panel sample of 16,821 observations, and for town $i$ in year $t$, we regress log population ($log\ L$) in year $t$ on a dummy $P_{i,1347-52}$ equal to one if there has been a persecution in the town during the Black Death period (1347–1352). Since this dummy is time-invariant, we interact it with a dummy $\mathbb{1}(t > 1400)$ for whether the year $t$ is strictly after the fourteenth century, so $t > 1400$ (and thus $[t-1;t] > [1300;1400]$):

$$\begin{aligned} \log L_{i,t} = \ & \psi P_{i,1347-52} \times \mathbb{1}(t > 1400) + \pi J_{i,1347-52} \times \mathbb{1}(t > 1400) \\ & + \kappa' J_{i,[t-1;t]} + \beta_t' M_{i;1347-52} + \xi_i' + \lambda_t' + \mu_{i,t}' \ . \end{aligned} \quad (4)$$

$\psi$ is our main coefficient of interest and measures the effects of persecutions on town growth in the centuries following the Black Death. Since we use the full sample of 1869 towns, including towns without a Jewish community circa 1347, we control for a dummy $J_{i,1347-52}$ equal to one if Jews were present in the town during the Black Death period, also interacted with the post-1400 dummy. Similar to Eq. 3, we add town fixed effects ($\xi_i$), year fixed effects ($\lambda_t$), the Jewish presence dummy ($J$) in $[t-1; t]$, and extrapolated mortality ($M$) interacted with year fixed effects. While Black Death mortality appears to be exogenous, note that Black Death persecutions are not, even if we control for mortality, so these results should also be taken with caution.

Row 1 of Panel B shows the effect of the Black Death persecution dummy after 1400 (relative to before 1400). The effect is negative, at −0.21*, implying that towns where Jews were persecuted in 1347–1352 grew relatively slower in the following centuries. An effect size of −21% is close to the effect of +33% for Jewish presence (see row 1 of Panel A). In row 2 of Panel B, we interact dummies equal to one if there had been a pogrom or an expulsion during the Black Death period with the post-1400 dummy (using the same framework as Eq. 4). The negative correlation between persecution and future growth is especially apparent when the persecution is a pogrom rather an expulsion (−0.31*** vs. 0.24, significantly different at 1%). By expelling instead of annihilating their community, towns may have been able to re-attract Jewish settlement. Consistent with the *complementarities* effect, towns with Jewish communities appear to have grown faster and towns that persecuted their communities appear to have grown slower in subsequent centuries.

Web Appendix Table A.21 examines whether these effects differed over time. In Eq. 4, we interact the Black Death persecution dummy $P_{i,1347-52}$ with dummies for each year strictly after the fourteenth century. This gives us the effect in each century relative to the first post-Black Death century. While pogroms have a negative but not significant effect in the first century following the Black Death, the effect becomes increasingly more negative, and significant, over time. Conversely, expulsions have a positive effect in the first century, which does not significantly change over time. One explanation is that towns which expelled rather than massacred their Jewish communities were more likely to be able to attract Jewish settlement in subsequent decades.[70]

Footnote 69 continued
population of the towns with a missing population, presumably because they were below the 1000 threshold used by Bairoch.

[70] Of course it is also possible that persecutions had institutional effects beyond the effects on Jewish community size, so we are cautious in interpreting these results.

Next, while we are always controlling for mortality and its effects over time in Eq. 4, one may wonder what the best scenario is for cities: having high mortality but low persecutions rates or having low mortality but high persecution rates. We thus examine which scenario was (not necessarily causally) associated with faster city growth. To do so, we classify cities into eight groups, depending on whether mortality was above or below median/mean mortality and whether a Black Death pogrom or expulsion was committed. We then use the same model as Eq. 4 but replace the persecution dummy and its interaction with the post-1400 dummy by six groups dummies interacted with the post-1400 year dummies, thus omitting the low mortality-no pogrom and low-mortality-no expulsion groups. Web Appendix Table A.22 shows increasingly more negative effects for the group with low (or high) mortality and pogrom than for the group with high (or low) mortality and no pogrom. This suggests that persecutions may have been more consequential than mortality in itself, especially in the longer run. This also shows that overall mortality in itself was not as consequential as the mortality of Jews, which is consistent with the *complementaries* effect.

## 6 Concluding discussion

We use the Black Death to study how a major shock affected the incentive to persecute Jewish communities in Europe. In theory, shocks can increase both the incentive to persecute a minority and simultaneously raise their economic value. Ultimately, the decision to persecute the minority depends on how the magnitude of the shock interacts with the utility one derives from persecution and the economic benefits associated with the presence of the minority. In line with the scapegoating hypothesis, we find that the Black Death led to the largest massacres of Jews prior to the Holocaust. However, at the micro-level, the intensity of the shock had a strong countervailing effect. Cities that were hit especially hard faced a demographic and economic crisis and were less likely to persecute their communities. We provide evidence that this micro-level variation can best be explained in terms of (i) a town's cultural inheritance and ideological predilection to scapegoat Jews, especially in a context where conspiracy theories abounded, and (ii) the economic role Jews played in a town. These results suggest that when there are latent biases against minorities, shocks can lead to these biases manifesting themselves as persecutions, with conspiracy theories one of mechanisms through which scapegoating spreads. However, when the minority and majority communities engage in economically complementary activities, then these relationships may be a powerful way to reduce inter-group conflict.

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