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# ABSTRACT

## Hate in the Time of COVID-19: Racial Crimes against East Asians

We provide evidence of the impact of the COVID-19 pandemic on racial hate crime in England and Wales. Using various data sources, including unique data collected through Freedom of Information (FOI) requests from UK police forces, a difference-in-difference and event study approaches, we find that racial hate crime against East Asians increased by 70-100%, beginning in early February and persisted until November 2020. This increase was greatest in the weeks leading up to the first national lockdown in the UK. The shock was then lower during lockdown, before increasing again in the summer 2020. We present evidence that hate crime increased as COVID-19 cases in China increased and following announcements from the government signalling that China or Chinese individuals posed a public health risk to the UK. This indicates that protectionism played an important role in the observed hate crime spike. The hate crime shock was also positively correlated with the salience of the national lockdown and government policies restricting certain freedoms. The effect was driven largely by changes in London. This suggests that retaliation for lockdown contributed to the rise in hate crime.

**JEL Classification:** J15, C23, D63

**Keywords:** COVID-19, hate crime, xenophobia, difference-in-differences, event study

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# 1 Introduction

*Motivation* — On the 31st of December 2019 the World Health Organisation (WHO) announced a novel coronavirus in the city of Wuhan, China. Within the next four months over 100 countries initiated lockdowns and halted international travel, and within 18 months there were nearly 4 million COVID-19 (henceforth "pandemic" or "COVID") deaths registered. Slowing the spread of the virus was the primary concern of the pandemic management.[1] However, its effects on the economy and society have been far reaching and include conspicuous increase in violence and xenophobia against racial minorities, especially those perceived to be ethnically Chinese. In this paper we estimate the COVID-19-related increase in hate crime against racial minorities in England and Wales and investigate various mechanisms behind the hate crime contagion.

From the Black Death to the more recent Spanish flu or Ebola, pandemics have brought forth not only disease but also violence and animus toward minorities (Trauner, 1978; Herek, 1999; Voigtländer and Voth, 2012; Prati and Pietrantoni, 2016; Clissold et al., 2020). Onset of COVID-19 and the information of its Chinese origin led to an increase in unfavourable views of China (Silver et al., 2020) and attacks on those perceived to be ethnically Chinese, particularly in western countries, including the UK. Most attacks were perceived as being directly motivated by the pandemic. Already by mid-February 2020 UK news networks reported an increase in discrimination against the British Chinese (Campbell, 2020).[2] COVID-related discrimination spilled over to the labour market, with Chinese unemployment increasing three times more than that of other ethnic groups in the UK (Francis-Devine and Foley, 2020).

Understanding the relationship between COVID-19 and hate crime as well as its magnitude is of particular importance for policy. Hate crime and prejudice produce

[1] See https://www.gov.uk/government/speeches/pm-statement-at-coronavirus-press-conference-3-march-2020

[2] In the US, the STOP AAPI (Asian American Pacific Islander) HATE reporting centre received 1,710 reports of COVID-19 discrimination from Asian Americans in the six weeks to 19th March 2020 (Horse et al., 2021). Another US survey found that over half of Chinese respondents reported experiencing direct in-person discrimination due to the pandemic (Cheah et al., 2020). In an Australian survey, two-thirds Chinese-Australians reported workplace discrimination (Biddle et al., 2020).

significant externalities and have sizeable impact on the victims and targeted communities. The negative effects of discrimination or perceived discrimination on mental and physical health of victims and communities (Li, 2014; Sawyer et al., 2012; Tynes et al., 2019; Padela and Heisler, 2010; Fowers and Wan, 2020) as well as birth outcomes (Novak et al., 2017; Gemmill et al., 2019) are well-documented. This includes research on depression and mental health of East Asians during the COVID-19 pandemic (Chen et al., 2021; Wu et al., 2021).

Hate crime is also harmful to social cohesion (Keel et al., 2022) and has externalities on the broader community (Paterson et al., 2019). For example, Gould and Klor (2016) find a reduction in assimilation of Arabs in the United States after a post-9/11 hate crime spike, while Deole (2019) shows that far-right terrorism reduced refugee assimilation in Germany.

*What we do* — In this paper we investigate whether the pandemic led to an increase in recorded hate crime against East Asians and other ethnic groups (Asians, Black people and Europeans) in England and Wales. This could be due to a number of factors, such as changes in mobility as the virus spread and governments reacted, retaliation or protectionism by individuals. We also analyse the dynamics of the relationship across stages of the pandemic.

We utilise difference-in-differences and event study models using other hate crime biases – homophobic, transphobic, and disability – as control groups. This allows us to control for the effect of reduced mobility of individuals on hate crime and isolate the effect of other mechanisms such as retaliation and protectionism, which are likely to change at the same time. To be precise, we expect other hate crime biases to be only directly affected by pandemic-induced mobility changes, which incapacitate the offender and victim, thus reducing potential interactions. We do not expect them to be affected by retaliation or protectionism. To confirm this reasoning we show that there were no significant changes in hate crime rates among these control groups except during periods of the first national lockdown. We distinguish between the effects of lockdowns and periods of less severe restrictions. We use a unique data set of tweets to explore the potential mechanisms affecting only racial hate crime, such as protectionism and retaliation.

We document the following; First, hate crimes against East Asians increased from as soon as early February 2020 and the higher levels of hate crimes against this group persisted until November 2020. Second, racial hate crimes against other groups did not increase until the end of the first national lockdown in June 2020 and remained elevated until autumn 2020. On the other hand the control crimes decreased during the first national lockdown due to reduced mobility. Third, we find evidence that hate crimes against East Asians increased with the increase of COVID-19 cases in mainland China and as discussion of a UK lockdown increased on Twitter. The results suggest that significant protectionist and retaliatory mechanisms drove the increases in hate crimes against East Asians, while a combination of a substitution and incapacitation effects reduced racial hate crimes against other groups during lockdowns. Fourth, the observed increase in East Asian hate crime is primarily driven by London which we attribute to the particular negative psychological effect of a strict lockdown in a large urban area such as London. Finally, greater international spread of the virus – particularly in Europe – eventually led to an increase in racial hate crimes against all groups in the summer of 2020.

*Related Literature* — Much of the research on hate crimes focuses either on the effect of Islamic terrorism on anti-Muslim hate crimes (Swahn et al., 2003; Deloughery et al., 2012; King and Sutton, 2013; Hanes and Machin, 2014) or political events (Levin and Grisham, 2016; Jenkins, 2017; Edwards and Rushin, 2018; Müller and Schwarz, 2021; Carr et al., 2020). More recently, Ivandić et al. (2019) examine the effect of a jihadi terrorist attack (Manchester bombing) on anti-Muslim hate crimes in Manchester. In these cases, the subsequent hate crimes are categorised as retaliation. The perpetrator is motivated by a desire to retaliate against a perceived attack on their community. While the situations are different, the COVID-19 pandemic could be perceived by some as an attack on or a threat to their communities by a foreign entity (e.g. China). Furthermore, in their analysis of the effect of the EU referendum in the UK on racial and religious hate crimes, Carr et al. (2020) argue that public information shocks can change the expected benefits of hate crimes and, therefore, have a significant effect on the number of hate crimes committed. In the context of COVID-19, we expect information on the origin and threat of the virus to affect the perceived benefit of committing a hate crime against those perceived to be Chinese.

In response to media reporting of a proliferation of anti-Chinese incidents, there have been some attempts to quantify the effect of the COVID-19 pandemic on hate crimes or incidents against ethnically Chinese individuals (Gray and Hansen, 2021; Dipoppa et al., 2021). Other literature has examined how views toward foreigners or minorities changed during the pandemic (He et al., 2020; Bartoš et al., 2021). In addition, Cao et al. (2022) document a rise in anti-Asian hate crime in the US in response to Donald Trump's "China Virus" tweets.[3]

*Our contribution* — We join complementary literature analysing how racial animus against East Asians increased during the COVID-19 pandemic (Dipoppa et al., 2021; Gray and Hansen, 2021; Zhang et al., 2021; Lu and Sheng, 2022). We contribute to this topic by using high-frequency data to analyse changes over the pandemic and to measure the effect of policy changes and government signals, both in the short and long run. Another contribution is the consideration of other ethnic groups to verify whether racial animus spilled over to others. We also contribute to the literature looking at the effect of the pandemic on criminal activity (Boman and Gallupe, 2020; Langton et al., 2021; Leslie and Wilson, 2020; Abrams, 2021; Campedelli et al., 2021).

