Locked Down, Lashing Out: Situational Triggers and Hateful Behavior Towards Minority Ethnic Immigrants

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Abstract

Covid-19 caused a significant health and economic crisis, a condition identified as conducive to stigmatization and hateful behavior against minority groups. It is however unclear whether the threat of infection triggers violence in addition to stigmatization, and whether a violent reaction can happen at the onset of an unexpected economic shock before social hierarchies can be disrupted. Using a novel database of hate crimes across Italy, we show that (i) hate crimes against Asians increased substantially at the pandemic onset, and that (ii) the increase was concentrated in cities with higher expected unemployment, but not higher mortality. We then examine individual, local and national mobilization as mechanisms. We find that (iii) a xenophobic national discourse and local far-right institutions motivate hate crimes, while we find no support for the role of individual prejudice. Our study identifies new conditions triggering hateful behavior, advancing our understanding of factors hindering migrant integration.

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1 Introduction

Exclusionary attitudes towards immigrants are on the rise in Europe: the number of native-born Europeans who oppose admitting ethnic minority immigrants into their country has increased by more than 30% in the past two decades.[1] Relatedly, far-right parties now control 10% of the seats in the European Parliament (Data Europa Portal).

Given that exclusionary attitudes can adversely affect immigrants' integration—both through public policies and day-to-day interactions between immigrants and the native-born—it is important to understand the drivers of such attitudes and how they are expressed. Covid-19, as an external threat to the social and economic order, is a likely contributor to dominant-group grievances, and might therefore find its expression in violent behaviors such as hate crimes. Using the case of Italy, one of the pandemic's earliest- and hardest-hit countries, we ask *whether* the Covid-19 pandemic has caused an increase in the incidence of hate crimes against minority immigrant groups. And if so —*why*?

Hate crimes against migrant groups have been increasing in many western democracies, even before the Covid-19 outbreak (Dancygier et al., 2020). Moreover, even if rare, hate crimes can have widespread impact on targeted communities. Following hate crimes, and other extreme forms of discrimination, members of targeted groups have been shown to modify their behavior: change how they dress, avoid certain locations (Perry and Alvi, 2012), relocate to 'migrant' neighborhoods (Bell, 2013), withdraw their kids from public schools (Fouka, 2020), and generally retreat into 'safe spaces' that minimize interaction with members of the native-born majority. Given the evidence documenting the negative relationship between hateful crimes and migrant integration (Dancygier and Green, 2010), and the positive spillover of successful integration (Hainmueller, Hangartner and Pietrantuono, 2015), whether and why Covid-19 caused an increase in hate crimes is a timely question with important implications for both theory and policy.

[1]Computed from the European Social Survey, 2002-2018.

Scholarly work has explained hate crime incidence as a reaction to real or perceived threats to the dominant group, particularly caused by either *broad structural changes*, including economic transformations and the increasing political power of previously disenfranchised groups, or as a result of *sudden life-threatening events*, like terrorism. While multi-faceted, Covid-19 has first and foremost triggered both a health crisis and a negative economic shock. To the extent that Chinese migrants (in Italy and elsewhere) have been portrayed as responsible for spreading the disease (Reny and Barreto, 2020), it is quite possible that being construed as posing a (health and/or economic) threat would unleash a violent backlash. Past work, however, provides mixed evidence suggesting this possibility is far from a forgone conclusion.

The literature linking economic threat to intergroup violence has generally focused on the role played by increased competition over scarce resources (Olzak, 1994; Dancygier, 2010). In these cases, resource competition is caused by economic decline due to long-term structural changes (such as globalization, automation, or migration) that threaten to disrupt existing social hierarchies. Methodologically, with few exceptions (e.g., Sharma, 2015), past work has tested the economic decline hypothesis cross-sectionally. In response to mixed findings questioning whether economic downturn causes an increase in hate crimes, some argue that economic decline contributes to hate crime incidence only in the presence of other supporting factors, such as an extreme-right government (Green, Glaser and Rich, 1998).

The threatening events literature—which has developed separately from the economic factors research—has narrowly focused on minorities perpetrating terror attacks (Deloughery, King and Asal, 2012) or serious crimes such as sexual assault (Jäckle and König, 2018). Offering a psychological (rather than structural) explanation for hate crime, these studies argue that such events, which are massive but punctual, instill fear that is directed at the out-group. From this perspective, health threats can be thought of as a particular type of threatening event, in which the threat of infection triggers fear and disgust, provoking in turn stigmatization and xenophobia (Taylor, 2019). Indeed, past studies have documented a relationship between disease outbreak and exclusionary attitudes towards migrants and other minority groups (e.g. Navarrete and Fessler, 2006),[2] but it remains unclear whether those attitudes are enough to spark anti-migrant crimes.

We bridge the economic threats and threatening events literature with the case of Covid-19, as an example of a sudden threatening event which *also* has economic consequences, plausibly thus having both structural and individual-psychological influence on intergroup violence. We complement the economic threats studies with an event that may affect hate crime but not through long-term economic restructuring. Nor is this a case of resource competition, given that in Italy, like in other OECD countries, Chinese immigrants do not usually work in the industries most affected by the pandemic, like hospitality.[3] And while the threatening trigger events studies mainly focus on fear to explain violent retribution against out-groups, our case accounts also for the possibility of scapegoating (Snowden, 2019); blaming an out-group for grievances they did not cause.

We employ a dataset of hate crime across Italy starting in 2007 compiled by a local NGO. We classify the victims' ethnic background from the description of the crime, and test whether the onset of the pandemic increased hate crimes against Asians. Using a generalized fluctuation test on the trend of monthly hate crimes, we confirm a structural break that coincides with the pandemic outbreak, corresponding to an eight-fold increase in hate crimes in February-March 2020, compared to pre-Covid levels. We further show that the increase in hate crime was limited to Asians—that is, it did not spill over to other migrant groups. Finally, we demonstrate that while hate crime incidence dropped following the February 2020 peak, crimes against Asians remained higher than before Covid-19, suggesting some persistence in the effects of the triggering event a year into the pandemic.

[2]Though see Adida, Dionne and Platas (2018) who did not find that the Ebola crisis worsened attitudes toward Africans in the USA.

[3]Most Chinese migrants in Italy are employed in commerce (67%) and in specialized industrial manual labor (18%) (Ministry of Labor and Social Policies, 2018, p. 42).

Second, we analyze the spatial distribution of hate crime to learn about its causes. In particular, we test whether hate crime incidence was more likely to increase where the effect of the pandemic on (a) unemployment and (b) mortality was most pronounced. Using the employment sectoral composition of Italian municipalities to measure Covid-19's local economic impact, and a difference-in-differences (DiD) estimation strategy, we find that the increase in hate crime against Asians was concentrated in municipalities that were most exposed economically. By contrast, we do not find evidence that Covid-19's *localized* health impact, measured using excess deaths, is responsible for the increase in hate crimes.

How does an economic shock increase hate crimes against a migrant out-group, when that group is not competing directly with the native-born in exposed occupational sectors? We explore three possible mechanisms, described, but rarely tested by the literature: (1) *individual* activation of pre-existing prejudice (proxied using right-wing parties' vote share in national elections); (2) *local* mobilization facilitated by political opportunistic behavior (measured using an indicator of extreme-right mayors, along with an assessment of their social media statements) and (3) *national* mobilization caused by a perceived change in social norms condoning exclusionary attitudes against Chinese people (measured using national newspaper and Twitter data, and a sentiment classifier in Italian). We find support for national media mobilization and the effect of local-level political factors, but not for prejudice. We advance research relating threat and fear to anti-migrant violent behavior, and in doing so we contribute to the understanding of factors hindering migrant integration.

2 Theoretical Framework

While the precise definition of hate crime varies across jurisdictions, there is a consensus that such crimes are motivated by animus against a targeted group—defined by its religion, race, ethnicity, gender or sexual orientation—in an attempt to harm that larger group, not just the individual victim (Green and Spry, 2014). Hate crimes are therefore message crimes, with the purpose of frightening vulnerable out-group members. In exploring Covid-19's possible effect on hate crimes, we build on three related bodies of work: threat theory, resource competition, and the trigger events literature.

Threat Theory Psychological accounts of hate crime assume that certain cognitive and affective processes, in particular, threat, fear, frustration displacement and stereotypical beliefs, lead perpetrators to identify targets and to take action against them, in order to satisfy a need for revenge, or as a means to remove what is perceived as a growing threat.

Past work has focused on identifying the contextual factors that give rise to those cognitive and affective dispositions. Threat, particularly is thought to be the result of structural changes that peril to disrupt existing social, economic and political hierarchies, and thus facilitate a backlash against out-groups perceived to be 'upstarts'. Contextual factors, which are generally assumed to unfold over a protracted period, include desegregation of workplace (Olzak, 1994) and neighborhood (Green, Strolovitch and Wong, 1998), and change in relative group size (Quillian, 1995), or political power (Dugan and Chenoweth, 2020). In these studies, hate crimes result from efforts to preserve the status quo in the face of a changing landscape that is both destabilizing and threatening.

Economic Decline Recent studies have linked anti-immigrant attitudes to changes in labor market conditions (Dancygier and Donnelly, 2013), recessions (Isaksen, 2019; Anderson, Crost and Rees, 2020) and economic hardship more generally (Czaika and Di Lillo, 2018). Moreover, the prejudice literature has long described a positive relationship between economic decline and violence against marginalized out-groups (D'Alessio, Stolzenberg and Eitle, 2002). The primary explanation for exclusionary attitudes and violence in these studies is that members of a dominant group are more likely to face real (or perceived) competition over resources with members of marginalized groups when they experience job and income loss. Such competition translates into hostility when minority group members are construed as a threat to the dominant group's economic security, interests and status (Quillian, 1995).

While the evidence that economic downturn increases exclusionary attitudes is rather robust, the evidence with respect to hate crime is mixed (Dancygier and Green, 2010; Green and Spry, 2014). Some studies suggest that hate crime increases with economic hardship. A positive association between hateful behavior and unemployment linked to intergroup competition has been observed in the USA (Medoff, 1999), the UK (Dustmann, Fabbri and Preston, 2011) and Germany (Falk, Kuhn and Zweimüller, 2011). Closely related, Dancygier (2010) shows that local economic deprivation is associated with higher rates of violence against immigrant-origin minorities in the UK, a result the author explicitly ties to greater economic competition. Other studies, however, do not find a negative relationship between local economic conditions and hate crime (e.g. Krueger and Pischke, 1997). Green and Spry (2014) argue that such evidence is inconclusive, in part, because past work has been insufficiently sensitive to statistical inference.

The literature on economic decline and hate crime suffers from two additional shortfalls. Theoretically, the focus is on resource competition, failing to account for cases, such as our own, in which the targeted group does not directly compete with the dominant group over resources. In addition, past studies (e.g. D'Alessio, Stolzenberg and Eitle, 2002) have generally sidestepped temporal considerations by assuming that worsening economic conditions, especially job and income loss, unfold over a protracted period. In turn, leaving out discussions of economic shocks, past empirical studies have focused on cross-sectional variation rather than on change.

Threatening (Trigger) Events The economy-centric studies of hate crime have, to date, developed independently from another branch of the literature that, following the 9/11 terror attacks, explored the role that dramatic events play in triggering anti-minority actions. The hypothesized mechanism is that threatening events, such as terror attacks, increase fear and distort the estimates of the risk posed by ordinary members of the out-group associated with the event. Trigger events are thus assumed to affect psychological dispositions but not structural conditions. This may explain why to date, with a few exceptions (e.g. Frey, 2020), trigger events studies have narrowly focused on demonstrating how a galvanizing event increases overall hate crime incidence (e.g. Hanes and Machin, 2014), but have not explored how it may have changed the spatial distribution of such crimes.

Epidemics are one type of threatening event: an outside unknown invader that attacks a population unprepared, triggering a sudden spike in mortality, and instilling fear (Snowden, 2019); particularly fear of being in close proximity to 'others' associated with the disease origin. Like the trigger events literature, scholarly work relating infectious diseases to stigmatization of out-groups provides an individual-psychological explanation. According to this literature, stigmatization is caused by our behavioral immune system (BIS)—a collection of psychological mechanisms that detect cues to the presence of infectious pathogens, triggering fear and disgust that motivate disease avoidance—which has evolved to being overly sensitive to cues that only superficially resemble environmental signs of infection, like skin color. Consistent with this response of the BIS, people who fear becoming infected might tend to blame out-groups for spreading the disease at higher rates (e.g. Aarøe, Petersen and Arcenaux, 2017). While past research generally finds a link between epidemic outbreaks and stigmatization of minority groups (Snowden, 2019), it remains unclear whether such attitudes translate into violence. Furthermore, to our knowledge, there is no work assessing whether during an epidemic actual exposure to the risk of infection (here in terms of number of deaths), may manifest in violent behavior against an out-group.

Disease avoidance can explain how an out-group which is not the vector of the disease can nevertheless be held as responsible. A similar process can happen when an unexpected economic crisis, rather than a health threat, is the trigger. From a psychological perspective, this form of scapegoating serves as an opportunity to making sense of personal failures (and/or socially shared frustrations) while maintaining one's positive self-image (and/or positive group identity) in the face of deteriorating conditions. This in turn can push individuals from dominant groups to lash out against vulnerable scapegoats (Pinderhughes, 1993), loosely responsible for the sense of deprivation, *even if the out-group is not competing with the dominant group directly over tangible resources.*

We thus bridge the divide between the economic threats and threatening events literature by calling attention to the fact that certain salient events may themselves affect structural conditions, triggering fear, anxiety and frustration directed at an out-group in the form of violence. Rather than narrowly focusing on trends in overall economic conditions, research tying economic factors to hate crime, we argue, should also explore the implications of rapid and unexpected change. We are unaware of previous research testing whether economic (and health) *shocks* lead to an increase in the incidence of hate crime.

3 Covid-19 and Chinese Immigration into Italy

Italy was hit early by the pandemic. The first confirmed cases of Covid-19 were two Chinese tourists in Rome on January 31, 2020. Italy's early rate of infection was high: with intensive care units exceeding capacity and no treatment yet available, the number of deaths increased rapidly, reaching 12,000 by the end of March.[4] The economic crisis followed suit. By February 22, the government ordered the lockdown of 10 municipalities, a decision extended to most of Northern Italy on March 8 and to the entire country the following day. On March 13, non-essential businesses were ordered to close. Even before lockdowns came into effect, mobility had dropped dramatically (Cartenì, Di Francesco and Martino, 2020) with dire implications to the tourism, transportation, hospitality, and retail sectors. As a result, the salience of the economic consequences for these sectors in particular, but also for the economy as a whole, increased in the national media.[5] Accordingly, based on public opinion data published by the World Economic Forum, since as early as February, Italians perceived the pandemic as

[4]ISTAT, Report on Covid-related Mortality, available at https://tinyurl.com/2bntletb.

[5]See, for example, La Repubblica, March 23rd, 2020.

posing a high threat to their jobs and businesses, but less so to their health. Consistently, from social media behavior, we find that early in February, people were more concerned about the pandemic's outbreak in places that rely economically on the hospitality industry.[6]

From early on, the Covid-19 outbreak was attributed to China. During January and February 2020, the Italian media reported rather extensively on the origin of the virus in the Wuhan province. As in other countries, far-right elites framed the pandemic as the direct responsibility of people of Chinese origin (Vachuska, 2020). Luca Zaia, the governor of Veneto and emerging leader of the anti-immigration Lega party, suggested that Covid-19 was caused by poor Chinese hygiene (NY Post, February 29, 2020). Other local politicians echoed the message. For example, the right-wing mayor of Solto Collina (in Bergamo), tweeted death wishes to Chinese Covid-19 patients (Open Online, February 22, 2020). However, not all politicians followed this example. Again in Bergamo, the mayor of Gori and his cabinet dined at a Chinese restaurant to express solidarity with the local Chinese community (L'Eco di Bergamo, February 11, 2020).

In early 2020, the Italian media started reporting racially motivated attacks against Asians. Incidences of Sinophobia included, among others, a Chinese-origin Professor at the University of Milan verbally abused on a train (Twitter, February 22, 2020), Chinese tourists insulted and spat on while visiting Venice (TGR Veneto, January 26, 2020), and children with Chinese-origin parents prevented from attending school allegedly to reduce the risk of contagion (Corriere del Veneto, January 28, 2020).

It is, however, unclear whether these Sinophobic attacks constitute a significant break from pre-pandemic crime rates against Asians. The Chinese immigrant community in Italy is both large (over 300,000 residents) and established, with immigration waves taking off in the 1980s (Zhang, 2019). As a result of Italy's *jus sanguinis* citizenship policy, Chinese immigrants, however, are not well integrated: only 20% of the second generation identifies as Italian (Marsden, 2014). Weak national identity coupled by entrepreneurial success has

[6]We present the details of these analyses in Appendix K.

in turn, lead many native-born to perceive Chinese Italians as a suspicious 'out group' (BBC News 13, April 13, 2007). Accordingly, episodes of hateful behavior against Chinese at the pandemic's onset might not necessarily imply a significant break from past behavior. Additionally, it is unclear whether Covid-19 triggered an increase in hate crime that is specifically targeted against Asians or that more broadly spilled over to other migrant groups.

4 Did Covid-19 increase hate crime in Italy?

4.1 Hate crimes data

Police data on hate crimes in Italy is only available at the national level, aggregated by year and does not include the victim's ethnicity (Ministry of Interior, Oscad). Instead, we use granular data from Lunaria, a non-governmental organization that curates information from the media, partner organizations, and direct reports to the NGO. The data collected by Lunaria are considered the country's most reliable and are used by international organizations like the UN High Commissioner for Human Rights and Human Rights Watch.[7] The number of hate crimes in Lunaria's dataset is about two-thirds of the hatefully motivated crimes registered by the police. Importantly, the distribution of the types of crimes Lunaria reports is comparable to those reported by the police, including violent crimes. By relying on information from a vast network of partner organizations, including more than 1,400 different local and online sources across the country, Lunaria's data is arguably less subject to issues of reporting bias which are common in hate crime datasets exclusively relying on news reports (Falk, Kuhn and Zweimüller, 2011).

Lunaria's data includes the date, the location, a description of the crime, and a classification of the type of hate crime (e.g., verbal abuse, physical violence). We use the description of the crime to extract information about the victims' background, particularly their country of origin and religion. Appendix A describes the process we devised to automate the

[7] See for example, OHCHR Report, 2019, Human Rights Watch, 2011.

extraction of victims' background information and Figure B1 presents the distribution of crimes by victim's origin and type of crime. During the period covered by Lunaria—from January 2007 to December 2020—a total of 7,895 hate crimes were recorded, and at least one took place in 1,327 of Italy's 7,943 municipalities.[8] Hate crimes against Asians are a rarer event: only 182 municipalities ever experience such crime during the period of observation. However, they are geographically dispersed across the country (Figure B2).[9] In the analysis below, we test for structural breaks in the trend using a subset of 4,670 hate crimes for which the description identifies an immigrant of a specific origin.

4.2 Results: National-level analysis

Covid-19's onset undoubtedly increased the incidence of hate crimes in Italy. Figure 1a shows the number of crimes against ethnic minorities and immigrant groups since 2007 (Figure 1b zooms to the period since 2018 and to crimes against Asians and Africans). Crimes targeting immigrants and ethnic minorities from any origin (*All*) follow a steady trend, with a large increase in 2014 due to the influx of Syrian refugees and African migrants into Italy. By contrast, hate crimes targeting Asians are around zero right until they peak dramatically in February 2020. Remarkably, in this month, Asian hate crimes exceeded crimes against African-origin immigrants, the most targeted group before the pandemic's outbreak.[10] With the restriction of movement measures imposed by the end of March 2020,

[8]Lunaria confirmed with the authors that they did not alter its data collection method and sources due to Covid-19.

[9]Political scientists often study rare but important events. For example, the probability of observing a civil war in a country during 1945–2016 is 0.012 (Sambanis and Schulhofer-Wohl, 2019), which compares to a probability of 0.022 of observing an Asian hate crime in a municipality in our sample.