Furthermore, we are the first to estimate the effect of protectionism–the desire to protect in-group society from those perceived to be foreign or outsiders–on racial hate crimes as previous research concentrates on the retaliatory effect. We do not argue a causal effect of our proposed mechanism measures due to simultaneity between the severity of the pandemic, policy response to the pandemic, reallocation of resources, slowdown of judicial system in lockdowns, etc. However, our results provide policymakers with information on how different sentiments were correlated with the observed hate crime shock and what effect announcements and policy changes may

[3]Outside of the context of the pandemic but related, Müller and Schwarz (ming) show that when Trump tweeted about Muslims this led to increases in hate crimes in the following days.

have on racial hate crime.

The remainder of the paper is structured as follows. Section 2 presents the background and theoretical framework. Section 3 contains definition of hate crime, description of data sources used in the analysis and econometric methodology. Section 4 presents the main results, and several robustness checks. Section 5 explores a number of mechanisms and explanations of the main results. Section 6 concludes the paper.

# 2 Background

## 2.1 COVID-19 in the United Kingdom

The UK confirmed the first cases of COVID-19 on the 31st of January 2020 among two Chinese nationals who arrived in the UK from China.[4] Around the same time the UK government announced a coordinated evacuation of British nationals from Wuhan and their repatriation to the UK. By the 1st of August 2021, the country recorded a total of 5.88 million confirmed cases (87,022 per million population) and 130,014 COVID-19 related deaths (1,914 per million population). Throughout the pandemic the government attempted to maintain steady communication, for instance through information campaigns and televised press conferences, which occurred daily during lockdowns (starting on the 16th of March 2020) and on a need-to-have basis at other times.

Since health is a devolved matter in the UK, its countries have the freedom to introduce separate measures in their jurisdictions. Indeed, there were differences in implementation and timing of restrictions across the UK, starting from lifting of the first national lockdown. Below we provide a brief, chronological overview of the UK government's response to COVID-19 developments. Many of the restrictions imposed

[4]However, there are some suspected earlier cases under investigation. Moreover, by now genetic sequencing traced most imports of the virus to Italy (late February), France (mid-to-late March) and Spain (early-to-mid March).

were justified by rapid increases in the infection rates and deaths related to COVID-19, and resultant pressures put on the UK National Health Service (NHS). We focus on measures introduced in England and Wales, given the geographical coverage of the data we use.

The UK government's initial response to the onset of COVID-19 has been reserved and relatively slow. First, a public information campaign was launched and the government introduced the Health Protection (Coronavirus) Regulations 2020 – both in February 2020. The initial lax approach was met with heavy criticism from opposition parties and scientists, which featured prominently in the media. It wasn't until March 2020 that the restrictions were elevated (including advice to self-isolate in case of symptoms). Schools closed on Friday, the 20th of March with two days' notice and lockdown was imposed on the 23rd of March without any prior notice – around the same time as in Greece or Germany but later than in Italy, Denmark, Spain or Poland.

*First national lockdown*

The first lockdown involved significant restrictions on movement of people. The government imposed stay-at-home orders. Non-essential travel and contact with other people outside of one's household was forbidden. Majority of schools, businesses and meeting spaces were shut. Social distancing measures were introduced with individuals asked to keep 2 meters apart, even in open air areas. Leaving home was permitted for essential purchases, essential work travel (if remote work impossible), medical needs and exercise outdoors once a day. The government enacted Coronavirus Act 2020 which gave it emergency powers and empowered the police to enforce the new measures. It was during this time that the Test & Trace system was developed. By the end of April 2020 the country registered 26,000 COVID-19 related deaths.

The restrictions were gradually lifted from May 2020 onward, at different speeds across the devolved nations. First, workers unable to work remotely were encouraged to return to work, exercise allowance was increased and travel within the UK restored. By the 28th of May 2020 groups of up to six people from different households were allowed to meet outside, whilst maintaining social distancing. Some children in primary schools were allowed to return to school as of 1st of June 2020.

*Summer 2020*

By mid-June non-essential shops were allowed to reopen and by the 4th of July 2020 most businesses were allowed to reopen. Gatherings of up to 30 people were allowed. At the same time, wearing face coverings in shops and supermarkets became mandatory in England. Moreover, those travelling to the UK from outside of the Common Travel Area were required to quarantine on arrival. Schools reopened full time from September 2020.

Summer was seen as a respite period. As cases in the UK fell and most restrictions were lifted, the government focused on economic recovery and encouraged spending. In fact the Chancellor introduced the Eat out to Help out Scheme[5] in August to encourage dining in restaurants and thus support hospitality sector, which was hit most by the closures.

*Autumn 2020 – differential measures*

After summer 2020 the COVID-management was diversified across the devolved nations of the UK, with England, Wales, Scotland and Northern Ireland all imposing own variations of restrictions, often following differing timescales. In particular, the areas managed differently the resurgence of COVID-19 cases and the second wave in the autumn of 2020. A system of local restrictions was developed and some areas of the UK, including Greater Manchester and parts of Yorkshire, saw an increase in restrictions between summer and autumn 2020.

September 2020 saw a reintroduction of restrictions to social gatherings – up to 6 people were allowed to meet. Some local restrictions on pub closing times were introduced in parts of northern England, followed by a blanket pub 10pm closure rule for the whole of the UK on the 22nd of September. In both England and Scotland, tiered restrictions were introduced in October and England went into a month-long lockdown on the 5th of November 2020, which was eased on the 2nd of December

[5] www.gov.uk/guidance/get-a-discount-with-the-eat-out-to-help-out-scheme

2020 and replaced by new tiered restrictions.

# 3 Data and Methodology

In England and Wales hate crime is defined as "any criminal offence which is perceived, by the victim or any other person, to be motivated by hostility or prejudice towards someone based on a personal characteristic," and is categorised by race or ethnicity, religion or beliefs, sexual orientation, disability and transgender identity (O'Neill, 2017). According to the College of Policing (2014) racial and religious hate crimes (RRHC) include any group defined by "race, colour, nationality or ethnic or national origin, including countries within the UK, and Gypsy or Irish Travellers." In comparison to other jurisdictions such as the US, hate crime recording in the UK is victim-centric. Recorded hate crime can include anything from verbal harassment including racial slurs and threats of violence, physical assault, and property damage (e.g. racist graffiti or damage to a cultural/religious institution, restaurant, business, etc.).[6]

## 3.1 Hate Crime Data – Freedom of Information Requests

In the United Kingdom individuals and groups can use the Freedom of Information Act to request from public bodies the release of information which is not yet publicly available (see Clifton-Sprigg et al., 2020). Doing so requires sending a freedom of information (FOI) request. The organisation then has 20 business days to reply to the request,[7] granting it or denying, citing an exemption. If the information is not provided the requester can appeal, at which point the organisation has an additional 20 days to review the original request.

We sent FOI requests to the 45 territorial police forces of the United Kingdom, including Police Scotland, 4 Welsh forces, and 39 English forces to obtain highly detailed hate crime data in the period January 2018 - December 2020. From each police

[6] See https://www.amnesty.org.uk/blogs/ether/hate-crimes-uk-victims-stories for hate crime victims' stories.

[7] This deadline was not strictly respected during the COVID-19 period due to staff limitations.

force we requested a list of hate crimes containing the following information: date, hate bias(es), location, offence group and ethnicity of the victim.