[10]Lunaria's crime descriptions are usually insufficiently specific to distinguish East from South Asians in the main analysis. Nevertheless, where this distinction can be made, the

hate crimes against any group dropped below pre-Covid levels, increasing again by June when freedom of movement was restored, and dropping back again with the reintroduction of restrictions in October.[11] Importantly, during the period in between movement restrictions (July–October), crimes against African-origin immigrants increased back to pre-Covid levels, but crimes against Asians remained at above pre-Covid levels, suggesting that attacks against Asians persist over time.[12] However, compared to the February 2020 peak, the following peak (in July) was of lower intensity, which suggests as well that the xenophobic response is especially concentrated in the months just after the pandemic breakout. This is in line with previous studies finding immediate and short-lived xenophobic responses to triggering events (e.g. King and Sutton, 2013).

A generalized fluctuation test on the trend of monthly Asian hate crimes confirms a structural break due to Covid-19. As shown in Figure B3a, the empirical process, which captures the fluctuation in residuals of a linear regression of monthly crimes on the intercept, crosses the limiting processes boundaries, indicating that the fluctuation is improbably large, leading to reject the null hypothesis of no structural breaks. Reassuringly, the estimated optimal breakpoint is December 2019: just prior to the pandemic's outbreak. In addition, in Appendix G, we present a causal estimation of the pandemic effects on Asian hate crime. We find that in the months of February and March 2020, there are an additional 42 Asian hate crimes compared to the expected number had the pandemic not occurred.

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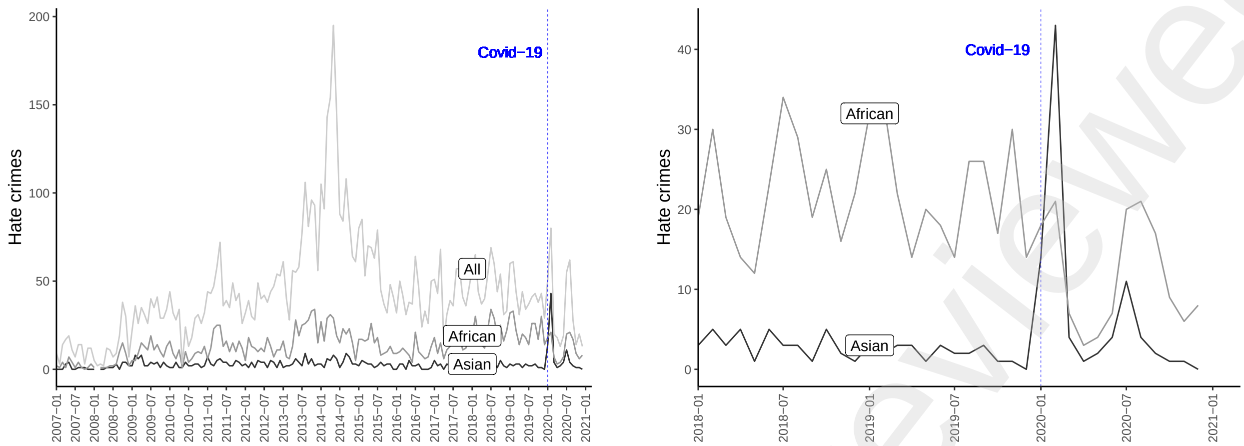
proportion of crimes against South Asians decreased from 65% to 7% with Covid-19, which suggests that pooling these two groups may be downward-biasing our estimate of the effect on East Asians.

[11]Hate crime incidence during the two strict lockdown periods (April–June and November–December) was no more than 45% of the average number of hate crimes committed in the same period over the four previous years (Figure C1).

[12]During this period, Asian hate crimes hit a record high compared to the same period in previous years (Figure C2).

Figure 1: Hate Crime Trends



(a) Hate Crime Trends

(b) Snapshot of Hate Crime Trends

Notes: *All* includes hate crimes targeting ethnic minorities and immigrant groups, regardless of having information about the victim's region of origin. *African* and *Asian* refer to crimes against identifiably African- and Asian-origin immigrants, respectively (b) shows trends since January 2018. Data source: Lunaria.

We find no evidence that hate crimes spill over to other ethnic minority groups: the generalized fluctuation test for Africans does not indicate a structural break at the pandemic's breakout (Figure B3b), and the average hate crime rate did not increase with Covid-19 (Table B1). Furthermore, the p-value of a Chow test assessing a structural break in the trend of African crimes in January 2020 is 0.34 while the p-value for the same test on Asian crimes is smaller than 0.0015. This result holds for all other non-Asian groups we tested, except for immigrants from Eastern and Southern Europe who experienced less rather than more attacks. We conclude that hate crimes against Asians increased as a result of Covid-19. In the next section we use the geographical distribution of hate crime to learn about its causes. Given that the restriction of movement measures greatly disrupted social dynamics, distorting, in turn, the patterns of hate crime, our analyses center on the months before such measures were imposed (until March 2020). Notwithstanding, in Appendix C, we show that the main results hold when we account for all months until December 2020.

4.3 Spatial Distribution: Local Health and Economic Environments

We turn to assess the motives for the observed increase in hate crimes against Asians leveraging spatial variation in (a) municipalities' exposure to pandemic-related deaths, and (b) pandemic-related unemployment.

Measurement of local exposure to infection To approximate individuals' perception of the threat of infection, we estimate excess deaths associated with Covid-19 in January 2020 in each of Italy's municipalities. We follow guidelines from the Centers for Disease Control and Prevention to compute the municipality's excess deaths by comparing the *observed* counts of deaths in January 2020 against its *expected* counts, estimated using monthly death trends since 2017 (from the Instituto Nazionale di Statistica (ISTAT)) and Farrington's surveillance algorithm (as implemented in the R package `surveillance`). We define municipalities with excess deaths above the median as high-exposure and those below as low-exposure. Given that Italy acknowledged the first case of Covid-19 at the end of January 2020, it is possible that January excess deaths do not entirely capture individuals' perception of infection exposure. However, as Figure B4 shows, January excess deaths is a good predictor of excess deaths in February, when the population was acutely aware of the disease threat. Moreover, January excess deaths already present a substantive uptick compared to previous months (Figure B5), suggesting that excess deaths in January is a good indicator of the severity of the pandemic's threat of infection. Importantly, the effects on hate crime are not sensitive to the choice of month in the computation of excess deaths.

Measurement of local economic vulnerability Using sectoral composition data of the local economy (from the 2011 Industry and Services Census), we construct a pre-treatment measure of municipalities' economic vulnerability to Covid-19.[13] We compute the share

[13] Dancygier and Donnelly (2013) and Anderson, Crost and Rees (2020) use a similar sectoral approach to measure the effect of local economic conditions on attitudes toward

of a municipality's workers employed in tourism, hotels, restaurants and transportation and define municipalities with a share above the median as high-exposure, and those below as low-exposure. Our sectoral measure captures pandemic-related unemployment accurately: since February 2020 these sectors were already expected to suffer from both underemployment and unemployment.[14] Moreover, our measure is an excellent predictor of the observed rate of regional unemployment in the first two quarters of 2020 (Figure B6).[15]

The death and unemployment exposure measures capture different dimensions of the pandemic's local effects. As shown in Figure B7, the correlation between exposure to deaths and unemployment is weak and somewhat negative. Municipalities with high exposure to unemployment have (somewhat) lower exposure to deaths.

Empirical strategy Neither excess deaths nor sectoral composition are randomly assigned, posing a challenge for causal inference from cross-sectional comparisons between high- and low-exposure municipalities. For example, municipalities that are more densely populated, are more likely to have more pandemic-related deaths and also more likely to experience hate crimes. As shown in Table B2 and B3, prior to the pandemic high- and low-exposure municipalities differ in important ways. We address this inferential challenge using a DiD approach, which estimates the differential change in hate crimes over time in high- and low-exposure municipalities. This strategy accounts for fixed municipal-level characteristics that may determine both hate crime and exposure to death or unemployment, such as the pre-existing level of social capital, degree of cosmopolitanism, and level of economic development. Causal identification of the death and unemployment effects on hate crime relies

migrants and racial animus, respectively.

[14]See Figure 11 in ISTAT Monthly Report, March 2020 reporting expected losses by sector. Transportation (-3%) and hospitality (-11%) are the most affected sectors.

[15]Employment sectoral composition is rather steady across time; the correlation between the number of workers in the sectors of interest in 2011 and 2018 at the regional level is 0.93.

on the assumption that high- and low-exposure municipalities would have followed parallel trends in the absence of the pandemic outbreak. We show that this assumption holds in Figure 2.

Estimation For municipality g in month t, we estimate the following DiD model:

$$y_{gt} = \beta D_{gt} + X_g^{'}\lambda_t\gamma + \alpha_g + \lambda_t + \epsilon_{gt} \tag{1}$$

where D_{gt} is an indicator variable for above the median exposure to Covid-19 related deaths or unemployment during the months of February and March, 2020. α_g is a municipality fixed effect, accounting for time-invariant municipality characteristics, and λ_t is a month fixed effect that accounts for monthly shocks common across municipalities. Additionally, we control for pre-Covid available municipality characteristics that may affect hate crime and also determine the local disease and economic environments. For example, it is possible that municipalities with more migrants experienced a higher number of deaths (absent safe work practices for migrants), and at the same time, blamed the crisis on migrant communities, further causing more hate crimes. To account for this type of concerns, $X_g^{'}\lambda_t$ controls flexibly for the share of the foreign born population, population without a college degree, population 65 years and older, and the party of the head of local government elected between 2014 and 2019.[16] The standard errors ϵ_{gt} are clustered by municipality to account for serial dependence.

The dependent variable y_{gt} is the number of Asian hate crimes per 10,000 residents in municipality g and month t. $\hat{\beta}$ captures the differential effect of municipalities' exposure to pandemic related deaths or unemployment on hate crimes against Asians. We use a linear probability model as our preferred specification for two reasons: First, it allows to interpret

[16]The results reported in Appendix D are robust to including the population share of East and Southeast Asian immigrants. We do not include it in the main analysis because we have missing data for 7.5% of the municipalities.

the effect estimates directly from the coefficients. Secondly, given that many municipalities do not experience Asian hate crimes during our studied period, the effect estimates are produced effectively using only the data from the Asian hate crime-experiencing set of municipalities. Despite such sample selection, the effect estimates are neither biased nor inefficient, but the marginal effects would be biased under a non-linear model (Cook, Hays and Franzese, 2020). We use a linear model to avoid such bias. Notwithstanding, we show in Section 4.5 that the results are robust to relaxing the linearity assumption.

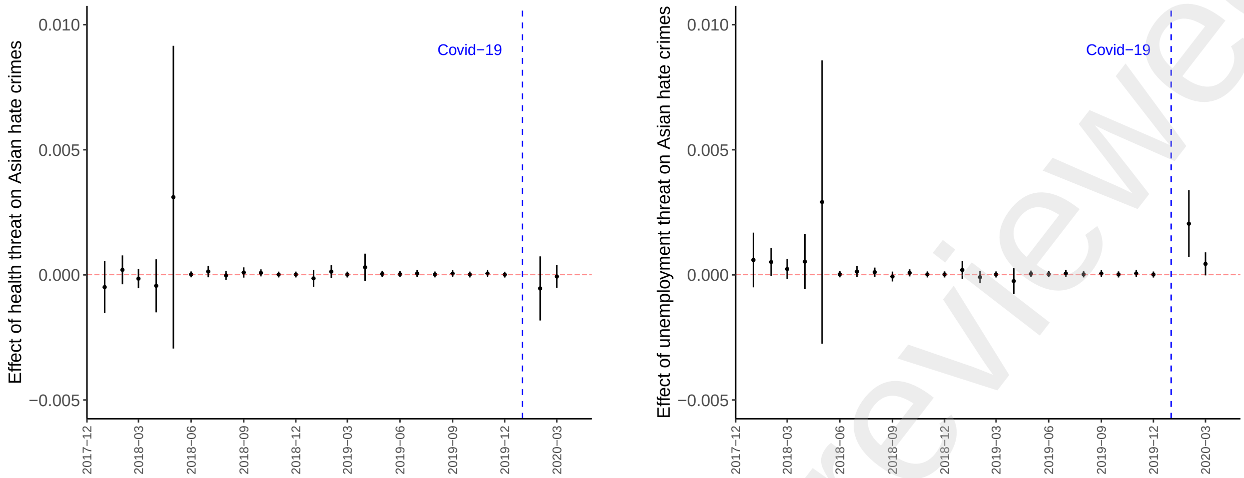
4.4 Results: Municipal-level Analysis

For illustrative clarity, Figure 2 presents a snapshot of the monthly-varying effects of Covid-19 death and unemployment on Asian hate crimes, and Figure B8 shows those effects for the entire period of analysis. The only statistically significant effects are the positive coefficient for post-Covid-19 unemployment, particularly in February 2020 (Panel 2a, Panel 2b). These patterns suggest that individuals exposed to a more threatening disease environment do not respond differently than individuals exposed to a less threatening environment. In contrast, individuals exposed to high economic distress do respond with more hateful behaviors against Asians than individuals exposed to less distress.[17]

Table 1 presents the estimated change in the number of Asian hate crimes per 10,000 residents in municipalities with relatively high deaths (Panel A) and high unemployment (Panel B) post pandemic outbreak. Column (1) compares the rate of Asian hate crimes between municipalities with high- and low-exposure to death and unemployment before and after the first confirmed case of Covid-19 in Italy. Column (2) introduces municipality and month fixed effects, and Column (3) flexibly controls for predetermined municipality-level variables. Given that not all of the pre-Covid-19 estimated treatment effects are exactly

[17]These patterns remain unchanged when we include data from the months after the restriction on movement measures were imposed, which greatly disrupted social dynamics (Figure C3 in Appendix C).

Figure 2: Snapshot of Monthly-Varying Covid-19 Effects on Asian Hate Crimes



(a) Health Threat Effects

(b) Economic Threat Effects

Notes: Points represent the estimated coefficients from linear regression of Asian hate crimes per 10,000 residents on the interaction between monthly indicators and municipality indicators for (a) above the median deaths associated with Covid-19 and (b) above the median share of workers in affected economic sectors by Covid-19. The model specification includes month and municipality fixed effects. Lines are 95% confidence intervals. Data are from Lunaria 2007 to March 2020, 2011 Industry and Services Census, ISTAT deaths counts 2017-2020. The plot presents a snapshot of the effects since January 2018.

zero (though none is statistically significant as shown in Figure 2), Column (4) adds group-specific linear trends to account for possible violations to the common trends assumption. In addition, Figure B9 indicates that, in particular for unemployment exposure, the results are robust to accounting for such violation when we instead follow a more conservative approach by estimating the *double* DiD estimator of Egami and Yamauchi (2019), as opposed to the standard DiD estimator. Finally, Column (5) also introduces province-specific time trends to account for unobserved confounders at the province level that change over time and may affect hate crimes against Asians smoothly over time.

Starting with Panel A, across all models specifications, we do not find evidence that variation in disease exposure can help explain the spatial distribution of hate crime in Italy in the months following the Covid-19 outbreak. Table B4 confirms these results when we instead estimate the effects on hate crimes of exposure to deaths in February 2020, when

Table 1: Covid-19 Effects on Asian Hate Crimes

	Dependent variable: Asian Hate Crimes per 10,000 residents				
	(1)	(2)	(3)	(4)	(5)
Panel A: Health Threat					
Excess deaths	−0.00000				
	(0.00006)				
After Jan, 2020	0.00067*				
	(0.00032)				
After Jan, 2020 × Excess deaths	−0.00031	−0.00031	−0.00083	−0.00091	−0.00091
	(0.00035)	(0.00035)	(0.00047)	(0.00048)	(0.00048)
Average Hate Crimes	0.00015	0.00015	0.00017	0.00014	0.00014
R^2	0.00000	0.00650	0.00706	0.00706	0.00713
Obs	1253594	1253594	1253594	1253594	1253594
N Municipalities	7885	7885	7885	7885	7885
Panel B: Economic Threat					
Expected unemployment	−0.00002				
	(0.00006)				
After Jan, 2020	−0.00010				
	(0.00008)				
After Jan, 2020 × Expected unemployment	0.00124***	0.00124***	0.00087***	0.00084**	0.00084**
	(0.00036)	(0.00036)	(0.00026)	(0.00028)	(0.00028)
Average Hate Crimes	0.00016	0.00016	0.00014	0.00014	0.00014
R^2	0.00001	0.00651	0.00671	0.00665	0.00671
Obs	1259477	1259477	1259477	1259477	1259477
N Municipalities	7922	7922	7922	7922	7922
Month FE	N	Y	Y	Y	Y
Municipality FE	N	Y	Y	Y	Y
Flexible controls	N	N	Y	Y	Y
Group-specific linear trends	N	N	N	Y	Y
Province-specific linear trends	N	N	N	N	Y

The dependent variable is monthly Asian hate crimes in a municipality per 10,000 residents. *After Jan, 2020* indicates the period after the first confirmed case of Covid-19 in Italy. *Excess deaths* and *Expected unemployment* indicate municipalities with an above the median number of deaths in January 2020 associated to Covid-19 and share of workers in affected sectors by Covid-19, respectively. Flexible controls include municipality population shares of foreign born and less than college educated interacted with month indicators. The health threat specification includes as well share of the population 65 years and older, and the party label of the mayor interacted with month indicators. Group-specific linear trends and Province-specific linear trends correspond to the interaction between a continuous time measure and the municipality indicator of infection or unemployment exposure, or province indicators, respectively. Municipality-clustered-robust standard errors reported in parentheses. *Average Hate Crimes* is the effective sample mean pre-Covid-19 hate crime rate in control municipalities, computed following Aronow and Samii (2016). Data are from Lunaria 2007 to March 2020, 2011 Industry and Services Census, Istat death counts 2017-2020. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

the population was made more aware of the virus and its deadliness, compared to January 2020. The estimated null response to disease exposure is not explained by a larger decrease in the population's mobility in municipalities experiencing higher deaths, which would have limited more the rate of contact with the out-group, and therefore the opportunities for hate crime in these areas. Figure B12, which uses data available at the province level from Pepe et al. (2020), shows that the decrease in mobility across areas with high- and low-exposure to Covid-related deaths is very similar.

By contrast, in Panel B of Table 1, while pre-Covid Asian hate crimes are similar across municipalities with high- and low-exposure to pandemic-related unemployment, after the first Covid-19 confirmed case, Asian hate crimes somewhat decrease in municipalities with low expected unemployment (although such decrease is not statistically significant), but they significantly increase in municipalities with high expected unemployment. The magnitude of such relative increase is between 0.87 and 1.2 crimes per 10 million residents, depending on the model specification. This effect is equivalent to an increase of 795% relative to the average Asian hate crime rate in municipalities with low exposure to unemployment. This finding is consistent with the idea that the native-born react to economic grievances with hateful acts against a minority group associated with the origin of such grievance.[18]

4.5 Robustness Checks

To ensure that the Covid-19 related unemployment findings are robust, we fit a series of alternative specifications. First, we control flexibly for the pre-Covid municipality's size of the East and Southeast Asian communities to correct for potential bias from omitting this variable (Table D1). Second, we show that effects are not driven by our choice to dichotomize our measure of expected unemployment at the median by using a *continuous* indicator (Table D2) and by estimating treatment effects at different quartiles of the share of workers in affected economic sectors (Figure D1). Third, we account for potential time-varying endogenous municipality characteristics, and for changes in the reporting of crimes across time, by restricting the analysis to a shorter time frame of one year (Table D3).

Fourth, it is possible that the reporting of crimes, particularly from the Asian community,

[18]The conclusion of these main results hold when we include data from the months after the restriction on movement measures were imposed (Table C1 in Appendix C).

increased with the pandemic onset, and therefore what we are capturing is not a surge in hate crime, but an increase in the reporting of crimes. This is unlikely, given that Lunaria's reporting sources and data collection processes did not change after the virus's outbreak, and that to the best of our knowledge, there was not an Italian *Stop Asian Hate* movement empowering Asian communities to report crimes. Nevertheless, we assess this possibility by running a series of analyses which account for potential bias in the reporting of hate crimes around the onset of Covid-19 in cities with higher expected unemployment. We account for both selective increase in reporting and systematic location misattribution. All tests and results are presented in Section E. Fifth, we rule out that the increase in hate crimes is a response to Chinese tourists perceived as spreading the virus in Italy (Figure D2). Finally, we alter the model's functional form to account for nonlinear models and to deal with excess zeros in the outcome variable (Table D4). These checks and their results, which strengthen our confidence in the robustness of the findings, are described in Appendix D.