In response, 16 police forces – 2 Welsh and 14 English – provided the requested daily data but only 10 of them provided information on ethnicity of the victim. Therefore, the data set contains daily data from 10 police forces. In addition to that, 13 police forces – 1 Welsh and 12 English – cited concerns regarding victim identification as an exemption to the request and only provided the month of the crime, rather than the date. Finally, 3 police forces provided monthly count of hate crime by bias (racial, religion, sexual orientation, disability, transgender). As a result, monthly counts of hate crime are available for 32 of the 45 police forces in the United Kingdom[8] but complete monthly level information covers 18 police forces.

We pool the daily data and rely on the weekly dataset containing information on ethnicity from 10 police forces in England and Wales.[9] This dataset, despite containing fewer areas, is most suitable for answering the research question for two reasons. First, higher frequency data is required for fleshing out mechanisms as policies and mobility changed frequently. Second, ethnicity information is necessary for understanding how victim ethnicity changed throughout the course of the pandemic. In the next section we test the representativeness of the principle dataset.

Figure 1 provides a comparison of weekly hate crimes by ethnic group in 2020 and monthly hate crime for the entire sample period (2018-2020) and the hypothesised mechanism measures. Hate crimes against East Asians are lower on average than hate crimes against other ethnic groups (Asians, Black people, and Europeans) as well as homophobic hate crimes. The count for East Asian racial hate crimes, which had been increasing from the beginning of February as the pandemic grew in strength and the threat to the UK increased, is about one-tenth of the level of the other groups. However, given that the East Asian population in the UK is about one-tenth that of the other three groups, the likelihood of race crime victimisation is equal across the four groups (Office for National Statistics, 2012). During the national lockdown (23rd

[8]The remaining police forces have either not responded to the request or denied crime-level data.
[9]Results using daily data support the baseline weekly results and are available upon request.

of March to June 2020) there appears to be no change in hate crime victimisation. Hate crimes increase again from mid-June, with all ethnic groups experiencing a visual increase in victimisation in the third quarter.

*Representativeness of the sample*

Given the response rates to our request, we undertake checks similar to Clifton-Sprigg et al. (2020) to ensure representativeness of the data. They can be found in Tables 1-4 in the Appendix. We find that 96% of the police forces responded to the request and 76% provided us with some kind of data, though only a fifth of the forces provided the exact data requested – i.e. daily counts of hate crimes, by ethnicity of victim. Based on their characteristics, forces which provided data were larger, had higher funding per 100 residents in the area, lower proportion of hate crimes per population and a higher proportion of residents who were not born in the UK. However, none of the differences are statistically significant (Table 1). Using regression analysis we find that no correlations exist between characteristics of the police force (size, funding), labour market indicators of the area (unemployment rate, GDHI per capita), and area demographic and crime controls (hate crimes, population of working age) and provision of data or of the correct data. One notable exception is the percent of population in the area who were not born in the UK; this control variable is positively correlated with the police force provision of data, but not of the right data (Table 2). The results do not change once we drop London-based police forces from the sample, acknowledging that crime occurrence as well as demographics differ significantly between London and the rest of the UK (Table 3). We find no evidence of correlations between these determinants and late response to the request or refusal to provide data (Table 4). Therefore, there is no strong evidence to suggest that the FOI data used in the analysis are not representative of all police forces.

*Data transformation*

Due to the fact that the calendar year–including leap years such as 2020–is not divisible cleanly by week (365 and 366 is not a dividend of 7) the last week of the calendar year contains 8 or 9 days. To ensure that this does not have any impact on the results we standardise the weekly count by the number of days in that week for all variables. Therefore coefficients can be interpreted as changes in the daily average while still using weekly data.

For this research the reported ethnicities are aggregated by broad ethnic groups—East Asians, Asians, Europeans, and Black people. East Asians, the main group of interest, contain victims of Chinese, Japanese, and Korean ethnicities who we posit are most likely to be perceived as Chinese by offenders. This is motivated by research in the other-race effect (ORE) which finds individuals have poorer ability to recognize other-race than own-race faces and likely would not be able to differentiate ethnicities within the broad ethnic group (see Meissner and Brigham (2001) for an overview of ORE). While there is still a possibility of confusion by perpetrators between Asians and East Asians, we believe that it is less-likely in the UK and would lead to an underestimation of the anti-East Asian animus.

## 3.2 Other Data

We complement the hate crime data with publicly available data sets on COVID-19 and our own data from Twitter to explore the mechanisms behind the baseline results. Summary time series graphs can be found in Figure 1 and 2. The data collection will be described below with a discussion of the co-movements following in Section 5.

To test the effect of protectionism on hate crimes we use data on COVID-19 cases and deaths by country,[10] China being our main country of interest. We expect that an increase in COVID-19 cases in China may lead to protectionist instincts among the natives who then use xenophobia and hate crime to "protect" the UK from potentially-infected foreigners (Chinese at the beginning). Moreover, to capture the salience of discussion of cases in China, we use a weekly count of tweets on COVID-19 cases in China.

To look at the effect of scapegoating or blaming China, and by extension those perceived as being ethnic Chinese, we collected Twitter data consisting of tweets includ-

[10] Available from the World Health Organisation

ing keywords or hashtags blaming or connecting China and the coronavirus: "China virus", "Kungflu", etc (see Deng and Hwang (2021) for additional research on COVID-19 and Twitter language). We then aggregate the data into a weekly count of tweets in the UK (no variation by area, only time).[11] Details of the keywords used in various Twitter searches in this paper can be found in Table 5.

To test the effect of (self-)incapacitation we use Google mobility data. Google mobility data contain the number and length of visits to different places–parks, transit stations, retail and recreation, residential, and workplaces–compared to the baseline period of 3rd of January to 6th of February 2020. These data (and other mobility data sets) do not contain a full set of information for the sample period (2018-2020) but rather changes in mobility beginning on the 15th of February 2020. Therefore, the effect of mobility on hate crime is restricted to the COVID-19 context and external validity in "normal" times is limited. Figure 1c shows that mobility to all places except residential areas decreased at the announcement of the first national lockdown, with mobility to parks increasing steadily.

We complement the mobility data with a second data set – the OxCGRT systematic data set on COVID-19 policies from the University of Oxford.[12] The data contain information on 23 indicators of government response, including containment and closure policies, economic policies, health care policies, and vaccination policies. For this research we are primarily interested in containment and closure policies which would forcibly incapacitate victims and offenders (due to consequences of violating policies) and self-incapacitate beyond the rules as these policies act as information shocks of the prognosis of the disease spread. However, we are also interested in the effect of economic policies on changes in hate crimes during the pandemic as these policies may mitigate some of the economic uncertainty and hardship caused by the government and individual responses to the pandemic.

Finally, to consider possible substitution effects between online and offline hate we use a second Twitter data set consisting of scrapped tweets with sinophobic language

[11] Tweets are aggregated by week to reflect crime data. Tweets are at the national level as local variation is not possible given the Twitter's search and API parameters.

[12] Source: https://github.com/OxCGRT/covid-policy-tracker

(see Table 5). Similar to the previous Twitter data set, this is aggregated into a weekly count of tweets in the United Kingdom.

Summary statistics of all variables, including hate crime by group, can be found in Table 6. These variables will be discussed further at length in the mechanisms section (Section 5).

### 3.3 Empirical Methodology

To estimate the effect of the pandemic on racial hate crime by ethnic group we first use a difference-in-differences (DD) estimator, using other hate crime biases as control groups and empirically testing the parallel trends assumption. The post-period begins on the 1st of January 2020 following the WHO announcement with the pre-period consisting of 2018 and 2019. Then we further split the COVID-19 pandemic into four treatment stages: prior to the national lockdown (1st of January - 23rd of March), national lockdown (24th of March - June), summer (June - August), second wave and lockdown (September - December).