In addition to these robustness checks, we test whether the violent reaction to pandemic-related unemployment spills over to other migrant groups and find that it does not. Moreover, using an indicator variable of whether a municipality has at least one crime as the outcome, we show an increase in the number of municipalities experiencing hate crimes, in addition to an increase in the crime rate (the number of hate crimes per 10,000 residents). These analyses are in Appendix F.

5 Explaining Retaliatory Crimes Against Asians

In line with threat theory, we argue that pandemic-related unemployment increased grievances perceived to be inflicted by Chinese-origin people. However, we recognize that hate-motivated behavior may not be reduced to economic frustration (Dancygier and Green, 2010). In this section, we seek to advance an explanation for the link between hate crimes and the biased perception against Asian-origin people. Specifically, we evaluate whether hate crime is a response to the interaction between the economic environment and (a) the psychological attributes of potential perpetrators, and/or (b) the institutional environments in which perpetrators operate. Following Koopmans (1996), we begin by assuming that hate crime is an expression of a broader xenophobic social movement, and thus rely on the theoretical insights from the literature on reactive mobilization as a response to threat (van Dyke and Soule, 2002). We focus on three possible levels of reactive mobilization.

First, we consider whether the economic downturn increases the likelihood of hate crimes against a minority-immigrant group among those who are already explicitly prejudiced against members of that group. The idea is that negative cognitive and affective associations with stigmatized groups become activated at the onset of a disrupting event involving that stigmatized group (Fiske, 2002). If true, we should observe hate crimes manifesting in places characterized by high levels of exclusionary attitudes towards migrants.

Second, following Green, Glaser and Rich (1998), we examine the role local right-wing politicians may play in mobilizing hateful behavior. During economic crises far-right parties often target minority groups to channel voters' discontent toward out-groups alleged to be responsible for voters' grievances (Mudde, 2004). Consequently, members of those groups are framed as enemies and become legitimate targets of hateful acts. In addition, even in the absence of direct calls for violence against minority-immigrant groups, nativist politicians can indirectly legitimize such behavior via their positions towards immigrants (Jäckle and König, 2017). Moreover, a government by a right-wing mayor may cause potential perpetrators to feel that they can act violently with impunity, especially in places such as Italy where mayors have influence over the police (Romarri, 2020). Any of these channels can in turn embolden members of the dominant group to commit violent actions.

Third, we assess whether negative sentiment in the national public discourse encourages violent behavior. The pandemic outbreak increased the salience of China in the media, attracting more attention towards Chinese-origin people. At the same time, pandemic related grievances may be lashed out at members of this group, finding their expression in hate speech. Given such increased salience in the media, more people may express bigoted views that in normal times are censured and may condone others for the same behavior (Bursztyn, Egorov and Fiorin, 2020). These dynamics can normalize hateful speech and reduce its social sanction, legitimizing or even encouraging hate crime (Müller and Schwarz, 2020).

Measurement of individual predisposition to prejudice In the absence of public opinion data at the municipal level and given that Google Trend searches (as in Müller and Schwarz (2020)) are not reported in Italy below the provincial level, we assume that individual prejudice against minority-immigrant groups aligns with the political discourse and agenda of extreme right parties. Thus, following Fitzgerald, Leblang and Teets (2014), we proxy prejudice using the municipal-level vote share for extreme right parties in the 2018 national election.[19] We consider as extreme right parties those that espouse nativism and an anti-immigrant agenda.[20] In the analysis, we define municipalities with relatively high predispositions to prejudice when their vote share for the extreme right in national elections is above the median.

Measurement of local political mobilization We use an indicator of whether a municipality is governed by a far-right mayor. While municipalities with far-right mayors have more prejudiced individuals—they vote for the extreme right in national elections at higher rates, as shown in Figure B10, municipal elections are generally less ideological and more about electing an effective executive. Conceptually, our measure of local political mobilization is distinct from the measure of individual predisposition to prejudice in that it captures the institutional context that legitimizes xenophobia and that may mobilize and (at least

[19]In Appendix I we use available survey data to support the validity of our measure as individual prejudice.

[20]Italy's main extreme right parties are the Lega and Fratelli D'Italia. The variable also includes the votes for Casa Pound, Italia agli Italiani, Grande Nord and Blocco Nazionale Per Le Liberta'. Election returns are from the Ministry of Interior Open Data portal.

implicitly) condone its violent expression. Mayors have the means to articulate and legitimate local grievances as well as the jurisdiction over the local police forces, which may result in differential deterrence of hate crimes. We gather data on the local administrators from the Ministry of Interior's portal, Anagrafe degli Amministratori and we focus on the serving Mayor at the onset of Covid-19. We consider Mayors as far-right if they run with a party or a coalition which includes only far-right parties or with a Civic List which name includes an extreme-right political leniency label.

Measurement of national shift in social norms We use all articles from 2018–2020 published by every Italian national newspaper (17 in total) to capture the climate of public discourse. We focus on articles mentioning China or Chinese people. As a reference group, we look at articles that mention Africa or Africans. The sample includes 17,500 articles, 42% about China. We identify anti-Chinese and anti-African articles with a sentiment classifier for the Italian language. Appendix J describes the classification procedure. To approximate the monthly sentiment of Italian discourse about Chinese-origin (African-origin) people, we use the extracted articles to compute a measure of anti-Chinese (anti-African) articles as a proportion of all Chinese (African) articles. We replicate this analysis using Twitter data from a random sample of 1% of all tweets in Italian from January 2018 to April 2020, including about 95,000 tweets, 35% mentioning China.

5.1 Results: Individual-, Local- and National-level Mobilization

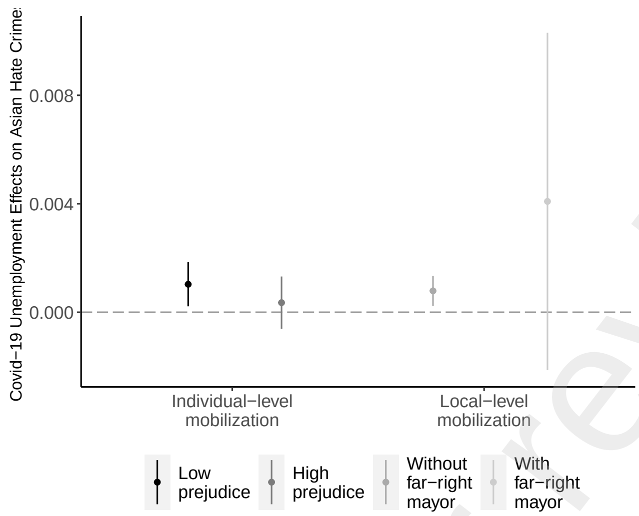
Figure 3 (left side) compares the Covid-19 unemployment effects across municipalities with low- and high-prejudiced population. We find that crimes against Asians increase more in municipalities with low prejudice, and while such increase is statistically significant, it is not statistically distinguishable from the effect in municipalities with high prejudiced individuals. This suggests that the observed increased hate crime in economically affected municipalities cannot be explained via the proposed *individual* mechanism. Simply having more individuals with prejudice dispositions in economically affected municipalities does not correspond with *differential* increase in hate crime.

Moving on to the *local* mobilization mechanism, the results presented in Figure 3 (right side) suggest that economic frustration alone can result in hate-motivated behavior, as the estimated coefficient of the Covid-19 unemployment effect on hate crime in municipalities without far-right mayors is positive and statistically significant. However, the results also suggest that local political elites may be playing a mobilization role, enhancing the hatred response to economic distress. The estimated effect in municipalities lead by far-right mayors is more than five times bigger the effect in municipalities without far-right mayors (although this effect is only statistically significant at the 10% (p-value of 0.07) computed via randomization inference), and the p-value of the t-statistic testing for equality of the two coefficients is 0.02, indicating that the two estimated coefficients are statistically distinguishable from each other.[21] This result is robust to flexibly controlling for vote share for the extreme right in national elections, confirming that this finding is independent and not driven by our measure of individual prejudice (Table I3). Tables H1 and H2 in Appendix H, present the estimated coefficients of the triple-differences models, which are depicted in Figure 3.

We analyze all Facebook posts of a small sample of 40 mayors in February and March 2020 to explore whether political rhetoric on social media can account for the difference in the Covid-19 unemployment effects on Asian hate crimes across municipalities without and with far-right mayors. While merely suggestive, we do not find evidence that far-right mayors used social media to broadcast hate-mongering political discourse to exploit the population's economic grievances. During these months, the information regarding China or Chinese people was neutral in tone, mostly about guidelines for people traveling back from China. It is possible, of course, that far-right mayors used other means to rally against

[21]Given that the number of municipalities led by far-right mayors is quite small, only 155 out of 7,922, we compute these p-values via randomization inference, by permuting the assignment of far-right mayors to municipalities.

Figure 3: Covid-19 Unemployment Effects Across Municipalities with Low and High Prejudice and Without and With Far-right Mayors

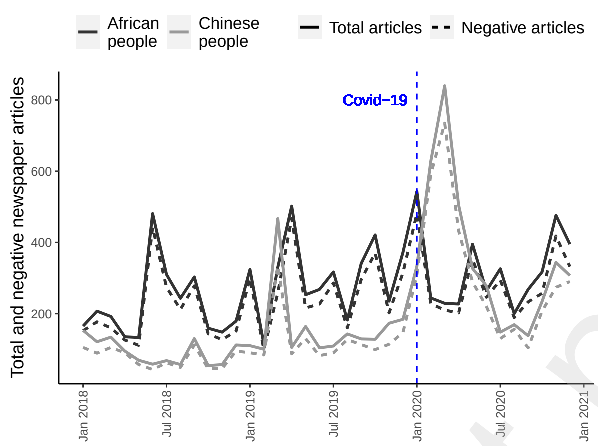


Notes: Points represent the estimated coefficients from linear regression of Asian hate crimes per 10,000 residents on the interaction between an indicator for municipalities with above the median share of workers in affected economic sectors by Covid-19 and an indicator for municipalities with high prejudice (*Individual-level mobilization*), and municipalities with far-right mayors (*Local-level mobilization*). The model includes month and municipality fixed effects, flexible controls, group-specific and province-specific linear trends. Lines are 95% confidence intervals. Data are from Lunaria, 2011 Census, and Ministry of Interior.

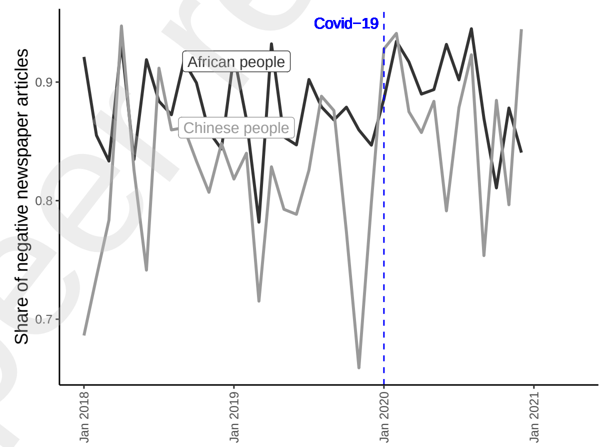
Chinese people. As mentioned, the institutional environment in municipalities lead by far-right mayors may also allow hate crime, particularly via the organizational bias of the police in failing to deter racist behavior and the fact that far-right politicians indirectly legitimize such behavior through their positions on immigrants and migration.

Finally, to assess the plausibility of a *national* shift in social norms we look at the trends of national newspaper articles referring to Chinese-origin people. The patterns in Figure 4a suggest that with the virus outbreak the salience of Chinese people in the public discourse increases; both the total number and the number of negative articles about people from China significantly increase, whereas as a reference, articles about people from African countries do not increase with the onset of Covid-19. Furthermore, a generalized fluctuation test on the trend of monthly negative articles about Chinese-origin people confirms a structural break due to Covid-19 (Figure B11). Not only Chinese-origin people are more salient in the public discourse at the onset of Covid-19, but also the pattern presented in Figure 4b suggests that the attitudes towards them are more negative than in previous months: the negative articles' share trend peaks with the pandemic's outbreak. We replicate these findings using Twitter data in Appendix J. In sum, we find evidence of a national public discourse shift against Chinese-origin people at the pandemic's onset. These findings suggest that increased hate crime is possibly explained as a response to bigoted community norms condoned by the national public discourse.

Figure 4: Italian Public Discourse Trends About Chinese People



(a) Total and Negative Articles' Trends



(b) Negative Articles' Share Trend

Notes: In (a) *total articles* indicates the monthly number of paper articles about Chinese- or African-origin people, and *negative articles* the monthly number of negative paper articles about these two groups. (b) displays the ratio of negative to total articles across the two groups. Articles are from the 17 Italian national newspapers.

6 Conclusion

Using the case of Italy, we show that Covid-19 caused a dramatic increase in the incidence of hate crimes against Asians, but not those against other migrant groups. Our study makes five unique contributions. First, we contribute to existing work on the relationship between pandemics and hateful behavior by treating the Covid-19 pandemic as a multi-faceted crisis that has consequences beyond health outcomes, and thereby demonstrating why the negative effect of the pandemic on hate crimes might be concentrated in areas that are not necessarily hardest-hit from a health perspective. It could be the case that we find no effect of increased threat of infection because when the population is threatened with severe infection the behavioral immune system is expected to be activated in almost everyone, regardless of localized level of threat. It is also possible that such a difference in response across economic and infection threats can be explained by the characteristics of the perpetrators of the crime, as it is more common that young individuals commit hate crimes (Craig, 2002), and they are also more likely to be exposed to the negative ramifications of unemployment (Davis and Von Wachter, 2011) rather than the health consequences of Covid-19.

Second, we advance the literature linking economic decline and hateful crimes. Past work has produced mixed results, in part because it generally used cross-sectional data (focusing on levels instead of change), and because it paid too little attention to causal inference (Green and Spry, 2014) and to regression models' functional forms. Using 13 years of monthly panel data, we show that sudden dramatic economic downturn can trigger an increase in hate crime, and that this result is robust to a variety of estimation strategies. We find that the reaction to structural economic conditions happens even in the absence of resource competition between the native-born and the migrant community, which leads us to advance the idea that job loss related to an economic shock can be scapegoated on an out-group. Moreover, our finding that differences in the violent reaction to unemployment are a function of local political context suggests that the local political environment can allow such scapegoating to occur.

Third, specifically with respect to Covid-19, we expand the study of the impact of the pandemic on Sinophobia both geographically and in scope. To date, most Covid-19 studies of Sinophobia consider the US context (e.g., Lu and Sheng, 2021), where such behavior might be driven by context-specific political factors that do not necessarily generalize elsewhere; for example, an ultra-nationalist president with an explicitly anti-China agenda (Müller and

Schwarz, 2020) during an electoral year. Moreover, while several studies have shown that the onset of Covid-19 is associated with an increase in *hate speech* against Chinese, at least on Twitter (e.g. Schild et al., 2020), we further look at its effect on hate crimes, which are driven by a different data generating process.

Fourth, we contribute to the growing literature on threatening events that can trigger increases in violent behavior. Trigger events have been assumed to change the psychological disposition (e.g., threat perception) of potential offenders, but not their environment (Disha, Cavendish and King, 2011). This may explain the exclusive focus on demonstrating a break from past trend, rather than variation in the spatial distribution of hateful behaviors in response to the triggering event (e.g., King and Sutton, 2013; Hanes and Machin, 2014). We show instead that some situational trigger events can themselves alter a structural condition (e.g., unemployment rate) that can contribute to intergroup violence. When this happens, the implications are not merely an increase in hate crime *incidence*, but also a change in *where* hate crimes are more likely to occur. Moreover, while past work focused almost exclusively on terror attacks conducted by members of a minority or immigrant group, we focus instead on an extra-social economic crisis.

Finally, we contribute to a literature studying the unequal impact of pandemics. Covid-19 had the largest negative effects on groups that were already worse off: women have been 24 percent more likely to permanently lose their job than men (Dang and Nguyen, 2021). In the US, people of color are more likely to die of Covid-19 and to lose their job (Gould and Rawlston-Wilson, 2020). Wealth inequality has increased across countries (Bottan, Hoffmann and Vera-Cossio, 2020) and poorer countries have yet limited access to vaccines. The increase in hateful behavior against minority immigrant groups we document is another dimension along which the pandemic disproportionately harmed minority groups.

We conclude that during a crisis causing economic grievance, ethnic minority and immigrant communities should receive better protection, especially in the most affected localities. However, we also acknowledge that protecting these communities may be challenging, particularly, considering the evidence we find suggesting that local institutions controlled by far-right politicians can escalate the hateful reactions to the crisis. Therefore, places where interventions to protect these communities are the most needed, are also places where we can expect the government to favor them the least.

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Appendices

— For Online Publication —

A Classification of Victims' Ethnic Background from Description of the Crime

To extract the victims' origins and religions, we first gather lists of countries and their adjectival and demonymic forms, ethnicities, and religions in the Italian language. In addition, we create a list containing terms which refer to immigrants, refugees and asylees (e.g. *immigrati*, *stranieri*, *clandestini*, *rifugiati*, which mean immigrants, foreigners, illegal immigrants and refugees, respectively). Secondly, in order to capture only the crimes against ethnic minority and immigrant groups, we remove from these lists all n-grams which are homonyms (e.g *Mali*, the West African country is the word *evil* in Italian), particularly homonyms of Italian place names (e.g. *roma* the Indo-Aryan ethnic group also means the capital of Italy), and n-grams that do not indicate an immigrant origin (e.g. *italia*, *italiana*). Thirdly, we match exactly the country, nationality and ethnicity n-grams to the text of the crime's description and we map the matching n-grams to countries and then to regions of the world to assign a country and region of origin to the victim, and we follow the same process to assign religions. Likewise, we define the victim as an immigrant when the text of the crime's description matches exactly at least one term in the immigrant keywords list.

As a result of these processes, we were able to identify at least one characteristic of the victim as an immigrant or ethnic minority for 92.7% of the total crimes. The description in the majority of the remaining 7.3% cases (577 crimes) does not specify any characteristics of the victim's origin. Among the cases with at least one identified characteristic, 93% (or 6,796 total crimes) correspond to immigrant victims, from which 4,670 are classified into a region of origin —with 48% assigned to Africa, 27% to Eastern and Southern Europe, 11% to Asia, 6% to the Middle East and the rest almost equally divided among other regions— and 2,126 are classified as immigrants, but with an unknown region of origin. The remaining 7% (522 crimes) corresponds to crimes against ethnic minorities based on their religion; about 56% of these are crimes targeted at Muslim people, 24% at Jewish people, 15% at Christians and the rest are targeting people from other non-dominant religions in Italy. Figure A1 presents a summary of the classification results. In the analysis of the Covid-19 effects on hate crimes we use the sample that we can classify as crimes against ethnic minorities and immigrant groups, which corresponds to 92.7% of the total crimes, and we focus on the sample with an assigned region of origin, which includes 4,670 crimes, when we assess crimes targeted at a specific immigrant group. As illustrated in Figure A2, neither the proportion classified as crimes against immigrants or ethnic minorities, nor the proportion which is classified into a region of origin is distinguishable across months before and after Covid-19.

Figure A1: Result of the hate crimes classification process

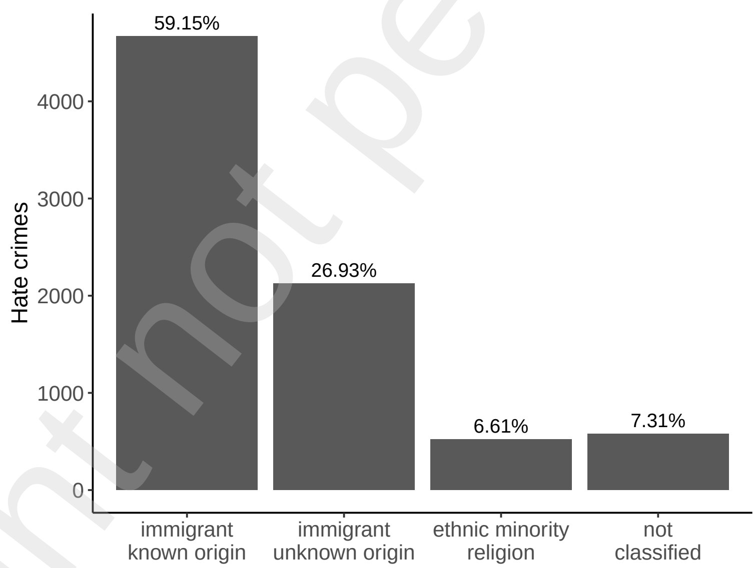
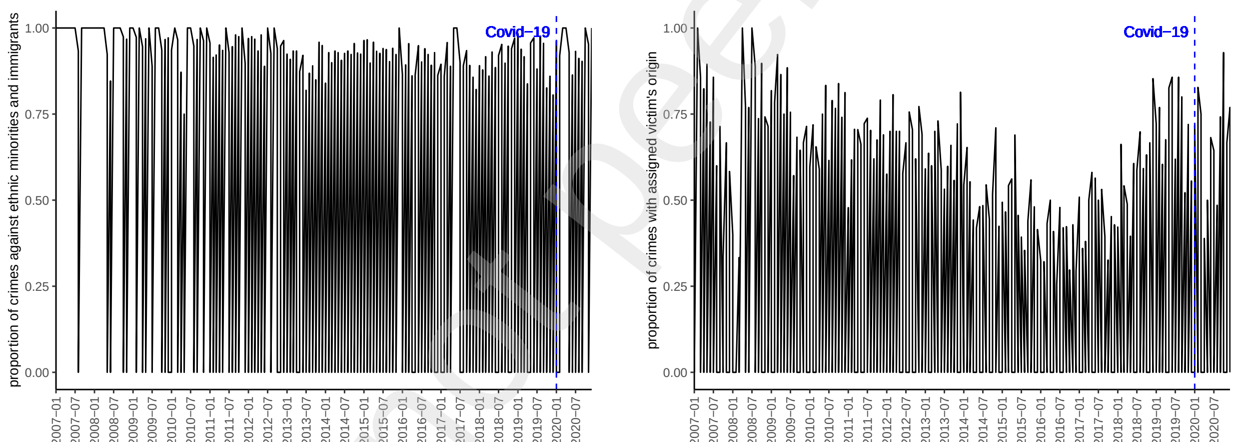


Figure A2: Proportion of classified crimes against ethnic minorities and immigrant groups, and with known victim's region of origin



B Additional Tables and Figures

We start by providing additional details on our dependent variable in Figures B1, B2, B3a and B3b. We plot the frequency of hate crimes by the victim's region of origin, identity of the perpetrator and type of hate crime (Figure B1) and map the distribution of hate crimes across Italian municipalities (Figure B2). In Figures B3a and B3b we present the results of a generalized fluctuation test for structural breaks, which reveals a large break in trends in crimes against Asians in January 2020, but no significant break in crimes against Africans in the same period.

We then include information on our treatments of interest, exposure to Covid-related deaths and unemployment. We start by presenting descriptive statistics by above and below median levels of exposure deaths (Table B2) and unemployment (Table B3). The two tables report the mean and standard deviation of hate crimes, socioeconomic variables and voting behavior before the start of Covid-19. The differences existent between cities in our treatment and control group before Covid, displayed in the last column in the tables, are differenced out by our estimation strategy, which absorbs pre-treatment differences in average outcomes across groups. Focusing on Covid-related death, Table B4 repeats our main DiD analyses on mortality using exposure to deaths in February 2020 rather than in January 2020. Even when the population is fully aware of the virus and its deadliness – unlike in January, when Covid had just started spreading – exposure to mortality does not predict a higher likelihood to observe hate crimes against Asians. Again focusing on mortality, Figure B4 shows the relation between predicted and observed probability of deaths, indicating that places with high mortality in January 2020 are a good predictor of where high mortality will be concentrated in February 2020. Consistently, the average number of excess deaths by month from 2018 to 2020 (Figure B5) reveals a sudden increase in excess mortality in January 2020, even before the diffusion of Covid was acknowledged in Italy. We repeat a similar exercise for Covid-related unemployment: Figure B6 shows the relation between predicted and observed probability of unemployment in the first two quarters of 2020, again indicating a high accuracy in prediction. Finally, in Figure B7 we show that there is a low correlation between our dichotomized treatment indicators capturing low and high exposure to Covid-related death and unemployment, as the number of municipalities in each of the four possible values taken by these variables is very similar.

We then present additional analyses discussed in the text. In the manuscript text, Figure 2 presented a snapshot of the effect of Covid-related deaths and unemployment in each month from 2018 to 2020. Here, we include the same figure for the whole period for which we have data, including all months since 2007 (Figure B8). In Figure B9 we show that our DiD result on unemployment is robust to the use of a double DiD estimator Egami and Yamauchi (2019), which accounts for potential violations of the common trends assumption. In Figure B12 we address the concern that the null response we observe to health-related threats might be the result of a larger reduction in mobility in areas affected by a health threat. However, we show that mobility did not decrease differentially in provinces with larger exposure to health-related threats in the period of consideration.

Finally, we present descriptives and analyses on the mechanism in Figures B10 and B11: first, we show that there is a low correlation between our measure of far-right voting at the national level and electing a far-right mayor in Figure B10. Second, we present a generalized fluctuation test on the trend of monthly negative articles about Chinese-origin people which indicates the presence of a structural break in correspondence with the start of Covid-19 (Figure B11).

Figure B1: Hate crimes by victim's region of origin, perpetrator and type

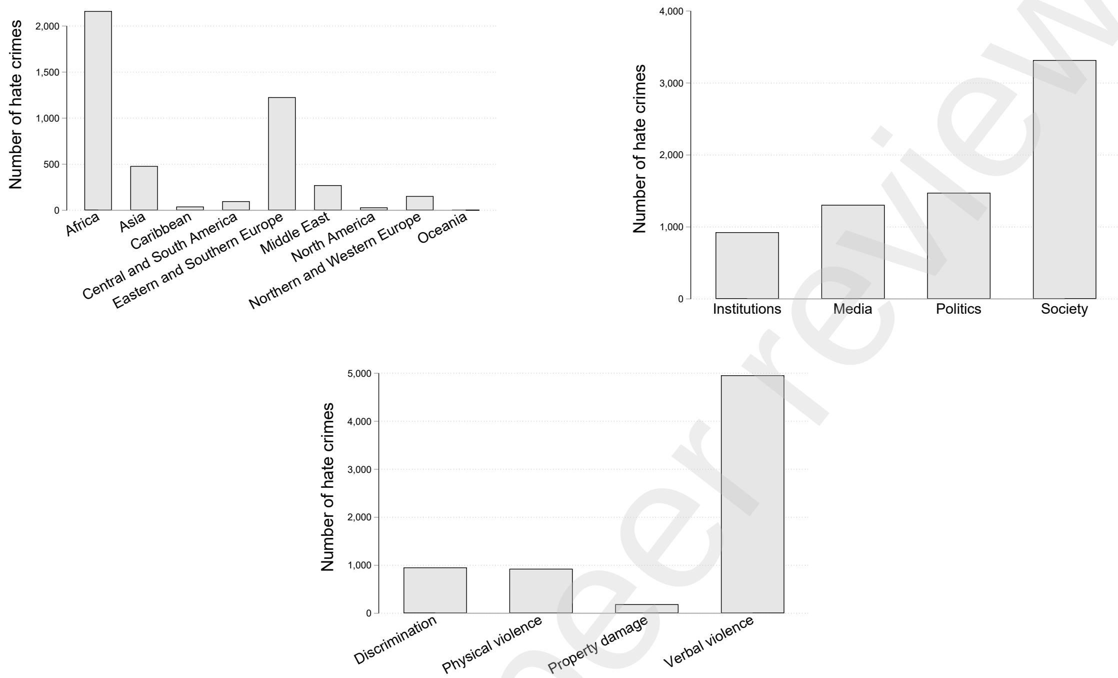


Table B1: Hate Crime Before and After Covid-19

	Before		After			
	Mean	SD	Mean	SD	Difference in Means	p-value
All hate crimes	0.0031	0.18	0.0017	0.04	-0.0014	0.000
Asian hate crimes	0.0001	0.03	0.0007	0.02	0.0005	0.004
African hate crimes	0.0009	0.07	0.0006	0.03	-0.0003	0.144

The table presents the pre- and post-Covid-19 mean and standard deviation of the outcome variables (hate crimes per 10,000 residents), and the difference in means. The p-values of the difference-in-means test indicate that the covariates are distinguishable across periods. Data are from Lunaria 2007 to March 2020. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\circ}p < 0.1$.

Figure B2: Cities with hate crimes (red) and hate crimes against Asians (black)

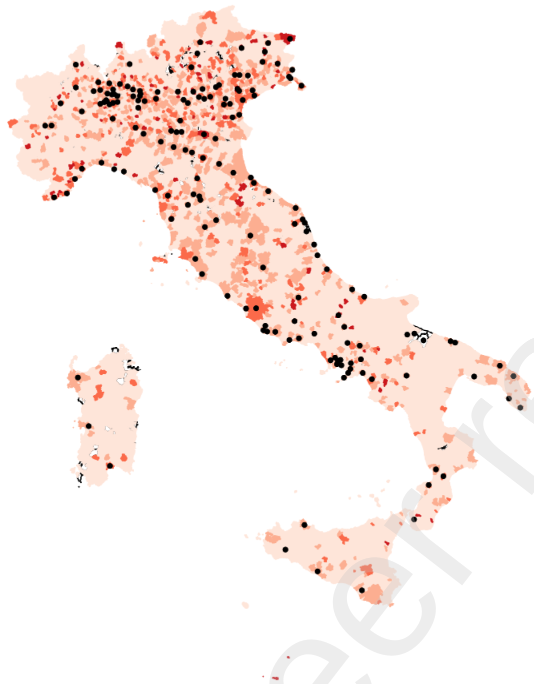
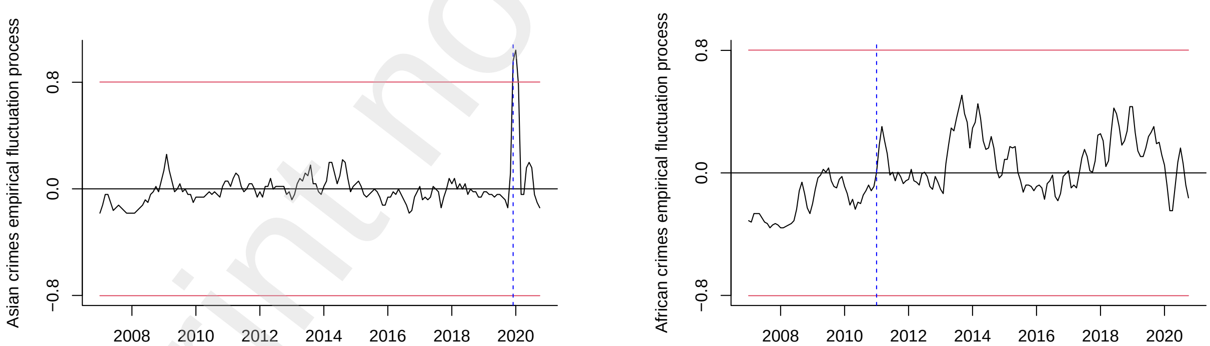


Figure B3: Generalized fluctuation test for structural breaks



(a) Empirical fluctuation process of crimes against Asian immigrants

(b) Empirical fluctuation process of crimes against African immigrants

Notes: The empirical fluctuation process is computed via moving sums of residuals within a window of 3 months using the R package `struccchange`. The horizontal red lines indicate the boundaries of the limiting process fluctuation. The vertical line indicates the estimated optimal breakpoint, considering only one breakpoint.

Table B2: Descriptive Statistics by Covid-19 Related Death

	Low Death		High Death		
	Mean	SD	Mean	SD	Difference in Means
All hate crime	0.0027	0.19	0.0035	0.17	0.0008*
Asian hate crime	0.0002	0.04	0.0001	0.02	−0.0000
African hate crime	0.0007	0.08	0.0011	0.07	0.0004**
Population	3785.5566	7006.55	11278.2209	57441.47	7490.5609***
% 65+	11.6073	4.31	9.9650	3.51	−1.6423***
% Foreign population	34.3289	28.73	42.5544	29.80	10.0113***
% Less college	58.4529	8.53	57.0781	8.26	−1.7268***
% Unskilled labor	18.7127	6.07	17.0073	4.92	−1.6388***
Unemployment rate	11.1076	7.71	9.1920	6.94	−1.8799***
% Male unemployed	8.8428	6.64	7.2020	5.91	−1.5942***
% Young unemployed	29.9939	17.59	25.7105	15.65	−4.1151***
Vote share extreme right	13.4835	9.02	16.4047	10.74	2.7476***
Far-right mayor	0.0125	0.11	0.0278	0.16	0.0154***

The table presents the pre-Covid-19 mean and standard deviation of the outcome variables (hate crimes per 10,000 residents), sociodemographic and political covariates across municipalities with low and high Covid-19 related deaths, and the difference in means. The p-values of the difference-in-means test indicate that the covariates are distinguishable across groups. In so far as these covariates affect the outcomes, such differences across groups are residualized by the DID approach, which accounts for the pre-Covid-19 difference in average outcomes across groups. In the analysis, we further account for these differences with municipality fixed effect and by controlling flexibly for these covariates. Data are from Lunaria 2007 to March 2020, 2011 Population Housing Census, 2011 Industry and Services Census, and Ministry of Interior. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\circ}p < 0.1$.

Table B3: Descriptive Statistics by Covid-19 Related Unemployment

	Low Unemployment		High Unemployment		
	Mean	SD	Mean	SD	Difference in Means
All hate crime	0.0027	0.15	0.0034	0.20	0.0007*
Asian hate crime	0.0002	0.04	0.0001	0.02	−0.0000
African hate crime	0.0008	0.08	0.0010	0.06	0.0002
Population	3401.0761	5672.32	11165.2625	55725.74	7740.9268***
% Foreign population	45.4338	30.65	30.9391	26.38	−13.6860***
% Less college	59.0927	7.68	56.5164	8.95	−2.8474***
% Unskilled labor	17.4664	4.49	18.3399	6.54	0.9581***
Unemployment rate	7.3189	5.16	13.0630	8.18	5.6956***
% Male unemployed	5.6128	4.46	10.5034	7.00	4.8734***
% Young unemployed	21.2098	12.52	34.6695	17.88	13.3565***
Vote share extreme right	18.4216	10.52	11.2901	7.92	−7.1071***
Far-right mayor	0.0292	0.17	0.0101	0.10	−0.0190***

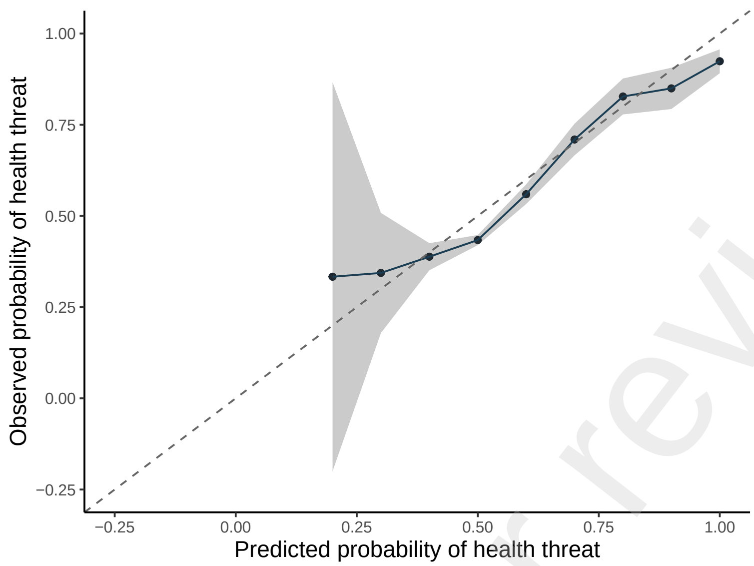
The table presents the pre-Covid-19 mean and standard deviation of the outcome variables (hate crimes per 10,000 residents), sociodemographic and political covariates across municipalities with low and high Covid-19 related unemployment, and the difference in means. The p-values of the difference-in-means test indicate that the covariates are distinguishable across groups. In so far as these covariates affect the outcomes, such differences across groups are residualized by the DID approach, which accounts for the pre-Covid-19 difference in average outcomes across groups. In the analysis, we further account for these differences with municipality fixed effect and by controlling flexibly for these covariates. Data are from Lunaria 2007 to March 2020, 2011 Population Housing Census, 2011 Industry and Services Census, and Ministry of Interior. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\circ}p < 0.1$.

Table B4: Covid-19 Death Effects on Asian Hate Crimes

	Dependent variable: Asian Hate Crimes per 10,000 residents				
	(1)	(2)	(3)	(4)	(5)
Excess deaths	0.00008				
	(0.00006)				
After Jan, 2020	0.00073*				
	(0.00032)				
After Jan, 2020 ×	−0.00043	−0.00043	−0.00069	−0.00062	−0.00062
Excess deaths	(0.00036)	(0.00036)	(0.00041)	(0.00042)	(0.00042)
R^2	0.00000	0.00650	0.00706	0.00706	0.00713
Obs	1253594	1253594	1253594	1253594	1253594
N Municipalities	7885	7885	7885	7885	7885
Month FE	N	Y	Y	Y	Y
Municipality FE	N	Y	Y	Y	Y
Flexible controls	N	N	Y	Y	Y
Group-specific linear trends	N	N	N	Y	Y
Province-specific linear trends	N	N	N	N	Y

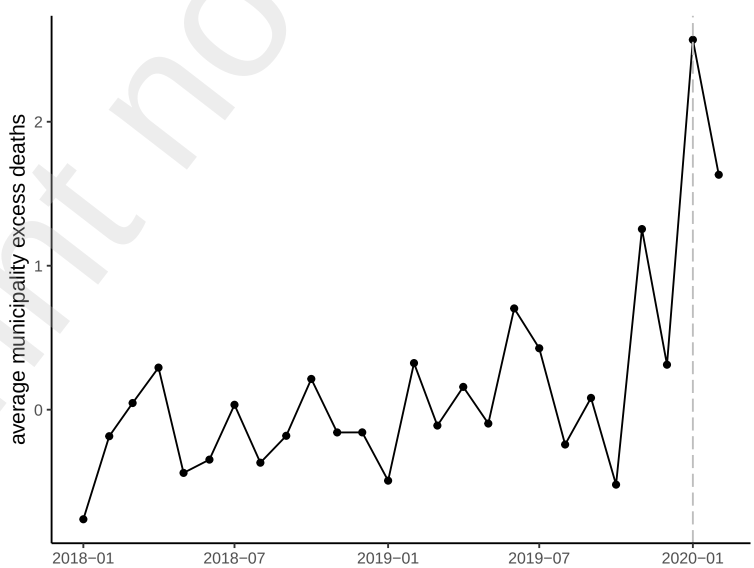
The dependent variable is monthly Asian hate crimes in a municipality per 10,000 residents. *After Jan, 2020* indicates the period after the first confirmed case of Covid-19 in Italy. *Excess deaths* indicates municipalities with an above the median number of excess deaths associated to Covid-19 in February 2020. Flexible controls include municipality population shares of foreign born, less than college educated, 65 years and older, and the party label of the head of local government interacted with month indicators. Group-specific linear trends and Province-specific linear trends correspond to the interaction between a continuous time measure and the municipality indicator of death exposure or Province indicators, respectively. Municipality-clustered-robust standard errors reported in parentheses. Data are from Lunaria 2007 to March 2020, Istat death counts 2017-2020, 2011 Population Housing Census. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Figure B4: Predicted vs. observed probability of death



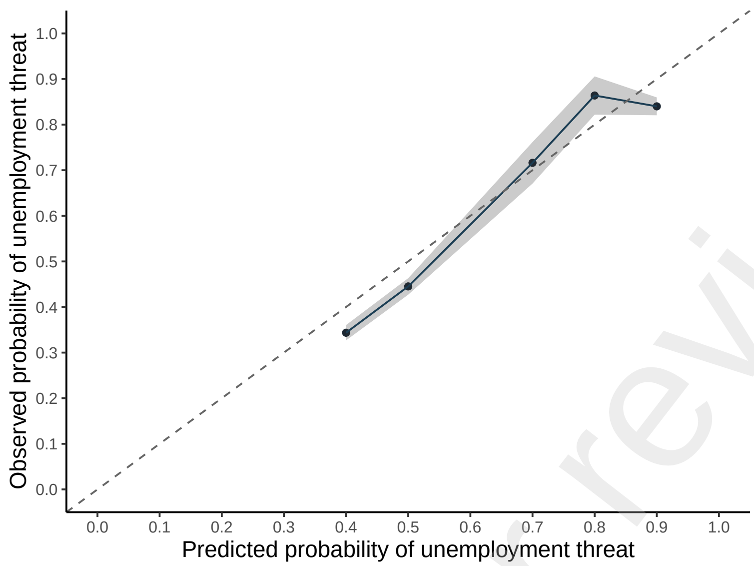
Notes: Points are the average number of municipalities under high threat of infection in February 2020 for equally spaced bins of the predicted probability of high excess deaths in February 2020 from logistic regression on the number of excess deaths in January 2020. The shaded band indicates 95% confidence intervals. Data are from ISTAT 2017-2020 and the 2011 Industry and Services Census.