We then expand the analysis by using an event study design. This allows us to test the temporal effects of the pandemic as well as the parallel trends assumption. We use October and November 2019 as our baseline period rather than all early months in our sample in order to explicitly test the parallel trends assumption. The choice of the baseline months is motivated by the fact that there was general election in the UK on the 12th of December 2019 and the news of a virus in Wuhan circulated around the same time. Therefore, we use the following regression equation:

$$y_{at} = \sum_{t=-2}^{14} \alpha_t(I_t) + \sum_{t=-2}^{14} \beta_t(R_a \times I_t) + \psi_t + \theta_a + \varepsilon_{at} \tag{1}$$

where $y_{at}$ is the logarithm-transformed number of recorded hate crimes against group $a$[13] in week $t$. $R_a$ is an indicator variable for the four treated ethnic groups. $\psi_t$ and $\theta_a$ control for fixed month-of-year and crime effects, respectively. Our event study horizon includes four pre-treatment time periods (including the baseline t=0) and 14 post-treatment periods. This allows us to look at the temporal changes in hate crime

[13]East Asians, Asians, Black people, white Europeans, homosexuals, transgender, disability.

during the COVID-19 period. $R_a$ is a treatment dummy variable equal to 1 for each of the four groups of racial hate crimes and equal to 0 for the control hate crimes: homophobic, disability, and transphobic. An additional control includes a dummy variable capturing the Hong Kong protests of June 2019 to control for the effect of the large protests on anti-Chinese hate crime in the UK. Finally, $\varepsilon_{at}$ are standard errors clustered at the hate crime-year level.

In our baseline models we use the log number of hate crimes aggregated across the 10 police forces. Later, as robustness checks in Section 4.2 we also test the effect using the crime count as the outcome variable and using monthly data containing hate crime information from 23 police forces. These results confirm the baseline findings of a positive and significant increase in racial hate crimes against East Asians.

We use other hate crime biases as the control group.[14] This way we account for changes in incentives to commit hate crimes and recording practices that are not specific to racial hate crime. Secondly, hate crimes tend to move together over time except when a specific group of people is hit by a shock event. These shock events, such as terrorist attacks or the EU referendum in the UK, usually increase the occurrence of crimes against racial minorities but not the other hate crimes. In the case of the COVID-19 pandemic, the global nature of the event and its origin in China would only impact the cost-benefit of committing a racial hate crime – specifically against those perceived to be Chinese – leaving other hate crimes only affected through incapacitation caused by lockdowns. By controlling for other hate crimes we can separate the effect of changes in mobility and in other factors, such as protectionism and retaliation. This helps us better understand the mechanisms which contributed to the observed increase in racial hate crimes and provide clearer policy implications. Using other hate crimes as control groups also allows us to account for changes in the reporting or recording standards of hate crimes by the police forces.

It is assumed that, in the absence of the pandemic, racial hate crimes would follow the same trend as the control hate crimes. We are able to empirically test this assumption by checking for a "treatment" effect in the periods before the baseline

[14] Religious hate crimes are excluded due to the ambiguity of whether they are treated by the pandemic.

of October and November 2019. For the assumption to hold the coefficient(s) of the interaction between the treatment dummy (=1 for racial hate crime) and the pre-treatment time dummies should be insignificant (see Figure 3, panel (b)). In order for the parallel trends assumption to not be violated the coefficients of panel (b) prior to our baseline period–December 2019–must be insignificant.

We consider the effect of COVID-19 on racial hate crimes by ethnic group to take into account possible substitution effects and gain a better understanding of mechanisms. An alternative approach would be to use racial hate crime against other ethnicities as a control group. Other ethnicities would be less appropriate as a control since they may also be targeted. This would lead to bias in the estimated treatment effects. Other ethnic groups may be targeted if all foreigners are perceived as potentially spreading the virus and its newly emerging variants. Second, even if other ethnic groups are not targeted more due to the pandemic itself, the potential substitution effect would result in an overestimate of the treatment effect on East Asians.

Therefore, the $\alpha$ coefficients in Equation 2 could be interpreted as the expected change in all hate crimes due to (self-)incapacitation. The $\beta$ coefficients would then capture the changes in racial hate crimes that are unexplained by changes in mobility, or the joint COVID-19 effect of other mechanisms on racial hate crimes. The total impact of the pandemic (sum of mobility, scapegoating, and protectionism) is provided by the sum of the coefficients by time period. Put differently:

1. What would have happened to racial hate crime in the absence of the pandemic $= \alpha_t + \beta_t$.

2. What would have happened if COVID-19 did not change the cost-benefit of a racial hate crime, net of mobility changes $= \beta_t$.

We are most interested in the latter question as incapacitation is a well-understood mechanical process that reduces all crime, while understanding the mechanisms that increase racial hate crime is of greater value for policymakers.

The benefit of this method is that it controls for the mobility in England and Wales, while also allowing and capturing substitution between ethnic groups that cannot be explained by mobility. We do not believe that there is substitution between racial hate crimes and homophobic, transphobic, and disability hate crimes. We expect any effect on these crimes to be due to COVID-induced incapacitation and other hate crimes to decrease in periods of greater incapacitation but remain unchanged in the summer months when there are near-normal levels of mobility. This hypothesis is formally tested in Section 5 where we measure separately the effect on the control group—or $\alpha_t$—to find the effects of mobility and to implicitly verify the suitability of the control group.

# 4 Results

## 4.1 Baseline Results

We first present the results of a traditional DD model with a single post-treatment period and empirically test the parallel trends assumption (see Table 7).[15] We find that in 2020 hate crimes against East Asians rose by 91.9 percent, against Asians by 19.3 percent, and against Europeans by 28.1 percent. No significant increase in hate crimes against Black people is observed across 2020. For each ethnic group we fail to reject the null hypothesis that the parallel trends assumption holds.

In a second step we look at the average effect of the pandemic across four different stages: pre-lockdown, during first national lockdown, immediately post-lockdown and during the second wave of autumn 2020. In Figure 3a, panel (a) we observe a significant increase in racial hate crimes against East Asians across all time periods. Hate crimes increased against this group by 70-100 percent in the first three periods.

Hate crimes against Asians and Europeans also increased pre-lockdown and remained elevated, but the spike is smaller than for the first group. Hate crimes against these two groups may have increased early in the pandemic due to the initial spread of the virus from China to Iran and Italy. Moreover, changes in hate crime against Europeans (and other ethnic groups to a lesser extent) may have been due to Brexit-

[15]Parallel trends assumption tested in Stata 17, the null hypothesis of the test is that the parallel trends assumption is not violated (McCaffrey and Bell, 2003; Donald and Lang, 2007).

related events such as the UK general election (12th of December 2019) and the official departure from the European Union (31st of January 2020)–both of which could potentially motivate additional racial hate crime.

The spike in hate crimes against Europeans and Black people was greatest in the summer (post-lockdown). This could be due to the fact that the pandemic was more severe in Europe compared to Asia while Black people in the UK were likely affected by the Black Lives Matter protests that began in late May 2020.

We next turn to the event study results to estimate more-detailed temporal effects of COVID-19 and containment policies (see Figure 3b, panel (b)). First, we find no evidence of a violation of the assumption of parallel trends prior to the pandemic as the "treatment" effects in the pre-baseline periods are insignificant across all four ethnic groups.

Hate crimes against East Asians increased significantly in late January and mostly persisted at higher levels throughout the pandemic as evident by the elevated plot for East Asians relative to the baseline in Figure 3. By the second month of the national lockdown racial hate crimes against all groups are significant as all plots are above the dashed line until mid-August 2020, though the magnitudes are smaller than for East Asians.

Hate crimes against East Asians remained at elevated levels after the national lockdown and once the spread of the virus in China was contained. At the same time mobility increased gradually from June. During summer 2020 the COVID-19 situation in China was optimistic and the epicentres of the virus were Europe and the Americas. However, it is clear that despite China no longer being a "threat" with regard to spreading the virus, East Asians were still being attacked at higher levels than expected. This suggests that protectionism cannot be the only mechanism at work.