Figure B5: Average number of excess deaths by month



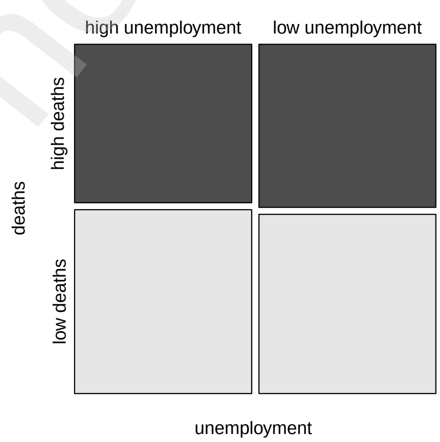
Notes: Points are the average number of excess deaths across Italian municipalities. Data are from ISTAT 2017-2020.

Figure B6: Predicted vs. observed probability of unemployment



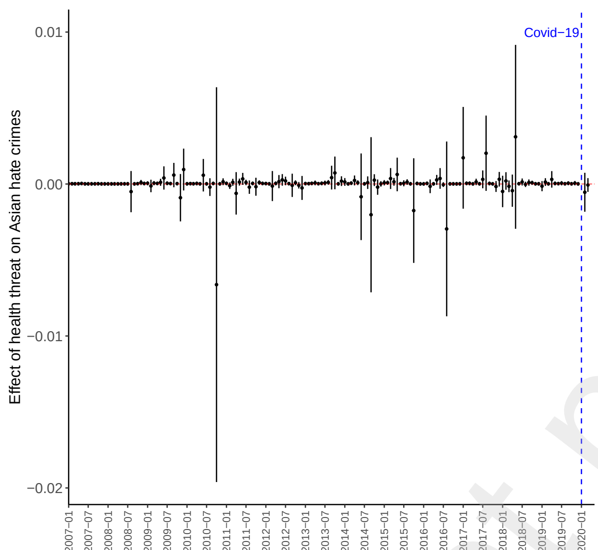
Notes: Points are the average number of municipalities under high threat of unemployment for equally spaced bins of the predicted probability of unemployment from logistic regression on regional unemployment rates in the first two quarters of 2020. The shaded band indicates 95% confidence intervals. Data are from ISTAT 2020 and the 2011 Industry and Services Census.

Figure B7: Correlation between local exposure to death and unemployment

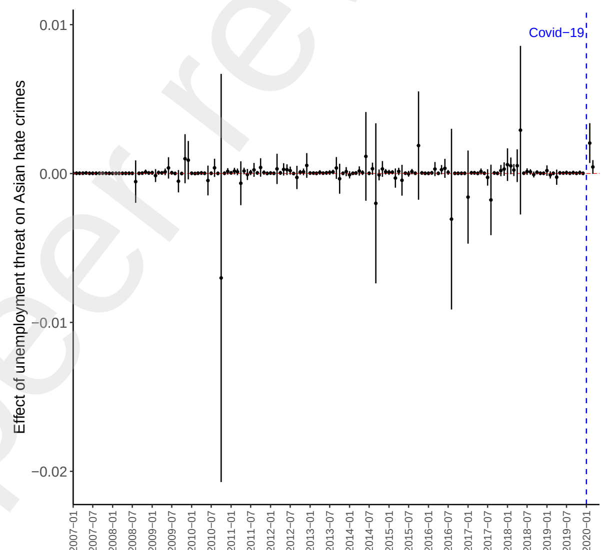


Notes: The box's size represents the share of municipalities in each of the four exposure conditions. Data are from ISTAT death counts 2017-2020 and the 2011 Industry and Services Census.

Figure B8: Monthly-varying Covid-19 effects on Asian hate crimes



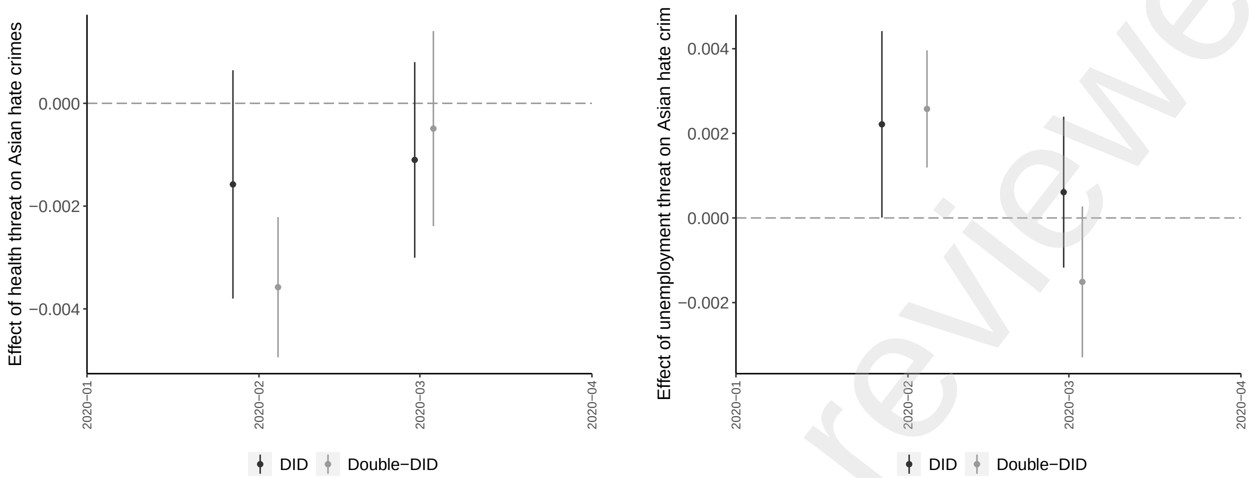
(a) Health threat effects



(b) Economic Threat Effects

Notes: Points represent the estimated coefficients from linear regression of Asian hate crimes per 10,000 residents on the interaction between monthly indicators and municipality indicators for (a) above the median deaths associated to Covid-19 and (b) above the median share of workers in affected economic sectors by Covid-19. The model specification includes month and municipality fixed effects. Lines are 95% confidence intervals. Data are from Lunaria 2007 to March 2020, 2011 Industry and Services Census, ISTAT deaths counts 2017-2020.

Figure B9: Covid-19 death and unemployment effects on Asian hate crimes: Accounting for possible violations to the common trends assumption

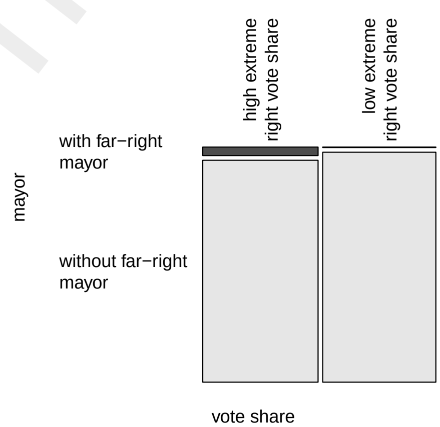


(a) Health threat effects

(b) Economic threat effects

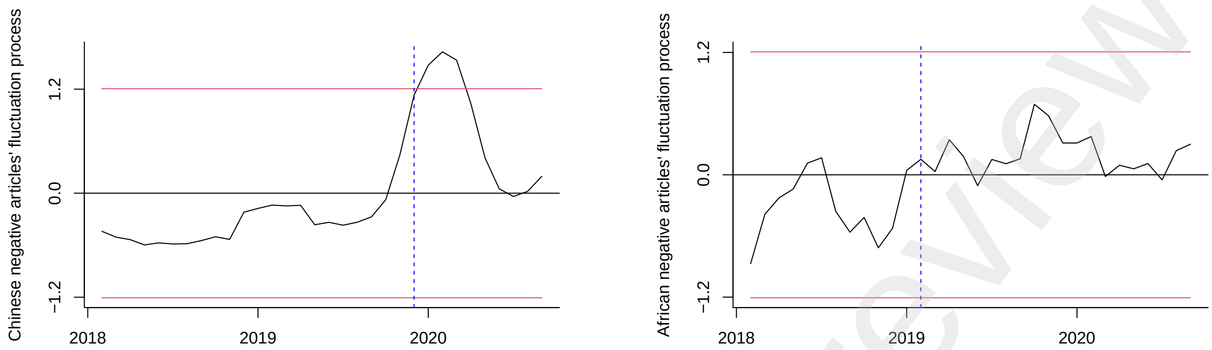
Notes: Points represent the estimated DiD and double DiD coefficients of effects of excess deaths and expected unemployment due to Covid-19 on Asian hate crimes per 10,000 residents as implemented by the R package `DIDdesign`. Lines are 95% confidence intervals. Data are from Lunaria 2007 to March 2020, 2011 Industry and Services Census, ISTAT death counts 2017-2020.

Figure B10: Correlation between vote for the extreme right in national elections and far-right mayors



Notes: The box's size represents the number of municipalities in each of the four conditions. Data are from the Ministry of Interior.

Figure B11: Generalized fluctuation test for structural breaks in the number of negative articles about Chinese- and African-origin people

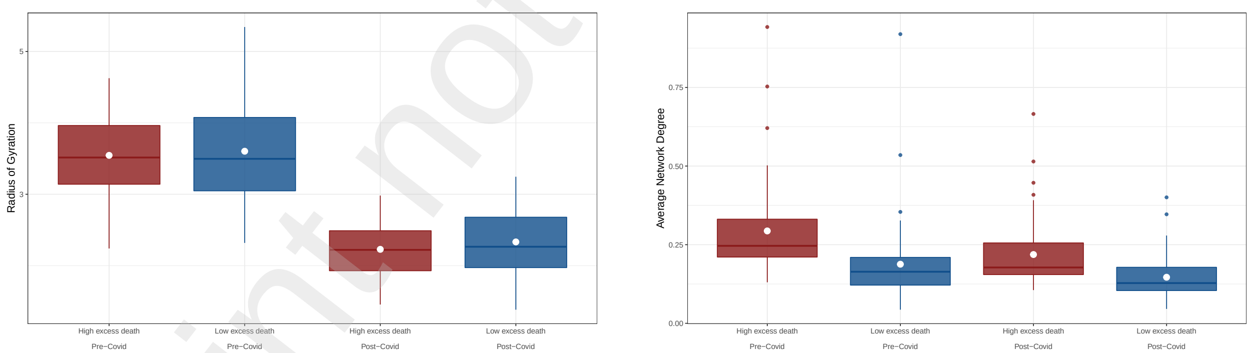


(a) Empirical fluctuation process of negative articles about Chinese people

(b) Empirical fluctuation process of negative articles about African people

Notes: The empirical fluctuation process is computed via moving sums of residuals using the R package `strucchange`. The horizontal red lines indicate the boundaries of the limiting process fluctuation. The vertical line indicates the estimated optimal breakpoint.

Figure B12: Change in mobility across provinces exposed to different levels of Covid excess deaths



(a) Changes in individual movement

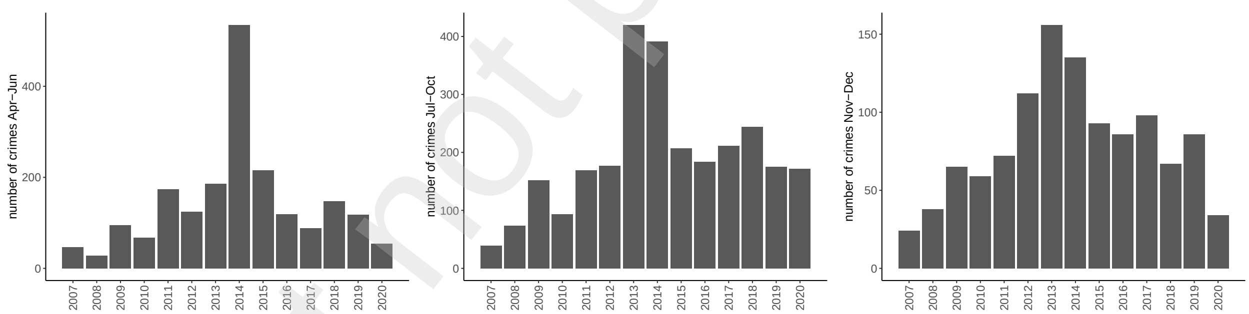
(b) Changes in social interaction

Notes: The plotted measure in (a) is based on weekly users' average radius of gyration by province, which captures the extent of individual movements, and in (b) on daily average degree of users' proximity network, which captures the level of social distancing by province. Pre-Covid includes available data from January 2020, and Post-Covid from February and March. The data at hand suggest that spatial variation in hate crimes is not a function of individuals' movement, at least as captured using available mobility data. Data are from Pepe et al. (2020).

C Hate Crime Analyses Including Periods with Restrictions on Freedom of Movement

In this section we assess our main results on hate crimes accounting for the months after the implementation of measures restricting freedom of movement. These strict measures were imposed by the end of March 2020 and maintained across the year, except for the months of July–October when they were temporarily lifted. Given that such measures greatly affected social dynamics, we first describe in Figure C1 how the patterns of hate crime respond to these measures: hate crimes are way below expected levels when restrictions are imposed in the periods of April–June and November–December (representing 45% and 40%, respectively of the average hate crimes over the previous four years), and at around expected levels when restrictions are lifted in July–October (equivalent to 85% of average hate crimes over the previous four years).

Figure C1: Total number of hate crimes during the lockdown periods



(a) Total number of hate crimes April–June

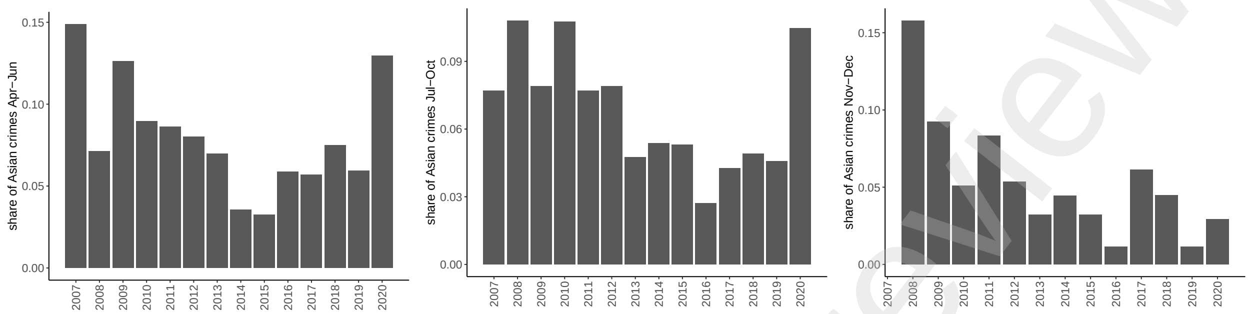
(b) Total number of hate crimes July–October

(c) Total number of hate crimes November–December

Despite such a disruption to the hate crime patterns, Asian hate crimes continued to be prevalent over this period of lockdowns, reaching a record high when compared to the previous decade. Figure C2 presents the number of hate crimes against Asians as a share of the total number of hate crimes, during these 3 different periods when restrictions were imposed, lifted and imposed again. During April–June and July–October, the share of Asian hate crimes represents at least 200% of that share in the previous four years, and

90% during November–December. These patterns suggest that Asian hate crimes may have persisted across the year of 2020, even with the strict lockdown measures.

Figure C2: Share of Asian hate crimes during the lockdown periods



(a) Share of Asian hate crimes April–June

(b) Share of Asian hate crimes July–October

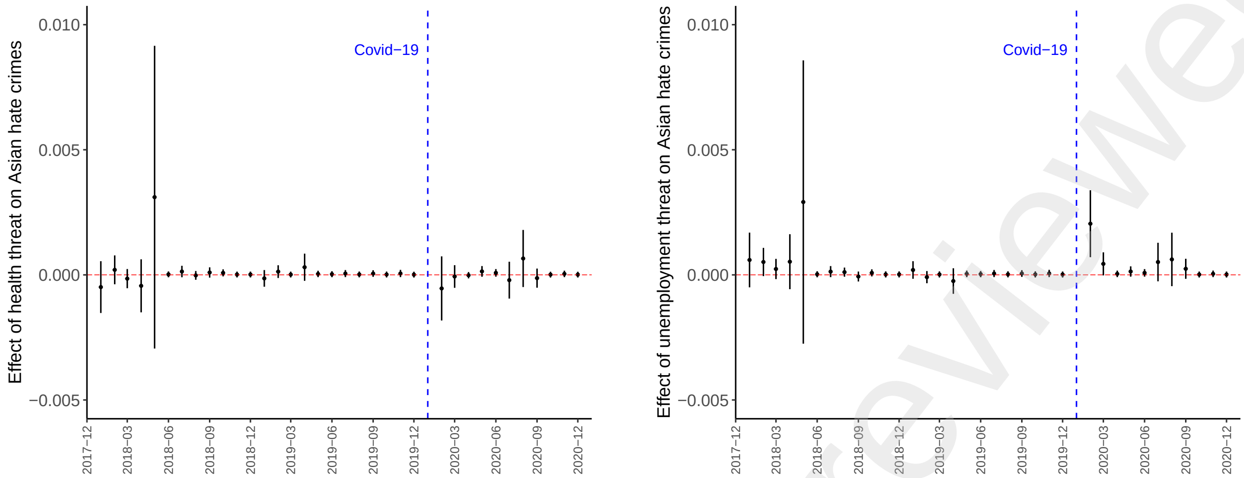
(c) Share of Asian hate crimes November–December

In the main text our analyses are centered on the months before the freedom of movement measures were imposed, as we aim to assess the reponse to the pandemic outbreak clean of other dynamics that may have affected the likelihood of hate crimes. However, considering that there may be persistence in the reaction to the pandemic outbreak in the form of Asian hate crimes, despite the disruption to social dynamics, as suggested in Figure C2, here we replicate the main results using the hate crime data from January 2007 to December 2020. The results are broadly consistent with those presented in the main text.

Figure C3 presents the monthly-varying Covid-19 related unemployment and mortality effects on Asian hate crimes for every month until December 2020 (we zoom in on 2018-2020 for illustrative clarity). Consistent with the results presented in the main text, the plots suggest that the effects of Covid-related death are not statistically significant at any month after the Covid outbreak (Figure C3a). In contrast, the effect of Covid-related unemployment on Asian hate crimes is positive and statistically significant only in February 2020. The magnitude of the effects decay after that month and are not statistically significant (Figure C3b).

Table C1 presents the main Covid-related death and unemployment effects on Asian hate crimes including the data from the months after the restriction on movement measures were

Figure C3: Monthly-Varying Covid-19 Effects on Asian Hate Crimes Until December 2020



(a) Health Threat Effects

(b) Economic Threat Effects

Notes: Points represent the estimated coefficients from linear regression of Asian hate crimes per 10,000 residents on the interaction between monthly indicators and municipality indicators for (a) above the median deaths associated with Covid-19 and (b) above the median share of workers in affected economic sectors by Covid-19. The model specification includes month and municipality fixed effects. Lines are 95% confidence intervals. Data are from Lunaria 2007 to December 2020, 2011 Industry and Services Census, ISTAT deaths counts 2017-2020. The plot presents a snapshot of the effects since January 2018.

imposed. The conclusion remains substantively the same as that reached from the results in Table 1: the effect of Covid-related unemployment on Asian hate crimes is positive and statistically significant, although the magnitude of the coefficient is smaller, and the effect of Covid-related deaths is not statistically significant.

Table C1: Covid-19 Effects on Asian Hate Crimes Including all Months Until December 2020

	Dependent variable: Asian Hate Crimes per 10,000 residents				
	(1)	(2)	(3)	(4)	(5)
Panel A: Health Threat					
Excess deaths	−0.00000				
	(0.00006)				
After Jan, 2020	0.00005				
	(0.00009)				
After Jan, 2020 × Excess deaths	−0.00001	−0.00001	−0.00011	−0.00021	−0.00021
	(0.00011)	(0.00011)	(0.00013)	(0.00016)	(0.00016)
Average Hate Crimes	0.00015	0.00015	0.00017	0.00014	0.00014
R^2	0.00000	0.00616	0.00671	0.00637	0.00643
Obs.	1324559	1324559	1324559	1324559	1324559
N Municipalities	7885	7885	7885	7885	7885
Panel B: Economic Threat					
Expected unemployment	−0.00002				
	(0.00006)				
After Jan, 2020	−0.00015**				
	(0.00006)				
After Jan, 2020 × Expected unemployment	0.00038***	0.00038***	0.00031**	0.00029*	0.00029*
	(0.00011)	(0.00011)	(0.00010)	(0.00013)	(0.00013)
Average Hate Crimes	0.00016	0.00016	0.00014	0.00014	0.00014
R^2	0.00000	0.00616	0.00630	0.00630	0.00636
Obs.	1330775	1330775	1330775	1330775	1330775
N Municipalities	7922	7922	7922	7922	7922
Month FE	N	Y	Y	Y	Y
Municipality FE	N	Y	Y	Y	Y
Flexible controls	N	N	Y	Y	Y
Group-specific linear trends	N	N	N	Y	Y
Province-specific linear trends	N	N	N	N	Y

The dependent variable is monthly Asian hate crimes in a municipality per 10,000 residents. *After Jan, 2020* indicates the period after the first confirmed case of Covid-19 in Italy. *Excess deaths* and *Expected unemployment* indicate municipalities with an above the median number of deaths in January 2020 associated to Covid-19 and share of workers in affected sectors by Covid-19, respectively. Flexible controls include municipality population shares of foreign born and less than college educated interacted with month indicators. The health threat specification includes as well share of the population 65 years and older, and the party label of the mayor interacted with month indicators. Group-specific linear trends and Province-specific linear trends correspond to the interaction between a continuous time measure and the municipality indicator of infection or unemployment exposure, or province indicators, respectively. Municipality-clustered-robust standard errors reported in parentheses. *Average Hate Crimes* is the effective sample mean pre-Covid-19 hate crime rate in control municipalities, computed following Aronow and Samii (2016). Data are from Lunaria 2007 to December 2020, 2011 Industry and Services Census, Istat death counts 2017-2020. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

D Robustness of Covid-19 Unemployment Effects

In addition to checking in Table 1 that results are robust to accounting for endogeneity due to presence of confounders (with monthly and municipality fixed effects, controlling flexibly for relevant covariates, and province-specific linear trends) and for possible violations to the common trends assumption (with group-specific linear trends), here we explore whether the Covid-19 related unemployment results are robust as well to a series of other specification considerations.

First, we control flexibly for the pre-Covid municipality's size of the East and Southeast Asian communities to correct for potential bias from omitting this variable. We do so with a restricted sample of municipalities that have information about their immigrant population. The patterns presented in the main analysis (in Table 1) remain when we control flexibly for the size of these communities, as shown in Table D1. This suggests that even when we compare within municipalities with similar size of the targeted community, we find a stronger violent reaction in economically affected municipalities.

Second, we use alternative definitions of exposure to unemployment. In particular, we use the continuous measure of the share of workers in affected sectors as opposed to the dichotomous variable which splits municipalities into below and above the median share. We also present the effect estimates by quartiles of the share of workers in affected sectors. Table D2 and Figure D1 confirm that the results are not sensitive to any specific cutoff of expected unemployment to determine exposures.

Third, we restrict the analysis to a shorter time frame, from January, 2019 to March, 2020, that may guarantee that the reporting standards of hate crimes, as well as relevant aspects of the municipalities, including the number of Asian residents and the political climate, are not changing substantially. These results are very similar to the results in our main specification, suggesting that even when we account for potential endogeneity caused by time-varying municipality characteristics, and also for potential changes in the reporting of crime, the Covid-related unemployment effects are unchanged.

Fourth, we rule out the alternative explanation that the violent reaction is a response to being exposed to Chinese tourists accused of spreading the virus in Italy, instead of a reaction to Covid-related unemployment. Although this alternative explanation is unlikely, given that

Table D1: Covid-19 Unemployment Effects on Asian Hate Crimes: Accounting for the Size of East and Southeast Asian Communities

	Dependent variable: Asian Hate Crimes per 10,000 residents				
	(1)	(2)	(3)	(4)	(5)
Expected	−0.00002				
unemployment	(0.00007)				
After Jan, 2020	−0.00011				
	(0.00008)				
After Jan, 2020 ×	0.00135***	0.00135***	0.00089**	0.00087**	0.00087**
Expected unemployment	(0.00039)	(0.00039)	(0.00028)	(0.00029)	(0.00029)
R^2	0.00001	0.00652	0.00669	0.00669	0.00677
Obs	1163918	1163918	1163918	1163918	1163918
N Municipalities	7321	7321	7321	7321	7321
Month FE	N	Y	Y	Y	Y
Municipality FE	N	Y	Y	Y	Y
Flexible controls	N	N	Y	Y	Y
Group-specific linear trends	N	N	N	Y	Y
Province-specific linear trends	N	N	N	N	Y

The dependent variable is monthly Asian hate crimes in a municipality per 10,000 residents. *After Jan, 2020* indicates the period after the first confirmed case of Covid-19 in Italy. *Expected unemployment* indicates municipalities with an above the median share of workers in affected sectors by Covid-19. Flexible controls include municipality population shares of foreign born, less than college educated, and East and Southeast Asian immigrants interacted with month indicators. Group-specific linear trends and Province-specific linear trends correspond to the interaction between a continuous time measure and the municipality indicator of unemployment exposure, or province indicators, respectively. Municipality-clustered-robust standard errors reported in parentheses. The sample includes only municipalities with immigrant population data (92% of those included in the main analysis). Data are from Lunaria 2007 to March 2020, 2011 Population Housing Census, 2011 Industry and Services Census, Istat death counts 2017-2020. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

flights from China to Italy were halted on January 31st, we assess it by redefining our measure of exposure to unemployment. Particularly, we exclude workers in tourism from the share of workers in affected sectors, and therefore, under this definition, the exposed municipalities do not to rely on tourism. Figure D2 shows that the Covid-related unemployment effects on hate crime excluding tourism are not distinguishable from the main effects, suggesting that the violent response is not explained by exposure to tourists.

Fifth, we alter the model's functional form to account for nonlinear models which deal with outcome variables of bounded support, and to deal with excess zeros in the outcome

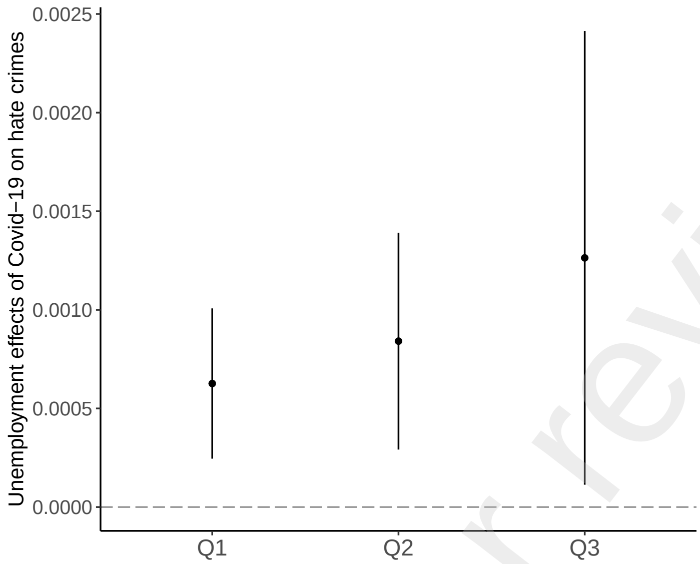
Table D2: Covid-19 Unemployment Effects on Asian Hate Crimes: Continuous Treatment Measure

	Dependent variable:			
	Asian Hate Crimes per 10,000 residents			
	(1)	(2)	(3)	(4)
Expected unemployment	−0.00000			
	(0.00000)			
After Jan, 2020	−0.00306**			
	(0.00094)			
After Jan, 2020 × Expected unemployment	0.00007**	0.00007**	0.00005**	0.00005**
	(0.00002)	(0.00002)	(0.00002)	(0.00002)
R^2	0.00001	0.00651	0.00665	0.00671
Obs	1259477	1259477	1259477	1259477
N Municipalities	7922	7922	7922	7922
Month FE	N	Y	Y	Y
Municipality FE	N	Y	Y	Y
Flexible controls	N	N	Y	Y
Province-specific linear trends	N	N	N	Y

The dependent variable is monthly Asian hate crimes in a municipality per 10,000 residents. *After Jan, 2020* indicates the period after the first confirmed case of Covid-19 in Italy. *Expected unemployment* is a continuous measure with the municipality share of workers in affected sectors by Covid-19. Flexible controls include municipality population shares of foreign born and less than college educated interacted with month indicators. Province-specific linear trends correspond to the interaction between a continuous time measure and Province indicators. Municipality-clustered-robust standard errors reported in parentheses. Data are from Lunaria 2007 to March 2020, 2011 Population Housing Census, 2011 Industry and Services Census. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\circ}p < 0.1$.

variable. In particular, using hate crimes as counts as opposed to the number of hate crimes per 10,000 residents, we fit within a DiD framework a negative binomial model and a zero-inflated negative binomial model with the mass points zeros modeled as a function of the size of the East and Southeast Asian communities in a municipality. These models allow for overdispersion, in part caused by excess zeros in the outcome variable. Estimating a DiD model with the standard specification of a nonlinear model does not fulfill the common trend assumption. This is because the common trend assumption relies on differencing out unobservable terms of the potential outcomes, which cannot be differenced out under

Figure D1: Effect of Covid-19 on Asian hate crimes by quartile of share of workers in affected economic sectors



Notes: Points represent the estimated coefficients from linear regression of Asian hate crimes per 10,000 residents on an indicator for above the first, second, and third quartile of share of workers in affected economic sectors by Covid-19. The model specification includes month and municipality fixed effects, flexible controls, group-specific and province-specific linear trends. Lines are 95% confidence intervals. Data are from Lunaria 2007 to March 2020, 2011 Industry and Services Census.

a nonlinear specification. Moreover, under a nonlinear specification, the common trend assumption only holds when the group specific differences are exactly zero, which means that the average outcomes cannot be different in levels. Therefore, estimating a DiD with the standard specification of a nonlinear model would produce an inconsistent estimator when the standard common trend assumption holds. In order to address this challenge, as suggested in (Lechner et al., 2011, p. 199–200), we use nonlinear parametric approximations to predict the four components of the conditional effects $\mathbb{E}(Y_t \mid X = x, D = d)$ for time t and treatment $d \in \{0, 1\}$, and then average the conditional effects according to the negative binomial or zero-inflated negative binomial distribution to obtain estimates for the treated population. For inference on the ATT, we permute the municipality's treatment assignment (in this case, their exposure to high or low pandemic-related unemployment) and estimate the ATT to compute the distribution under the null hypothesis of no treatment effects, which

Table D3: Covid-19 Unemployment Effects on Asian Hate Crimes: Shorter Time Frame

	Dependent variable: Asian Hate Crimes per 10,000 residents				
	(1)	(2)	(3)	(4)	(5)
Expected unemployment	-0.00001				
	(0.00007)				
After Jan, 2020	-0.00004				
	(0.00009)				
After Jan, 2020 × Expected unemployment	0.00124^{***}	0.00124^{***}	0.00102^{**}	0.00114^{*}	0.00115^{*}
	(0.00036)	(0.00037)	(0.00034)	(0.00048)	(0.00049)
R^2	0.00048	0.07027	0.07066	0.07066	0.07172
Obs	118830	118830	118830	118830	118830
N Municipalities	7922	7922	7922	7922	7922
Month FE	N	Y	Y	Y	Y
Municipality FE	N	Y	Y	Y	Y
Flexible controls	N	N	Y	Y	Y
Group-specific linear trends	N	N	N	Y	Y
Province-specific linear trends	N	N	N	N	Y

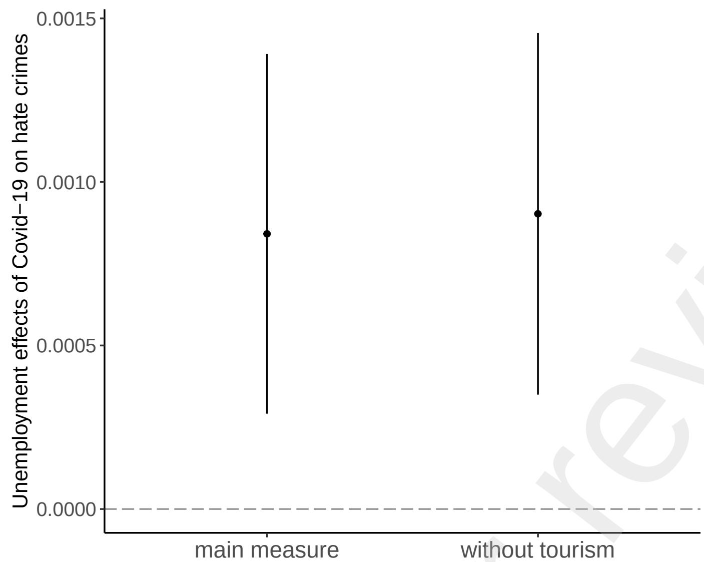
The dependent variable is monthly Asian hate crimes in a municipality per 10,000 residents. *After Jan, 2020* indicates the period after the first confirmed case of Covid-19 in Italy. *Expected unemployment* indicates municipalities with an above the median share of workers in affected sectors by Covid-19. Flexible controls include municipality population shares of foreign born, less than college educated interacted with month indicators. Group-specific linear trends and Province-specific linear trends correspond to the interaction between a continuous time measure and the municipality indicator of unemployment exposure, or province indicators, respectively. Municipality-clustered-robust standard errors reported in parentheses. The sample is restricted to January, 2019 - March, 2020. Data are from Lunaria 2007 to March 2020, 2011 Population Housing Census, 2011 Industry and Services Census, Istat death counts 2017-2020. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

we compare with the observed ATT to compute the two-tailed p-value.

We also estimate the changes-in-changes estimator of Athey and Imbens (2006). This framework drops the linearity assumption in the DiD framework and models the outcome with its actual distribution. This allows for the common trend assumption to hold even for skewed variables with bounded support, and therefore to causally identify the ATT. The results of these three models are presented in Table D4, which suggests that the estimated effects are robust to such variations in the model's functional form.

Finally, in Table D5 we re-run our most restrictive specification of the main analysis (Model 5 in Table 1) including flexible controls for youth unemployment (Column 1) and for

Figure D2: Covid-19 unemployment effects on Asian hate crimes: excluding tourism



Notes: Points represent the estimated coefficients from linear regression of Asian hate crimes per 10,000 residents on an indicator for above the median share of workers in affected economic sectors by Covid-19 excluding tourism. The model specification includes month and municipality fixed effects, flexible controls, group-specific and province-specific linear trends. Lines are 95% confidence intervals. Data are from Lunaria 2007 to March 2020, 2011 Industry and Services Census.

population density (Column 2). Considering this additional set of controls interacted with month dummies allows us to exclude the possibility that the effect we observe are driven by factors correlated with Covid-related unemployment and that might also have a time-varying effect on hate crimes. The robustness of our findings across these analyses suggests that those factors do not explain our results.

Table D4: Covid-19 Unemployment Effects on Asian Hate Crimes: Nonlinear Models

	Dependent variable: Number of Asian Hate Crimes		
Model	Estimate	p-value	Obs
Negative binomial model in DiD framework	0.0052	0.0000	1259477
Zero-inflated negative binomial model in DiD framework	0.0056	0.0000	1163918
Changes-in-Changes framework	0.0109	0.0480	1259477

The dependent variable is the number of monthly Asian hate crimes in a municipality (Mean=0.004). Estimates for the *Negative binomial model in DiD framework* and *Zero-inflated negative binomial model in DiD framework* are computed by first estimating a negative binomial (or zero-inflated) model on each of the three subsamples (before-control, before-treated, after-control) to predict the four components of the DiD estimator on the treated sample, and secondly by taking the difference of the two differences and averaging to obtain the ATT. Estimate for *Changes-in-Changes framework* follows Athey and Imbens (2006) and is implemented using the R package `qte`. *p-value* are computed via randomization inference with 500 iterations. The sample in the zero-inflated model includes only municipalities with information about their immigrant population. Data are from Lunaria 2007 to March 2020, 2011 Population Housing Census, 2011 Industry and Services Census.

Table D5: Covid-19 Unemployment Effects on Asian Hate Crimes: Youth Unemployment and Population Density Flexible Controls

	Dependent variable:	
	Asian Hate Crimes per 10,000 residents	
	(1)	(2)
After Jan, 2020 ×	0.00083***	0.00097***
Expected unemployment	(0.00029)	(0.00036)
R^2	0.00675	0.00656
Obs	1,259,318	1,259,477
Month FE	Y	Y
Municipality FE	Y	Y
Group-specific linear trends	Y	Y
Province-specific linear trends	Y	Y

The dependent variable is monthly Asian hate crimes in a municipality per 10,000 residents. *After Jan, 2020* indicates the period after the first confirmed case of Covid-19 in Italy. *Expected unemployment* is a continuous measure with the municipality share of workers in affected sectors by Covid-19. Flexible controls include youth unemployment (Column 1) or population density (Column 2) interacted with month indicators. Province-specific linear trends correspond to the interaction between a continuous time measure and Province indicators. Municipality-clustered-robust standard errors reported in parentheses. Data are from Lunaria 2007 to March 2020, 2011 Population Housing Census, 2011 Industry and Services Census. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\circ}p < 0.1$.

E Testing for reporting bias

Data on hate crimes may be subject to differences in reporting which might bias the estimates in the direction of our findings if reporting is higher in places with highest expected unemployment after Covid. We check for possible bias in the reporting of hate crimes that may have coincided with the pandemic onset in Table E1.

We start by assessing whether the observed effects are an artifact of the increase in salience of hate crimes against Asians in the media. In particular, national newspapers might report crimes against this group at higher rates due to the preponderance of Asia in the news caused by Covid-19. We show that this is not the case in Column 1: when we exclude from the count of hate crimes all instances of crimes reported by national media, we still observe a positive significant impact of Covid-related unemployment on hate crimes.

Second, instead of media paying higher attention, it might be victims of hate crimes that report at higher rates due to the awareness of being singled in relation to Covid. Column 2 shows that our findings are robust also to this concern: findings are robust when dropping all crimes reported by individuals directly to the NGO.

Third, findings are robust to excluding verbal hate crimes: verbal crimes might be less seriously threatening and thus they might be only reported when crimes against Asians are newsworthy, as it might have happened in February and March 2020. However, subsetting to either verbal (42% of the hate crimes, estimates in Column 4) or to non-verbal hate crimes (58% of the hate crimes, estimates in Column 3) we obtain similar findings.

Fourth, we subset the analyses by type of attacker: we consider crimes committed by individuals or groups (74% of the sample) and crimes committed by institutions, media and politicians (26% of the sample). We find that the increase in hate crimes against Asians is entirely driven by attacks committed by individuals or groups (Column 5), while there is no increase for crimes committed by institutions, politicians, or media (Column 6). This finding confirms our interpretation: if the increase in hate crimes is driven by frustration related to loss of income, we should expect crimes being committed by individuals rather than by institutions, politicians, or media.