The magnitude of the shock in racial hate crimes against all ethnicities decreased in the late summer and early autumn as the estimates are closer to the dashed line and have larger errors measured by the plot's vertical lines. The shock becomes positive and significant against East Asians again in mid-October as cases again rose but returns to expected levels in the last weeks of 2020.

There is little evidence that hate crimes against other groups decreased significantly relative to the control group. Given that hate crimes against the three other ethnic groups were 10 times more frequent than hate crimes against East Asians, there would only need to be a small relative decrease in other racial hate crimes to compensate a large relative increase in racial hate crime against East Asians. This relative increase may be too small to be precisely estimated. For this reason we later look at how crime counts changed. It will help us better understand any substitution effect (See Section 4.2 for details). The lack of a significant effect on the other groups is unsurprising. These groups should not be affected by the beginning of the pandemic as most of the blame or concern was toward China (and to a much lesser extent, Europe). Therefore, we expect the same temporal effects as the control group absent of a substitution effect.

## 4.2 Robustness checks

We perform two robustness checks of our baseline difference-in-differences model using weekly data with the findings confirming our baseline estimates.

First, we repeat the analysis using crime count as a dependent variable (see Figure 4). Due to the fact that, by construction, the last week of the year contains more than 7 days, the outcome is the daily average of hate crime count in the week. Looking at the effect on the crime count (level), we find what appears to be relatively small effects on East Asians. However, it is important to remember that racial hate crimes against other ethnic groups are about 10 times more frequent than hate crimes against East Asians. Therefore, a similar shock in levels would be a ten-fold greater relative increase for East Asians. Again, we find evidence that hate crimes increased in the lockdown against Europeans, with large (level) shocks being observed in the summer; an average increase in daily hate crimes against this group is 9 incidents. These results also suggest that there was no substitution effect between the groups as there is no corresponding decrease in non-East Asian racial hate crime to compensate the rise in racial hate crime against East Asians.

In Figure 4 we show the results for East Asians are robust to the measurement of the outcome variable when using the daily average crime count for each calendar week. We find that reported hate crime against East Asians increased by 2-3 cases per day throughout the pandemic in the 10 police forces. Compared to the baseline results we find an insignificant effect in the other three ethnic groups in the first two periods. However, we persistently find significant increases in the post-lockdown period during the summer of 5-9 crimes per day for the other three ethnic groups.

We then verify the results using (aggregated) monthly data from 23, rather than 10 police forces as 13 additional forces provided monthly hate crime data (see Figure 5 Panel (a)). We find similar results as our baseline analysis. However, the parallel trends assumption is violated for East Asians as we observe significant changes in the outcome variable in the pre-pandemic periods relative to the control groups. One explanation for this is that the monthly data contain fewer observations and as such the trends and seasonality are imprecisely estimated.

In a final robustness check we restructure the event study periods, specifically the pre-baseline (October and November 2019) periods to create 5 pre-periods (January to June 2018, June to December 2018, January to May 2019, June to September 2019 and October to November 2019). The results are in line with those presented in baseline.

# 5 Mechanisms

## 5.1 Conceptual framework

Following the Becker crime model (Becker, 1968) we model the choice of committing a racial hate crime as a cost-benefit analysis by a potential offender. In the context of COVID-19, offenders will commit more hate crimes if they believe that the benefits of crime, e.g. lowering of the risk of disease transmission from a "foreigner" to a "native", have increased relative to the costs. However, COVID-19 increased costs as well as benefits of committing a hate crime, as violation of lockdown rules came with a potential financial penalty and a greater risk of catching the virus.

Below we outline six mechanisms through which COVID-19 pandemic may influence occurrence of hate crime, with the theoretical direction of the relationship indicated in parentheses. We focus particularly on the period leading up to and the first month of the national lockdown.

1. Protectionism (+) — as cases rise in China (and other places), hate crimes against ethnic groups of these origins will increase in order to "protect" UK society (natives) from the virus. This mechanism is further motivated by research on hate crime motives by McDevitt et al. (2002).[16] To test this we use data on COVID-19 infections and deaths in different countries and regions, including China and the European Economic Area.

2. Scapegoating or retaliation (+) — as COVID-19 spreads and leads to negative consequences, individuals may increasingly blame China, Chinese or ethnically-Chinese individuals for the pandemic. To test this we use original data on tweets containing language associating COVID-19 with China or ethnically-Chinese individuals.

3. Incapacitation (−) — government policies in response to the pandemic restrict individual movement. This reduces interactions between offenders and victims. In the case of COVID-19, both victims and offenders would be incapacitated due to lockdown and stay-at-home orders. Incapacitation can also include self-incapacitation, where individuals choose to reduce their mobility due to their perceptions of the public health risk. We test the relationship between mobility and hate crime during the pandemic using data on government responses to COVID-19 and local mobility data from Google. We also implicitly control for mobility – incapacitation and self-incapacitation – in our econometric framework by using other hate crimes.

4. Economic hardship (+) — COVID-19 led to an increase in economic instability

[16] The authors categorised four hate crime offenders: retaliatory, defensive (protectionism), thrill seeker, and mission using a case study of hate crimes committed in Boston, Massachusetts.

which may increase anger toward minorities or those perceived to be foreign. To test this we use data on governmental economic aid during the pandemic, tweets on unemployment, and tweets on economic recession in the UK.

5. Substitution ($-$) —- when faced with various restrictions, xenophobes may switch from in-person attacks to online hate speech. This can include public posts (Twitter) or more insular discussion (4chan, parlor, etc.). Xenophobes may also substitute between ethnic groups based on the current COVID-19 and societal events (including the Black Lives Matter protests of the summer 2020). To test this we compare the temporal effects of the pandemic on racial hate crimes by ethnic group. In the case of substitution across ethnic groups hate crimes against other groups will decrease as hate crimes against Chinese increase. We also look at the relationship between online and offline hate using tweets containing sinophobic language.

6. Reporting ($+$) — Media reporting on and social media salience of hate crimes during the pandemic may have led to an increase in the reporting of victimisation to the police, thereby raising recorded hate crime without an increase in its occurrence. To capture social media salience we use the count of tweets containing "*#coronaracism*".

## 5.2 Analysis

Following the baseline DD and ES models, we attempt to disentangle and measure the mechanisms discussed in Section 2. To this end, we consider the effect of 1) mobility, 2) policies and 3) social media on hate crime by ethnicity using a fixed effects panel approach. We rely on public data on policies and cases, unique data on Twitter use, and comparisons across ethnic groups where the incentives would differ.

The challenge lies in disentangling the various factors at play. Ideally, we would use exogenous policy changes or mobility shocks to identify a causal effect of these on hate crimes. However, policies, mobility, and the state of the virus were changing simultaneously. Furthermore, policy choices were endogenous to culture and politics, as well as expectations of the cases and hospitalisation levels. For instance, government responses were often conditioned by changing national COVID-19 statistics.

What is more, these changes theoretically have opposite effects on hate crime. For example, as the COVID-19 situation in the UK deteriorated, there was more incentive to retaliate (or protect the local health system) against East Asians. At the same time there was higher potential risk from going out and having contact with others, which is necessary to commit an offline hate crime.

Therefore, it is not within the purview of this research to argue causality of the relationship between hate crime and the measures of hypothesised mechanisms behind it. Nonetheless, we believe that these results can still be informative for policymakers. By comparing the effects for different ethnic groups we can more plausibly argue which mechanisms led to changes in hate crimes, in order to prevent future increases over the course of the virus and future global pandemics. For mechanisms which should only impact certain ethnic groups, the other ethnicities serve either as a substitution or placebo effect.

*Mobility*

To estimate the effect of mobility on hate crime we look at the temporal effect of the pandemic on our control hate crimes: disability, homophobic, and transphobic hate crime. We look at these biases rather than racial hate crimes as the latter is impacted by the pandemic in ways beyond mobility. We argue that mechanisms such as scapegoating and retaliation, which affect racial hate crime in the context of COVID-19, do not apply to the other hate crime biases. The only mechanism all these have in common is mobility; thus, by analysing the other types of hate crime, we isolate the effects of mobility.