A related concern is that the location of the hate crime might be misattributed. For example, when institutions, media or politicians commit hate crimes, the location we observe might not be where the attack took place (at the national level, or where the institution or media is located), but rather where the victim lives. The test in Columns 5 and 6 also allows us to account for such potential concern: our findings are entirely driven by crimes committed by individuals, who, unlike institutions, politicians and news media, are likely to be present in the same location as the victim, avoiding the misallocation issue.

Another case in which crimes committed by individuals might be misallocated is when those happen on social media. Lunaria does not report information on whether the hate crime was first published on social media, but rather only reports as source the outlet or the NGO that reported about the hate crime. However, we build this information by searching in the description of the hate crime the words: Facebook, Twitter, Youtube, Instagram, TikTok. If any of the most commonly used social networks in Italy[22] is listed in the description, we conservatively mark the hate crime as happening on social media. In a robustness test now reported in Table E.1, Column 7, we drop hate crimes with descriptions mentioning social media from the analyses (14% of the total number of hate crimes). Our findings are robust to the exclusion of these events, indicating that potential misattribution in location when the true location is unknown does not drive our results.

[22]Source: Most commonly used social networks in Italy, 2021

Table E1: Covid-19 Unemployment Effects on Asian Hate Crimes, Reporting Bias Test

	Dependent variable: Asian Hate Crimes per 10,000 Residents, restricted to:						
	(1) Source: drop National media	(2) Source: drop Individuals	(3) Type: Not verbal	(4) Type: Verbal	(5) Attacker: Person	(6) Attacker: Institutions	(7) Where: Not social media
After Jan 2020 × Expected unemployment	0.00077***	0.00086***	0.00053**	0.00031**	0.00082***	0.00004	0.00080***
	(0.00024)	(0.00028)	(0.00024)	(0.00014)	(0.00028)	(0.00002)	(0.00026)
Observations	1,259,477	1,259,477	1,259,477	1,259,477	1,259,477	1,259,477	1,259,477
R-squared	0.00656	0.00666	0.00714	0.00652	0.00658	0.00729	0.00661
N municipalities	7922	7922	7922	7922	7922	7922	7922

The dependent variable is monthly Asian hate crimes in a municipality per 10,000 residents restricting the analyses to one of the following categories: dropping crimes reported by national media (Column 1) or by individuals (Column 2); including only crimes that are not verbal (Column 3) or verbal hate crimes (Column 4); including crimes committed by individuals only (Column 5) or by institutions, media and politicians (Column 6); dropping crimes committed on social media (Column 7). *After Jan, 2020* indicates the period after the first confirmed case of Covid-19 in Italy. *Expected unempl* indicates municipalities with an above the median share of workers in affected sectors by Covid-19. All columns include municipality and month fixed effects, flexible controls for the municipality population shares of foreign born and less than college educated interacted with month indicators, group-specific linear trends and province-specific linear trends. Municipality-clustered-robust standard errors reported in parentheses. Data are from Lunaria 2007 to March 2020, 2011 Population Housing Census, 2011 Industry and Services Census. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\circ}p < 0.1$.

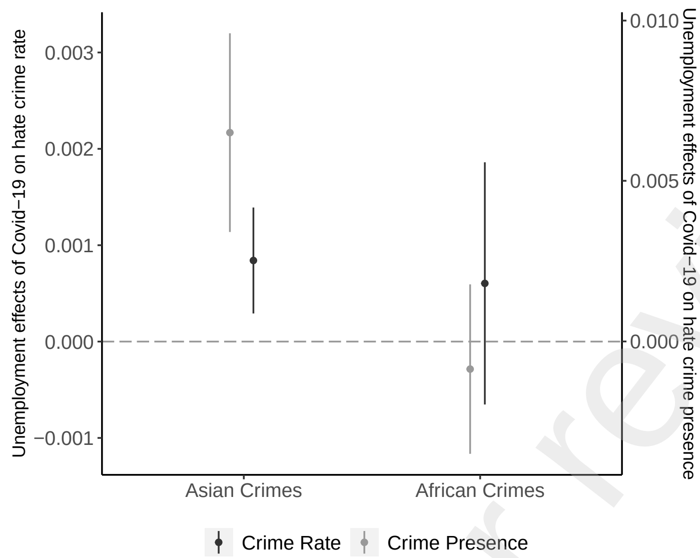
F Auxiliary Analysis of Covid-19 Unemployment Effects

We have shown that the threat of unemployment caused by Covid-19 triggers a violent reaction against Asian-origin people. Here, we conduct additional analyses to first inquire whether such violent reaction spills over to other ethnic-minority immigrant groups, in particular, people from African origin, given their history as targets of bigoted attacks in Italy. And secondly, to assess whether in addition to an increase in the crime rate (the number of crimes per 10,000 residents), there is as well an increase in the number of municipalities experiencing hate crimes.

A comparison between the unemployment Covid-19 effects on hate crimes against Asians and Africans shown in Figure F1 suggests that the hateful response to economic grievances as a result of the virus outbreak is targeted at the group perceived to be inflicting such grievances, as the effect of unemployment threat on African hate crimes is smaller than the effect on crimes against Asians and it is not statistically significant.

Moreover, not only the number of crimes against Asians increases in February 2020, but also the number of municipalities experiencing Asian hate crimes increases, suggesting that municipalities without a history of attacks on the Asian population are mobilized against Asian-origin immigrants after the virus outbreak. This finding is in accordance with a Bayesian-like rationale suggesting that triggering events can be disproportionally more consequential where initial levels of hateful behaviors are low (Frey, 2020; Ferrín, Mancosu and Cappiali, 2020), given that against their prior beliefs, people from these places perceive immigrants from Asia as a threat perhaps for the first time, and therefore heavily update on this information, evoking a stronger reaction against Asian-origin immigrants. As Figure F1 shows, such increase in the number of municipalities with presence of crimes against Asians is higher and statistically significant in municipalities under the threat of unemployment relative to less economically affected municipalities.

Figure F1: Comparison of Covid-19 unemployment effects across Asian and African number and presence of hate crimes



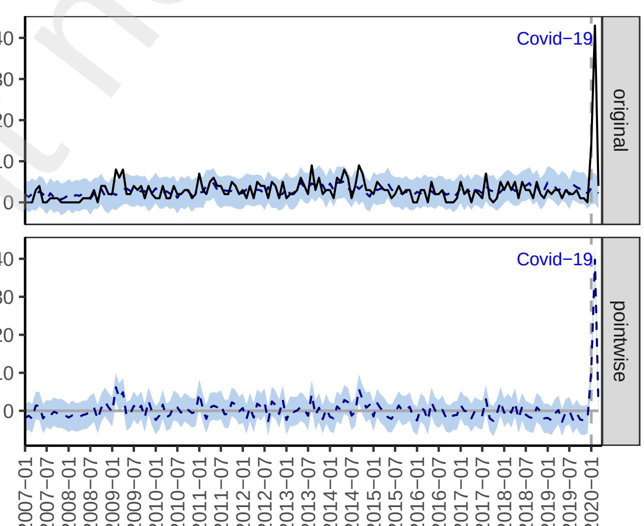
Notes: Points represent the estimated coefficients from linear regression of Asian (left side) or African (right side) hate crimes per 10,000 residents (in dark gray and left-axis), or an indicator of hate crimes (in light Gray and right-axis), on an indicator for above the median share of workers in affected economic sectors by Covid-19. The model specification includes month and municipality fixed effects, flexible controls, group-specific and province-specific linear trends. Lines are 95% confidence intervals. Data are from Lunaria 2007 to March 2020, 2011 Industry and Services Census.

G Covid-19 Effects on Asian Hate Crime: Bayesian Structural Time-Series Model Approach

We estimate the causal effect of the pandemic on the number of hate crimes against Asians at the national-level via a Bayesian structural time-series model, as developed by Brodersen et al. (2015) and as implemented in the `CausalImpact` R package. This method employs a state-space time series model that regresses the pre-pandemic national trend of Asian hate crimes on the set of pre-pandemic national trends of hate crimes against non-Asian immigrant groups (except for the trend of Caribbean-origin immigrants which shows a significant uptick at the pandemic onset), flexibly accounting for seasonality, to construct a synthetic control trend. Then, it computes the posterior distribution of the counterfactual trend of crimes against Asians using the pre-pandemic national trend of Asian hate crimes and the post-pandemic synthetic trend of hate crimes against other immigrant groups. A semi-parametric Bayesian posterior distribution for the causal effect of the pandemic is obtained by taking the difference between the post-pandemic observed and counterfactual trends of crimes against Asians.

The top panel in Figure G1 shows the observed (solid line) and the predicted counterfactual (dashed line) trends and the bottom panel presents the difference between these two trends (that is, the causal effect). While the pre-pandemic difference between the observed and the predicted counterfactual trends is not distinguishable from zero (which suggests that the computation of the predicted counterfactual is accurate), we observe a large increase in such difference after January 2020. This effect corresponds to an additional 21 crimes against Asians on average during the months of February and March 2020 with a 95% interval of [18.01, 23.29], which corresponds to a relative increase in crimes of 760% with a 95% interval of [659%, 852%]. Considering the cumulative crimes during these two months we have 42 additional crimes. These effects are statistically significant as the Bayesian one-sided tail-area probability is p = 0.0005.

Figure G1: Observed and predicted counterfactual trends and their difference



H Additional Information and Tests of Mechanisms

Tables H1 and H2 present the estimated coefficients of the triple-differences models that we use to compute heterogeneous Covid-related unemployment effects on hate crime by prejudice and far-right mayors in Figure 3. It is important to note that the inference on the coefficient of the triple interaction *After Jan, 2020* $\times$ *Expected unemployment* $\times$ *Far-right* would suggest that there is no difference in the effect of Covid-related unemployment across cities with and without far-right mayors. However, given that the number of municipalities led by far-right mayors is quite small, only 155 out of 7,922, we compute the p-values on the triple interaction coefficient via randomization inference, by permuting the assignment of far-right mayors to municipalities, as described in the main text. In Table H3, we repeat the analyses testing for reporting bias we perform in Section E on the triple-difference model. First, as for the main analyses, results are robust across reporting sources also when we consider differences between cities with and without far-right mayors (Columns 1-2). Second, while nonverbal crimes appear less likely in cities with high unemployment and far-right mayors after covid (Column 3), there is no statistically detectable difference in coefficients between this regression and that considering verbal hate crimes only (Column 4). Third, we confirm the finding that most of the effect of covid related unemployment on hate crimes is driven by individuals rather than by institutions, media, or politicians. Also in this case, when we test whether the coefficients in Columns 5 and 6 are statistically different, we reject the hypothesis that they are. Finally, we find consistent results when excluding social-media hate crimes from the count of events (Column 7). As for the main analyses, also in this case the p-value from the randomization inference returns a significant coefficient (p-value=0.0283).

Table H1: Covid-19 Unemployment Effects on Asian Hate Crimes by Prejudice

	Dependent variable: Asian Hate Crimes per 10,000 residents				
	(1)	(2)	(3)	(4)	(5)
Expected unemployment	0.00009*				
	(0.00004)				
After Jan, 2020	−0.00003*				
	(0.00002)				
High prejudice	0.00018*				
	(0.00008)				
After Jan, 2020 × high prejudice	−0.00010	−0.00010	−0.00006	−0.00006	−0.00007
	(0.00011)	(0.00011)	(0.00012)	(0.00012)	(0.00013)
Unemployment × high prejudice	−0.00013				
	(0.00010)				
After Jan, 2020 × unemployment	0.00133**	0.00133**	0.00106*	0.00104*	0.00103*
	(0.00047)	(0.00047)	(0.00041)	(0.00042)	(0.00041)
After Jan, 2020 × Unemployment × high prejudice	−0.00038	−0.00038	−0.00071	−0.00071	−0.00068
	(0.00069)	(0.00069)	(0.00075)	(0.00075)	(0.00074)
R^2	0.00001	0.00651	0.00665	0.00665	0.00671
Obs	1259477	1259477	1259477	1259477	1259477
N Municipalities	7922	7922	7922	7922	7922
Month FE	N	Y	Y	Y	Y
Municipality FE	N	Y	Y	Y	Y
Flexible controls	N	N	Y	Y	Y
Group-specific linear trends	N	N	N	Y	Y
Province-specific linear trends	N	N	N	N	Y

The dependent variable is monthly Asian hate crimes in a municipality per 10,000 residents. *After Jan, 2020* indicates the period after the first confirmed case of Covid-19 in Italy. *Expected unemployment* indicates municipalities with an above the median share of workers in affected sectors by Covid-19. *High prejudice* indicates municipalities with an above the median vote share for extreme right parties in national elections. Flexible controls include municipality population shares of foreign born and less than college educated interacted with month indicators. Group-specific linear trends and Province-specific linear trends correspond to the interaction between a continuous time measure and the municipality indicator of unemployment exposure, or province indicators, respectively. Municipality-clustered-robust standard errors reported in parentheses. Data are from Lunaria 2007 to March 2020, 2011 Population Housing Census, 2011 Industry and Services Census, Istat death counts 2017-2020. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Table H2: Covid-19 Unemployment Effects on Asian Hate Crimes by Far-right Mayors

	Dependent variable: Asian Hate Crimes per 10,000 residents				
	(1)	(2)	(3)	(4)	(5)
Expected unemployment	0.00000				
	(0.00006)				
After Jan, 2020	−0.00008				
	(0.00008)				
Far-right mayor	0.00075°				
	(0.00042)				
After Jan, 2020 × Far-right mayor	−0.00080°	−0.00080°	−0.00084°	−0.00084°	−0.00115*
	(0.00042)	(0.00043)	(0.00046)	(0.00046)	(0.00050)
Unemployment × Far-right mayor	−0.00051				
	(0.00048)				
After Jan, 2020 × unemployment	0.00119**	0.00119**	0.00082**	0.00080**	0.00079**
	(0.00037)	(0.00037)	(0.00027)	(0.00028)	(0.00028)
After Jan, 2020 × Unemployment × Far-right mayor	0.00387	0.00387	0.00309	0.00309	0.00330
	(0.00318)	(0.00319)	(0.00320)	(0.00320)	(0.00320)
R^2	0.00001	0.00651	0.00665	0.00665	0.00671
Obs	1253753	1253753	1253753	1253753	1253753
N Municipalities	7886	7886	7886	7886	7886
Month FE	N	Y	Y	Y	Y
Municipality FE	N	Y	Y	Y	Y
Flexible controls	N	N	Y	Y	Y
Group-specific linear trends	N	N	N	Y	Y
Province-specific linear trends	N	N	N	N	Y

The dependent variable is monthly Asian hate crimes in a municipality per 10,000 residents. *After Jan, 2020* indicates the period after the first confirmed case of Covid-19 in Italy. *Expected unemployment* indicates municipalities with an above the median share of workers in affected sectors by Covid-19. *Far-right mayor* indicates municipalities governed by a far-right mayor. Flexible controls include municipality population shares of foreign born and less than college educated interacted with month indicators. Group-specific linear trends and Province-specific linear trends correspond to the interaction between a continuous time measure and the municipality indicator of unemployment exposure, or province indicators, respectively. Municipality-clustered-robust standard errors reported in parentheses. Data are from Lunaria 2007 to March 2020, 2011 Population Housing Census, 2011 Industry and Services Census, Istat death counts 2017-2020. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Table H3: Covid-19 Unemployment Effects on Asian Hate Crimes by Far-right Mayors, Reporting Bias Test

	Dependent variable: Asian Hate Crimes per 10,000 Residents, restricted to:						
	(1) Source: drop National media	(2) Source: drop Individuals	(3) Type: Not verbal	(4) Type: Verbal	(5) Attacker: Person	(6) Attacker: Institutions	(7) Where: Not social media
After Jan 2020 × unemployment	0.00073***	0.00082***	0.00053**	0.00027*	0.00079***	0.00002	0.00078***
	(0.00024)	(0.00028)	(0.00024)	(0.00015)	(0.00028)	(0.00002)	(0.00026)
After Jan 2020 × Far-right	-0.00056*	-0.00089*	-0.00050*	-0.00020	-0.00035	-0.00053**	-0.00083*
	(0.00031)	(0.00046)	(0.00027)	(0.00018)	(0.00039)	(0.00025)	(0.00046)
After Jan 2020 × unemployment × Far-right	0.00343	0.00315	-0.00104*	0.00395	0.00256	0.00050**	0.00113
	(0.00314)	(0.00318)	(0.00062)	(0.00316)	(0.00318)	(0.00023)	(0.00259)
R^2	0.00657	0.00666	0.00714	0.00652	0.00658	0.00730	0.00661
Obs	1,253,753	1,253,753	1,253,753	1,253,753	1,253,753	1,253,753	1,253,753
N municipalities	7886	7886	7886	7886	7886	7886	7886

The dependent variable is monthly Asian hate crimes in a municipality per 10,000 residents restricting the analyses to one of the following categories: dropping crimes reported by national media (Column 1) or by individuals (Column 2); including only crimes that are not verbal (Column 3) or verbal hate crimes (Column 4); including crimes committed by individuals only (Column 5) or by institutions, media and politicians (Column 6); dropping crimes committed on social media (Column 7). *After Jan, 2020* indicates the period after the first confirmed case of Covid-19 in Italy. *Unemployment* indicates municipalities with an above the median share of workers in affected sectors by Covid-19. *Far-right* indicates municipalities governed by a far-right mayor. All columns include municipality and month fixed effects, flexible controls for the municipality population shares of foreign born and less than college educated interacted with month indicators, group-specific linear trends and province-specific linear trends. Municipality-clustered-robust standard errors reported in parentheses. Data are from Lunaria 2007 to March 2020, 2011 Population Housing Census, 2011 Industry and Services Census. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\circ}p < 0.1$.

I Validation of National Far-Right Vote as a Proxy for Prejudice

In the analysis, we use far-right voting in the 2018 national elections as a proxy for prejudice against immigrants. In this section, we test the validity of voting as a measure of prejudice using nationally representative public opinion survey data from Italian National Election Survey (Itanes) 2018 (N respondents=2,573). This survey includes questions on both voting intentions and on attitudes towards immigrants. This allows to test whether, for the same individual, there is a correspondence between hostility against immigrants and voting for far-right parties. A further advantage of this survey as a validation method is that it was administered right before the 2018 elections, meaning that public opinion data refer to the same period as the measure of prejudice based on voting we want to validate. We note that *Itanes* also collected responses after elections from the same individuals, but we use pre-election data for consistency with the voting measure.

The survey gauges attitudes towards immigrants by asking whether Italy receives too many immigrants on a scale from 1 (too few) to 7 (too many). We correlate this measure to voting intentions for the Lega and for Brothers of Italy, the two main far-right parties in this election cycle, in Figure I1. For both parties, we observe a strong positive correlation between voting and hostility towards migrants, with Lega prospective voters concentrated in the most hostile group. Since our voting measure includes also other smaller far-right parties for which Itanes did not record voting intentions (small parties are all grouped in 'Other'), we also test the correlation between hostility against immigrants and self-positioning on a left-right scale (1-7), another question included in the survey. Figure I2 shows an almost perfect mapping between each level of hostility against immigrants and self-positioning as right-wing. The correlations highlighted in these figures are robust to the use of individual level controls for education, employment, gender, age as well as province fixed effects, as reported in Table I1.

Table I1: Effect of far-right on hostility against immigrants, survey data

	Dependent variable: Exclusionary attitudes		
	(1)	(2)	(3)
Lega (0-1)	1.525***		
	(0.083)		
Brothers of Italy (0-1)		1.358***	
		(0.144)	
Self-positioning left-right (1-7)			0.298***
			(0.012)
Observations	2,488	2,488	1,768
R-squared	0.139	0.103	0.347
Province FE	Y	Y	Y
Controls	Y	Y	Y
Mean DV	5.265	5.265	5.265

Note: The dependent variable is the response to the question "Does Italy receive too many immigrants?" on a 1-7 scale. Lega and Brothers of Italy are self-reported voting intentions (0-1) for these parties. Self-positioning is the respondent's reported collocation on a left-right scale (1-7). All regressions control for education, gender, age and employment of the respondent and include province fixed effects.

This evidence confirms that, at least in our context, prejudice towards immigrants is a fundamental component of the far-right voter's ideology, making voting for far-right parties a good proxy for hostility against foreigners.