To do so, we look at the temporal effects from our baseline DD and ES models for the control group. We find that control hate crimes decreased significantly during the first national lockdown by an order of 25 percent (See Figure 6a). However, prior to and following the first national lockdown other hate crimes were not significantly different from the expected levels as the plots for these three periods lay on the dashed line representing no treatment effect. More precisely (See Figure 6b), we find that these hate crimes decreased during the national lockdown before steadily returning to normal levels until the second national lockdown in November, where we observe an insignificant decrease. In the figure the plots remain at the expected level (dashed line) until the 25th of March when the two subsequent plots fall far below the dashed line. However, by late May the tail of the plot returns to the dashed line, meaning that the incapacitation effect was only significant between the 25th of March and the 19th of May.

*Tweets, Cases, Government Response*

To investigate the role played by (social) media and government policy in tackling COVID-19, we begin with a descriptive analysis. We overlay a time series of (detrended and deseasonalised) East Asian hate crimes and different proxies for the mechanisms, separately for each (see Figure 2).

We make the following observations. First, the spike in COVID-19 cases in China preceded the first spike in hate crimes prior to lockdown. Second, we notice that UK COVID-19 cases appear to follow an opposite pattern to hate crime. Third, the salience of UK cases as measured by tweets shows a similar pattern as hate crime in the early period of the pandemic; note that at this point there was no government policy causing incapacitation. This suggests a protectionist or retaliation effect. Fourth, measures reflecting the lockdown and government policy seem negatively correlated with the hate crime cases. This likely captures an incapacitation effect and may have prevented a more persistent increase in spring 2020. Fifth, we observe that tweets capturing scapegoating (associating the virus with China) followed the spike in hate crime. This could be evidence of a substitution effect which became prominent as individuals were incapacitated by the virus or of a spillover effect from the origin of scapegoating tweets: the US. Finally, tweets on *#coronaracism* seem to follow a similar pattern with scapegoating tweets, lagging the movement in hate crime in the beginning of the pandemic. Later we find visual evidence that hate crimes trended upward following a shock in tweets. This could be due to changes in the reporting behaviour of victims and police.

Results of the regression analysis are captured in Figures 7a-7b. They show the correlation with recent tweets about Covid cases, tweets on scapegoating, *#coronaracism*, and policies (disaggregated and as a general governmental response index incorporating a range of policies). For ease of interpretation we standardise all variables by dividing by their standard deviation.

One challenge of estimating the effect of these proxies on racial hate crimes is that the measures will be correlated with the prognosis of the pandemic in the UK. Therefore, they will impact mobility while also changing mechanisms such as protectionism and retaliation. For this reason, we interact the mechanism measures and the treatment dummy with the baseline; this allows us to capture the correlation with the control hate crimes (mobility/incapacitation effect). Specifically, the interaction term captures other mechanisms holding all else equal. The model takes the following formula:

$$y_{at} = \alpha_m mech_{m,t} + \beta_m (R_a \times mech_{m,t}) + \psi_t + \theta_a + \varepsilon_{at} \qquad (2)$$

where $y_{at}$ is the log-transformed crime, $mech_{m,t}$ is the measure for mechanism $m$ at time $t$ and $\psi_t$ and $\theta_a$ are fixed time and group effects, respectively. The $\alpha_m$ coefficients would capture correlations between mechanisms and all crimes and would remove the effect of the mechanisms (tweets, policies, etc.) on mobility while the $\beta_m$ coefficients capture the differential effect on each of the four ethnic groups ($R_a$).

From Figure 7a we find that hate crimes against East Asians are positively correlated with tweets on cases in China and the salience of a UK lockdown but not with tweets on the UK cases. On the other hand, tweets on *#coronaracism* are negatively correlated with East Asian hate crime. This suggests that *#coronaracism* tweets signalled what the norms against hate crime are, triggering a decrease in hate crime. We also find that tweets on cases, rather than cases themselves, have a higher correlation and are more-precisely estimated.[17] Therefore only tweet counts are include as this better captures the saliency of cases in the respective countries.

Looking at the correlation between disaggregated policy measures and East Asian

[17] Full results available on request.

hate crime (Figure 7b), we find that there is a significant positive correlation between international travel controls and racial hate crime. Meanwhile, there is a negative (though insignificant) correlation between hate crime and workplace closures as well as restrictions on gatherings. This result is juxtaposed by the positive and marginally significant correlation between hate crime and restrictions on internal movement as well as stay-at-home orders. It could be that the first two policies, particularly work-from-home order, were viewed more positively and as an improvement to peoples' lives, while the latter two were viewed as restrictions to personal freedoms. Restrictions on gatherings, meanwhile, could have reduced alcohol consumption and time spent in public–for example commuting to gatherings–while intoxicated, leading to less opportunities to commit hate crime.

We find that other policy measures such as international travel restrictions and public information campaigns are also positively correlated with East Asian hate crime. The former could have been interpreted as a signal that foreigners are a threat to the public health of the UK (protectionism), while the latter may be an information shock on the severity of the virus within the UK (retaliation).

*London vs. Non-London Areas*

We also analyse the differential effect of the pandemic on racial hate crimes between the Metropolitan Police Service[18] and the other police forces. We do this because London contains much of the minority population of England and Wales, including over a third of the Chinese population (Office for National Statistics, 2012). Given the mechanisms which may drive hate crime decisions, particularly protectionism, there may be stronger reactions in London–the city with large minority population and a greater "COVID-19 threat" of these groups, as perceived by potential offenders. We expect protectionism to play a bigger role during times of lower restrictions (or more mobility), as the perceived benefit of protectionism would be greater (i.e. preventing another lockdown or the initial spread of the virus). At the same time, search costs are likely lower in London due to a larger pool of potential victims. Results can be found

[18] Greater London, excluding the City of London.

in Figures 8a and 8b. Due to violations of parallel trends for non-London areas when using log-transformed crime count the outcome variable is percent deviation from the pre-COVID average by hate crime group, however the interpretation of results does not change with the measure of crime.

We find that the positive shocks in hate crimes were relatively greater in London compared to non-London police forces in the period before the national lockdown. This aligns with our hypotheses of greater benefit to protectionism and lower search costs. There is also no evidence of an incapacitation effect in London, with hate crime staying elevated during lockdown. Meanwhile, in non-London areas lockdown period hate crimes against East Asians were not significantly higher compared to the control hate crimes. What is more, although the magnitude of the shock in London was lower at the beginning of lockdown, it increased throughout this period. This could point to differential effects of the first national lockdown between highly-populated cities (like London) and more rural police forces. The former may have experienced a stronger negative effect on individuals as cities would have provided less outdoor space for isolated individuals and may contain more people living alone and lacking a support network, such as young professionals, students, and foreigners (see for example van Leeuwen and Bourdeau-Lepage (2020)). Perhaps due to this stronger psychological effect of lockdown, in London we also observe a sustained increase in racial hate crime in the post-lockdown summer months and increases in hate crime against other ethnic groups.

Overall, we find that for all ethnic groups hate crimes increased significantly as lockdown was lifted in June, with the exception of Asians in London. However, the stronger reaction or retaliation against East Asians in London suggests that protectionism (at the beginning of the pandemic) and scapegoating (as lockdown was eased or self-incapacitation reduced) were important mechanisms. By the end of the lockdown the pandemic became truly global with the epicentre rotating between Europe, North America, and South America. This meant that various groups could be seen as posing a risk to containing the pandemic. Therefore, protectionist hate crimes against all ethnicities increased.

*Regression Discontinuity in Time (RDiT)*

As a final mechanism check we use an augmented local regression discontinuity in time (RDiT) difference-in-differences to exploit the discontinuities occurring at different points in time due to the pandemic and governmental response in the UK. This allows us to test for an effect of an information shock using daily data. The running variable is the day of observation. Our data contain 36 months (1096 days) and four COVID-19-related discontinuities. The first discontinuity occurs on the 1st of January 2020, when the WHO announced a novel coronavirus. The second discontinuity occurs on the 29th of January, following announcements for the government and airlines discouraging travel to mainland China (and the next days the first UK COVID-19 cases were made public). The third discontinuity occurs on the 24th of March, the day after the announcement of the first national lockdown in the UK. The fourth discontinuity occurs on the 8th of June, when outbound international travel was restricted (whilst within-country restrictions were being gradually eased).

To perform an augmented local regression discontinuity difference-in-differences model we first regress the log crime count on day-of-week dummies, month-of-year dummies, and a linear time trend, allowing all control variables to differ by all groups (4 ethnic groups, homophobic, transphobic, and disability hate crimes) using a (crime) fixed effects panel model. The residual of this regression is then used in a local regression model which estimates the discontinuity of the residuals at the time of the event of interest. The benefit of this method, compared to a traditional regression discontinuity model, is that we can use the full sample period (2018-2020) to remove long-term components of the data such as seasonality,[19] trends, and fixed effects. We can focus only on the days in the immediate vicinity of the cutoff to measure the discontinuity and exclude the other three potential discontinuities, preventing them from biasing the results. As before, we use a difference-in-differences approach in order to distinguish the (self-)incapacitation effect of the lockdown announcement from other mechanisms such as protectionism and retaliation.

[19]month-of-year and day-of-week

The augmented local RDiT model is given by

$$y_{at} = \alpha_0 + \alpha_1 run_t + \alpha_2 Discont_t \times run_t + \alpha_3 Discont_t + \beta Discont_t \times R_a + \varepsilon_{at}, \quad (3)$$

where $y_{at}$ is the residual log-transformed number of recorded crimes against ethnic group $a$ (East Asians, Asians, Europeans, and Black people) in day $t$. $\beta$ is our coefficient of interest capturing the difference in the discontinuity between the ethnic group and the control hate crimes due to the interaction between the discontinuity dummy and $R_a$, an indicator variable for racial hate crime. $\alpha_2$ captures a change in the time trend at the discontinuity, resulting in a kink RD design. Additionally, in order to give greater emphasis on days close to the cutoff we use triangular kernel weights.

Similar to the event study design, the empirical strategy of the regression discontinuity difference-in-differences allows us to distinguish between an incapacitation effect and other mechanisms triggered by the information shock. To illustrate this let us assume that a government announcement constitutes an information shock. Suppose it sends a signal to the population that the COVID-19 situation is worsening and, resultantly, leads to greater (self-)incapacitation. This then mechanically reduces crimes (i.e. incapacitation effect). At the same time it also increases perceived benefits of hate crimes for certain groups who see an act of hate crime as a way of protecting or retaliating against a threat to their society. In this case, incapacitation would be captured by $\alpha_3$ while other mechanisms would be captured by $\beta$.

However, the event study and regression discontinuity have a different baseline counterfactuals and therefore merit different interpretations. The event study model uses October and November 2019 as the baseline period as there was no hate crime shock in this period. On the other hand, the baseline for a regression discontinuity are the observations just preceding a threshold, in our case different policies or announcements. Therefore, the interpretation of the RDiT results is not the effect of the policies relative to expected hate crime at the time (i.e. in the absence of COVID) but rather the effect of the policies compared to the time just before the announcement or enforcement.

We begin with the regression discontinuity using the 30 day local augmented RD model (See Figure 9). We find no significant changes in hate crime at the time of the WHO announcement (beginning 1st of January 2020) (Figure 9a). This is unsurprising, because there would have been little expectation that the announcement of a novel coronavirus in Wuhan, China, would impact life in the United Kingdom.[20]

The second discontinuity occurs at the end of January 2020 when the government and British Airways discouraged travel to China and the first UK cases of COVID-19 were confirmed (Figure 9b). Here we find that control hate crimes and hate crimes against all but East Asians were unchanged by the announcements. On the other hand, hate crimes against East Asians increased by nearly 50 percent. This can be attributed to both retaliation for the first cases and protectionism due to the "risk" China and ethnically-Chinese individuals pose to the UK. The latter was reinforced by the announcements discouraging or closing travel to mainland China. Indeed, we find that the residual used in the augmented local RD was already positive in the few days between the announcements discouraging travel and the confirmation of the first UK cases. However, it increased in magnitude after the confirmation. This suggests that both mechanisms contributed to the observed significant increase at the discontinuity.

We next look at the effect of the national lockdown which occurred abruptly on the 24th of March 2020, following an announcement from Prime Minister Boris Johnson the day before (Figure 9c). We expect this to have a significant incapacitation effect on both potential victims and offenders, reducing interactions between these two groups. We find that there was a significant baseline decrease by nearly 25 percent (negative coefficient of the control group is significant at the 10 percent level). This means that at the time of the announcement of the first national lockdown hate crime decreased significantly from the period just preceding lockdown.

Finally, we consider the fourth discontinuity occurring toward the end of the national lockdown when international travel was restricted on the 8th of June 2020, whilst other freedoms were restored (Figure 9d). We find no significant discontinuities for the controls or ethnic groups.

[20] Even in the long-run as there had not been a global pandemic in Europe for the previous 100 years.

The differences between the event study and regression discontinuity results can be explained by the difference in the comparison period. In the event study approach we are evaluating changes in hate crime compared to a period pre-COVID (controlling for a variety of seasonality and fixed effects). In the regression discontinuity comparisons are made relative to the 30 days immediately before the announcement or policy change. Therefore, a negative discontinuity does not mean that hate crime is lower than expected but rather that hate crime has decreased significantly at the cut-off (and could still be higher than expected in the absence of the pandemic).

Overall the results suggest that hate crimes against East Asians rose with the salience of cases in China. We attribute it to protectionism. The increase continued as the salience of cases in the UK increased and a UK lockdown was introduced, pointing to retaliation as a significant mechanism. However, the magnitude of the shock decreased as potential victims and offenders were incapacitated by governmental policy (first national lockdown).

As the restrictions were eased, the shock persisted; this was despite baseline hate crime levels already being higher in warmer months and control hate crimes being at expected levels. Protectionism cannot explain the hate crime shock in the summer as cases in China were low and the shock spread to other ethnic groups. Given that the control hate crimes did not increase in the summer, it is unlikely that offenders were making up for their previous incapacitation. Instead, it is likely that racial animosity increased as a result of the lockdown and the social, economic, and personal distress it caused.

# 6 Conclusion

There has been much interest and media reporting on hate crime experiences of Chinese individuals in western countries. Racial hate crimes against East Asians increased in late February and early March 2020 in the UK. During the national lockdown hate crimes decreased temporarily for all ethnicities, though the effect was much smaller in magnitude for crimes against East Asians. While there is evidence that total racial hate crimes increased following the end of the national lockdown in June 2020, there is no significant increase in hate crimes against East Asians in the same period. We conclude that while animosity and scapegoating of East Asians, particularly individuals perceived to be Chinese, may have persisted throughout the pandemic, racial hate crime against this group only significantly increased during the period when COVID-19 cases were rapidly increasing in China.

Based on our empirical findings, changes in racial hate crimes during the pandemic were driven by xenophobic protectionist and retaliatory motives. Moreover, lockdowns and (self-)incapacitation of would-be victims and offenders may have prevented an even greater increase in victimisation of East Asians and, to a lesser extent, Europeans.

The results suggest that the threat of lockdown and the experience of having been in lockdown increase hate crime victimisation. Hate crimes increased first due to protectionism and concerns of a national lockdown. They then continued to increase against East Asians during lockdown or the first wave; this was due to retaliation. Therefore, on one hand lockdown may have incapacitated individuals and prevented a larger increase from occurring. On the other hand, the effect of lockdown tweets which we find suggests that fear and experience of lockdown have the opposite effect prior to and following the lockdown, respectively.