A related question is whether it is possible to distinguish empirically between voting for far-right politicians at the national level and electing a far-right mayor. We show that this is the case by presenting robust results when we run the triple-interaction analyses controlling for the prejudice variable in the regression considering far-right mayors and for far-right mayors in the regression with prejudice (Tables I2 and I3, Column (2) controls flexibly for one or the other variable).

Figure I1: Correlation between voting intentions for far-right parties and anti-immigrant attitudes

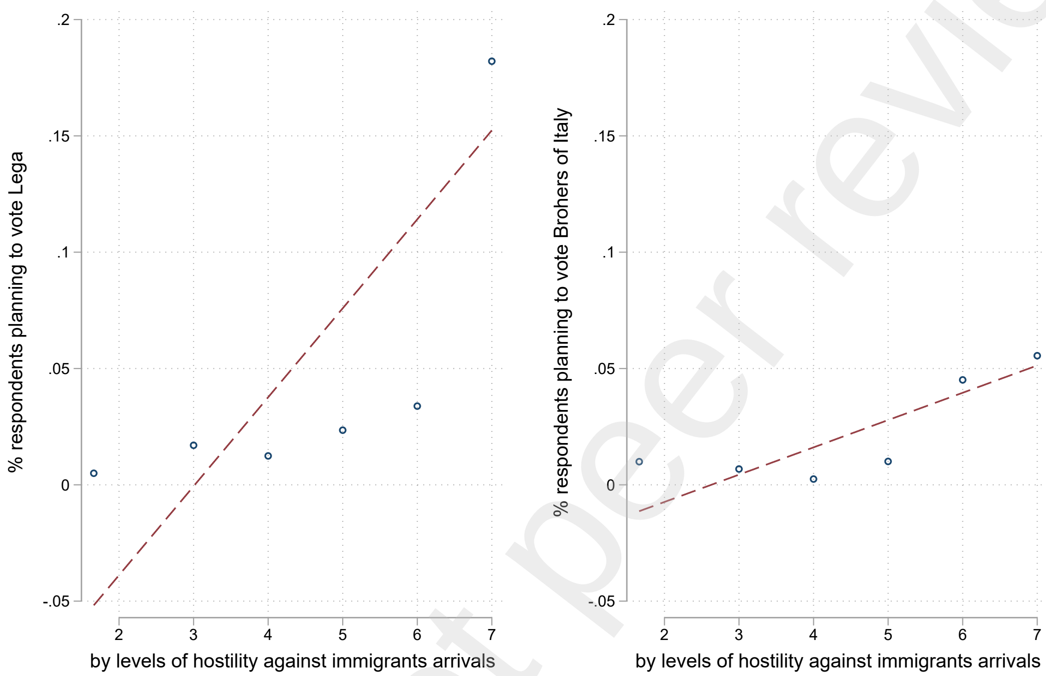


Figure I2: Correlation between self-positioning on a left-right scale and anti-immigrant attitudes

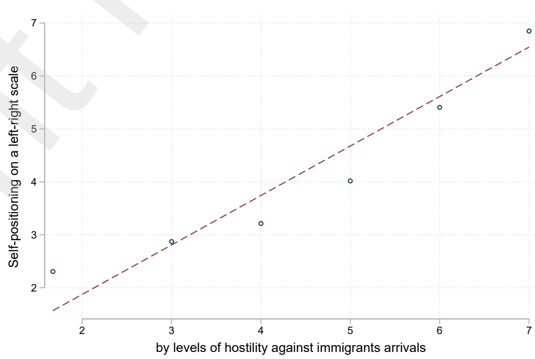


Table I2: Covid-19 Unemployment Effects on Asian Hate Crimes by Prejudice Controlling for Far-right Mayors

	Dependent variable:	
	Asian Hate Crimes per 10,000 residents	
	(1)	(2)
Expected unemployment After Jan, 2020		
High prejudice		
After Jan, 2020 × high prejudice	−0.00007	−0.00003
	(0.00013)	(0.00014)
Unemployyment × high prejudice		
After Jan, 2020 × unemployment	0.00103^{*}	0.00103^{*}
	(0.00041)	(0.00041)
After Jan, 2020 × Unemployment × high prejudice	−0.00068	−0.00069
	(0.00074)	(0.00074)
R^2	0.00671	0.00706
Obs	1259477	1259477
N Municipalities	7922	7922
Month FE	Y	Y
Municipality FE	Y	Y
Flexible controls	Y	Y
Group-specific linear trends	Y	Y
Province-specific linear trends	Y	Y

The dependent variable is monthly Asian hate crimes in a municipality per 10,000 residents. *After Jan, 2020* indicates the period after the first confirmed case of Covid-19 in Italy. *Expected unemployment* indicates municipalities with an above the median share of workers in affected sectors by Covid-19. *High prejudice* indicates municipalities with an above the median vote share for extreme right parties in national elections. Flexible controls include municipality population shares of foreign born and less than college educated interacted with month indicators in column (1), and column (2) adds an indicator for a municipality led government by a far-right mayor interacted with month indicators. Group-specific linear trends and Province-specific linear trends correspond to the interaction between a continuous time measure and the municipality indicator of unemployment exposure, or province indicators, respectively. Municipality-clustered-robust standard errors reported in parentheses. Data are from Lunaria 2007 to March 2020, 2011 Population Housing Census, 2011 Industry and Services Census, Istat death counts 2017-2020. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Table I3: Covid-19 Unemployment Effects on Asian Hate Crimes by Far-right Mayors Controlling for Prejudice

	Dependent variable:	
	Asian Hate Crimes per 10,000 residents	
	(1)	(2)
Expected unemployment After Jan, 2020		
Far-right mayor		
After Jan, 2020 × Far-right mayor	-0.00115^{*}	-0.00102^{*}
	(0.00050)	(0.00052)
Unemployment × Far-right mayor		
After Jan, 2020 × unemployment	0.00079^{**}	0.00064^{*}
	(0.00028)	(0.00027)
After Jan, 2020 × Unemployment × Far-right mayor	0.00330	0.00336
	(0.00320)	(0.00320)
R^2	0.00671	0.00684
Obs	1253753	1253753
N Municipalities	7886	7886
Month FE	Y	Y
Municipality FE	Y	Y
Flexible controls	Y	Y
Group-specific linear trends	Y	Y
Province-specific linear trends	Y	Y

The dependent variable is monthly Asian hate crimes in a municipality per 10,000 residents. *After Jan, 2020* indicates the period after the first confirmed case of Covid-19 in Italy. *Expected unemployment* indicates municipalities with an above the median share of workers in affected sectors by Covid-19. *Far-right mayor* indicates municipalities governed by a far-right mayor. Flexible controls include municipality population shares of foreign born and less than college educated interacted with month indicators in column (1), and column (2) adds an indicator for municipalities with an above the median right-wing vote share in national elections interacted with month indicators. Group-specific linear trends and Province-specific linear trends correspond to the interaction between a continuous time measure and the municipality indicator of unemployment exposure, or province indicators, respectively. Municipality-clustered-robust standard errors reported in parentheses. Data are from Lunaria 2007 to March 2020, 2011 Population Housing Census, 2011 Industry and Services Census, Istat death counts 2017-2020. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

J National Media Analysis: National Newspapers, Tweets, and Sentiment Classification

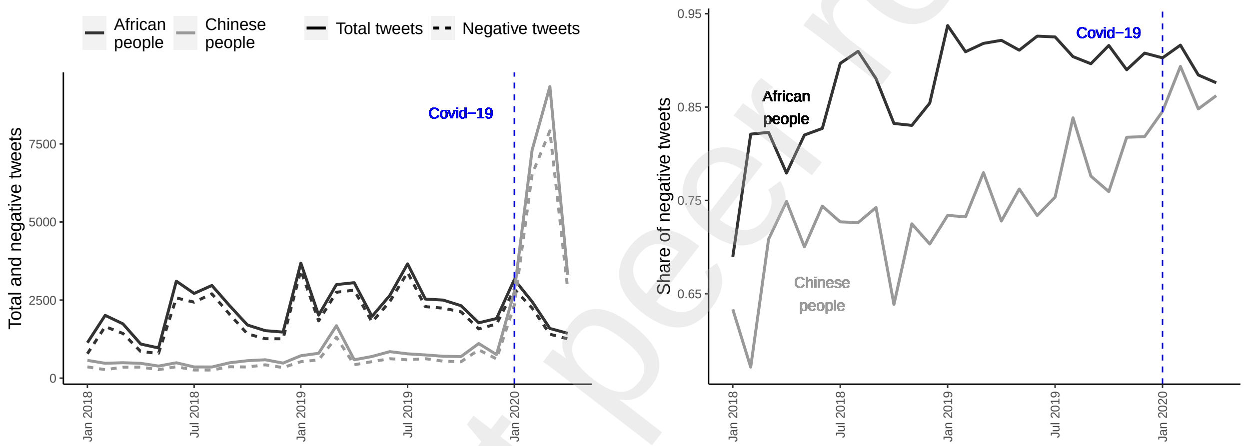
National Newspaper Data We collected all articles from 2018–2020 from the 17 Italian national newspapers using the following steps. To obtain the articles published by each of these newspapers, we looked up the newspaper URLs in Common Crawl (an open repository of web crawl data containing a snapshot of every web page at the moment of the crawl). Particularly in the Index for 2021-21 crawl, the most recent crawl at that moment. We retrieved the WARC (Web ARChive format) records for each crawled page from the newspaper, and extracted the pages' HTML. From the HTML, we extracted the text, title, and byline using the Python package `readabiliPy`, and the publication date using the Python library `htmldate`. In order to select the subset of articles that reference China or Chinese people (Africa and African people), we extracted mentions of the words *cinese*, *cinesi*, and *cina*, and the top 20 mentions of African countries and nationalities, as well as articles with the word Africa. The sample includes 17,500 articles, with 42% about China or Chinese people.

Twitter Data We acquired a random sample of 1% of all tweets in Italian from January 2018 to April 2020 from the Annenberg School for Communication at the University of Pennsylvania. In order to get a sample of tweets with mentions about China or Chinese people (Africa and African people), as done with newspaper articles, we extracted tweets with the words *cinese*, *cinesi*, and *cina*, and the top 20 mentions of African countries and nationalities, as well as tweets with the words Africa and its derivatives. This sample includes approximately 95,000 tweets, 35% of which contain mentions about China or Chinese people.

Sentiment Classifier and Validation To define the tone of speech about China and Chinese people (Africa and African people) we use the FEEL-IT Python package from Bianchi, Nozza and Hovy (2021) for inferring two-category sentiment from Italian text. In the case of newspaper articles, we classify as positive or negative every mention of our terms of interest (or every sentence including these terms). In the case of tweets, we classify every tweet containing these terms. To validate the sentiment classification, a human judge annotated in the two-category classification scale a sample of 130 article mentions and 300 tweets. Comparing the human annotations to the classification of the model for the positive and negative categories and defining the positive class as the negative sentiment category, we have that for newspaper article mentions the FEEL-IT sentiment annotator has an accuracy of 67%, weighted accuracy of 67%, precision of 64%, recall (or true positive rate) of 96%, and F1-score (or harmonic mean of precision and recall) of 77%. Although the model over-predicts the negative mentions as compared to the human annotations (the precision is 64%), it gives us a reasonable, if imperfect, measure of negative speech about China and Chinese people in the newspaper articles. This performance is comparable to other classifiers conducting similar tasks in English and Italian. The anti-Muslim speech classifier in Alrababah et al. (2019) achieves 0.7 weighted accuracy, and the hate speech classifier in Del Vigna et al. (2017) achieves 0.73, 0.63, 0.57, 0.59 in accuracy, precision, recall, F1 score, respectively. For tweets, the sentiment annotator has an accuracy of 76%, weighted accuracy of 78%, precision of 79%, recall (or true positive rate) of 93%, and F1-score (or harmonic mean of precision and recall) of 85%. These are reasonable statistics for sentiment classification, as compared to other classifiers performing similar tasks. Given that the FEEL-IT classifier is trained on a sample of annotated tweets from a broad range of topics, it is not surprising that its performance is better on tweets than in news articles.

National Media Analysis with Twitter Data We replicate our findings on the possible mobilization of hatred behavior by the national media with Twitter data. To assess the plausibility of a national shift in social norms we look at the trends of tweets referring to Chinese-origin people. The patterns in Figure J1a suggest that with the virus outbreak the salience of Chinese people in the public discourse increases; both the total number and the number of negative tweets about people from China significantly increase, whereas as a reference, tweets about people from African countries do not increase with the onset of Covid-19. Furthermore, a generalized fluctuation test on the trend of monthly negative tweets about Chinese-origin people confirms a structural break due to Covid-19 (Figure J2). Moreover, the pattern presented in Figure J1b suggests that the attitudes towards Chinese people are more negative than in previous months as the negative tweets' share trend increases with the pandemic's outbreak.

Figure J1: Italian Public Discourse Trends About Chinese People: Using Twitter Data



(a) Total and Negative Tweets' Trends

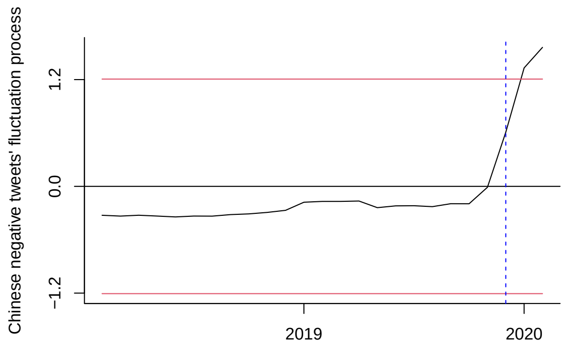
(b) Negative Tweets' Share Trend

Notes: In (a) *total tweets* indicates the monthly number of tweets about Chinese- or African-origin people, and *negative tweets* the monthly number of negative tweets about these two groups. (b) displays the ratio of negative to total tweets across the two groups. Twitter data is a 1% sample of all tweets in the Italian language.

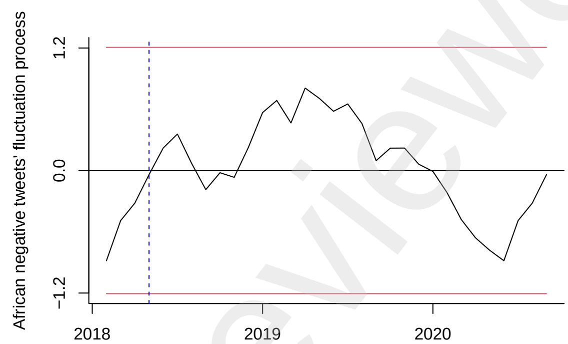
Negative tweets against China and Chinese people overwhelmingly involve Covid-19 related speech, identifying the country as responsible for the spread of the disease, expressing the belief that China should provide economic reparations to other affected countries, and indicating fear of contagion from Chinese individuals. Text analysis of negative tweets also reveals that those are more likely to include violent terms.[23]

[23] We search for words related to violence, killings, punching, stabbing and raping. Of the tweets containing violent words, 73% are in the sample of tweets displaying negative

Figure J2: Generalized fluctuation test for structural breaks in the number of negative tweets about Chinese- and African-origin people



(a) Empirical fluctuation process of negative tweets about Chinese people



(b) Empirical fluctuation process of negative tweets about African people

Notes: The empirical fluctuation process is computed via moving sums of residuals using the R package `struccchange`. The horizontal red lines indicate the boundaries of the limiting process fluctuation. The vertical line indicates the estimated optimal breakpoint.

K Media Coverage of Covid-19 and Unemployment

K.1 Salience of the Economic Consequences of Covid-19

We conduct a media analysis of the period January-March 2020 examining all news articles containing the words *Covid* and *Unemployment* in the two main Italian newspapers, Il Corriere and La Repubblica. This analysis documents that, already in this early period, media were reporting that unemployment would increase as a consequence of the virus diffusion and of the restrictions adopted to address it. Not only the Italian Institute for Statistical Analyses (ISTAT) anticipated that the tourism, transportation and restaurants sectors would suffer the largest loss in terms of employment in February, as mentioned in the manuscript, but similar information were widely reported by the media. La Repubblica uses the title *"The virus that kills employment"* in an article reporting that there would be *"social and economic consequences on all sectors, including tourism and transports"* (La Repubblica,

sentiment against asians and only 27% in the sample of tweets not classified as negative.

March 23, 2020). Already in February 2020, labor unions organized a conference to discuss the employment consequences of Covid-19, and asked the government to take actions to face the *"emergency in the tourism industry"* (La Repubblica, March 8, 2020).

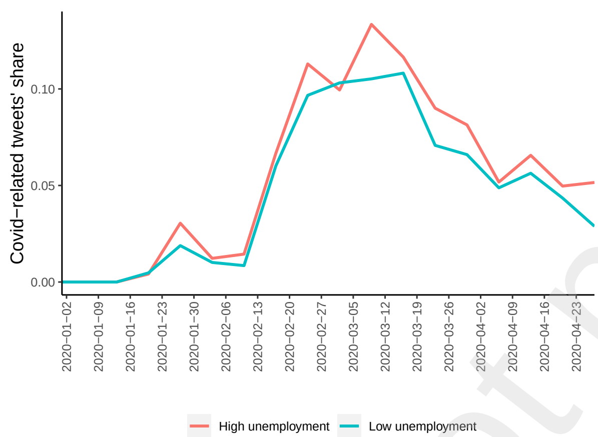
Consistently with a demand for actions from unions and public opinion, the national and regional governments took initiatives already from February, suggesting that in this period people might have already anticipated the negative impact of Covid-19 on the hospitality industry. After approximately a month of discussion, on March 12th, the Italian government approved and enforced a plan for the suspension of taxation to avoid weighting on workers' income (La Repubblica, March 12, 2020.), and on March 18th the President signed a plan to address the rising unemployment with subsidies directed to any worker, a large expenditure which would have been impossible to approve in absence of a national agreement on the seriousness of the unemployment emergency (La Repubblica, March 18th, 2020).

The results of this media analysis are confirmed by public attitudes towards Covid-19: a survey by the World Economic Forum shows that In February Italians perceived the pandemic as posing a high threat to their jobs and businesses, but less so to their health.

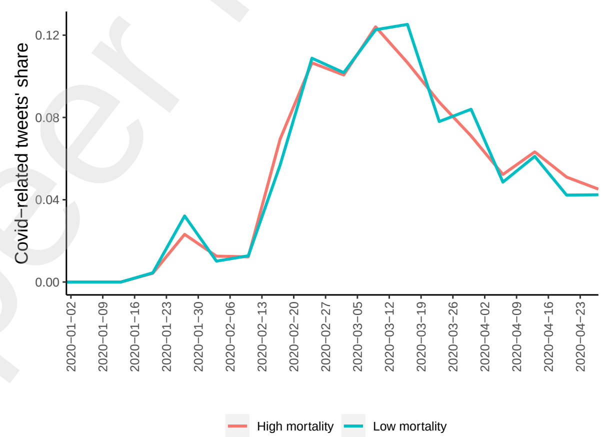
K.2 Economic Concerns in Places with High Unemployment

We use Twitter data to capture Italians concerns with Covid-19 since early February 2020. In particular, we approximate the degree of concern with the pandemic's outbreak with the number of Covid-19 mentions as a share of all tweets. To do so, we work with a random sample of 1% of all tweets in Italian from January 2018 to April 2020 that can be georeferenced to a municipality. From this sample, we extract all tweets with mentions of Covid-19 and related terms (e.g. using the stems corona, virus). To get a sense of whether Italians were more concerned in areas expected to suffer economically the most (given a municipality's sectoral composition), in Figure K1a, we compare the share of tweets mentioning Covid-19 across municipalities with a high share of their population employed in the hospitality industry (transportation, restaurants, hotels) versus municipalities with a low share of workers in such an industry. Figure K1a suggests that concerns about the pandemic peaked by the third week of February 2020, and that since early February, Italians in municipalities expected to be economically exposed were more concerned that people in less exposed areas. A difference-in-means test between the share of tweets mentioning Covid-19 during the first two weeks of February across these two types of municipalities is positive and statistically significant at the 0.02 level.[24] To the contrary, we find no difference across municipalities that differ in their number of excess deaths (Figure K1b; the p-value of the difference-in-means test is 0.4).

Figure K1: Trends of tweets with mentions of Covid-19



(a) Comparison of trends by Covid-related unemployment



(b) Comparison of trends by Covid-related mortality

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