In comparison to other literature, we find that COVID-19 had greater and longer relative impact on hate crime than did terrorist events (Ivandić et al., 2019) or political shocks (Carr et al., 2020). The former find that jihadi terrorist attacks in the UK led to an increase in racial hate crime by 15-70 percent for three weeks. The latter find that the Brexit referendum led to a 20 percent increase in hate crime in the month following the vote. This has a few plausible explanations. First, the impact of COVID-19 was more personal, with personal freedoms being restricted to reduce the public health consequences of the virus. Second, the COVID-19 event was far longer lasting with more media salience. It also had a greater death toll than any terrorist attack. Moreover, while our data include two Brexit-related events in the sample–the 2019 general election and the official departure from the EU–we find by a large magnitude more significant effects of the pandemic (or BLM protests) on racial hate crime.

It is important to remark on the limitations of the research given available data and methods. First, in addition to an increase in victimisation, a rise in recorded hate crime could be due to the greater reporting probability of victims or police. In communications with us, the Metropolitan Police Service (MPS) of Greater London indicated that there was indeed outreach to the East Asian community once the hate crime contagion was first observed to encourage future reporting. Given our results and the conversation, it is most probable that the initial hate crime increase was due to changes in victimisation, while later observed higher levels of recorded hate crime may be inflated by changes in the victim behaviour. Moreover, MPS indicated that individual officers have very little impact on the recording of the crime and the designation of a hate crime would be based on the victim's perception. This rules out changes in police reporting as a mechanism.

Second, as with all research employing difference-in-differences, there should be healthy skepticism of the validity of the control groups. We have attempted to mitigate this by empirically testing the parallel trends assumption and clearly presenting these results. One property of our model is that by using other hate crime biases as controls we can isolate the incapacitation effect and capture separately changes in the psychological benefits of hate crime due to COVID. This is based on the assumption that the psychological benefit of other hate crimes was not impacted by the pandemic. While this may seem a strong assumption, we show that the control crimes only change significantly during times of incapacitation but not during the summer. In the case of substitution between the treated and control groups we would expect to find a negative effect throughout the pandemic.

The COVID-19 pandemic is a unique and once-in-a-generation event. Nonetheless, it adds to literature looking at the effect of pandemics on racial animus. Just as historic pandemics caused an increase in hate and prejudice against outside groups, we find that hate crimes increased significantly against East Asians in the UK.

Our findings bring forth important lessons for policymakers and society. First, careful attention must be paid by the media and politicians to not attach ownership (and thus blame) of a pandemic or epidemic to a specific group of people or country. This can be extrapolated to other current events such as the war in Ukraine which could lead to increases in anti-Russian hate crime in western nations given popular sentiment toward Russia. Second, when signals are sent that foreigners pose a risk to the society, there must be strong message given by politicians, police, and private citizens that xenophobia and racial hate crimes will not be tolerated. Research has made it clear that signals play an important role in the hate crime-decision process (Bursztyn et al., 2020; Carr et al., 2020). Therefore, to prevent any future hate crime shocks, consideration should be given to how the signals sent and words used are interpreted by potential offenders. Until members of the society, particularly politicians and media, appreciate their own role in creating and echoing signals, hate crimes against racial minorities will increase each time a negative global event occurs.

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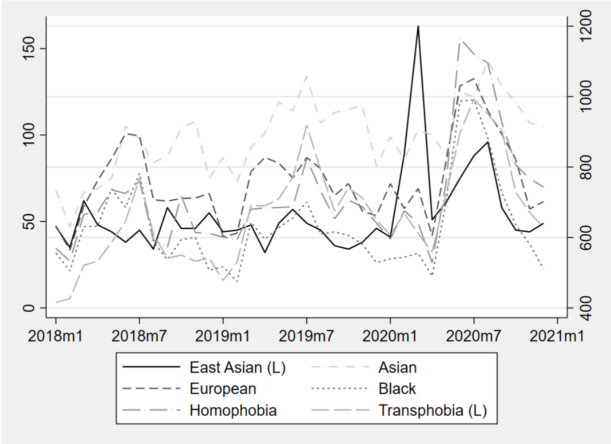
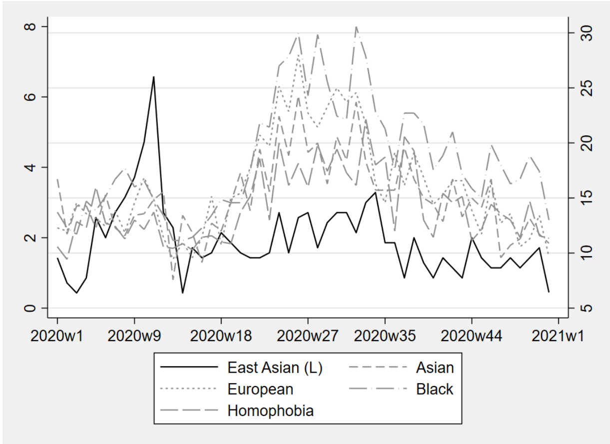
# Figures and Tables

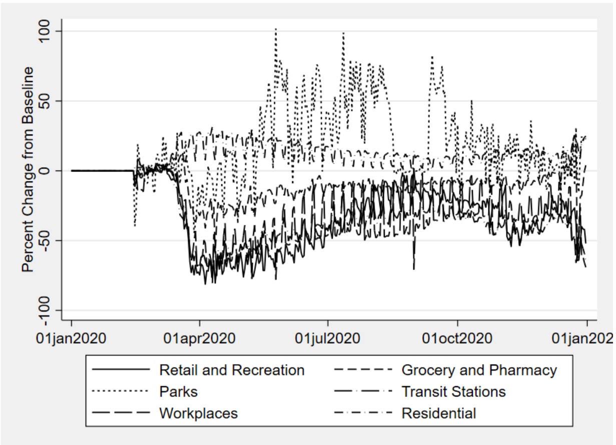
Figure 1: Time series plots hate crime, mobility and policy measures



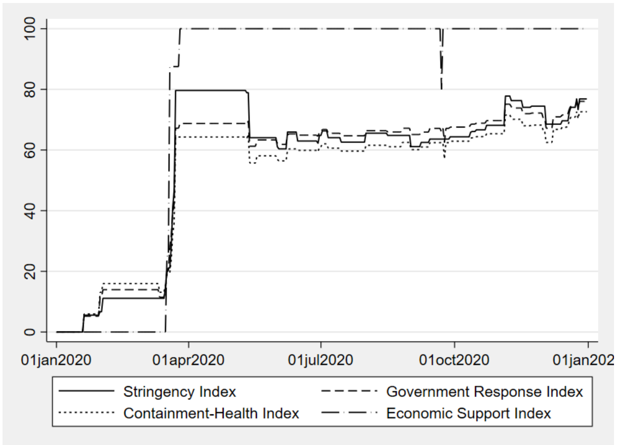

(a) Monthly hate crime counts




(b) Weekly hate crime counts



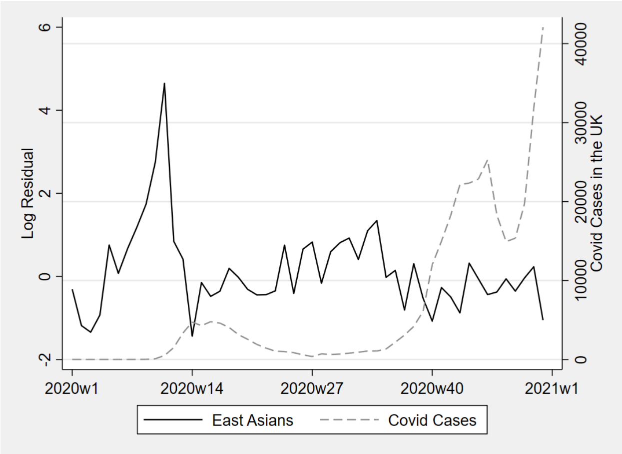

(c) Mobility measures from Google



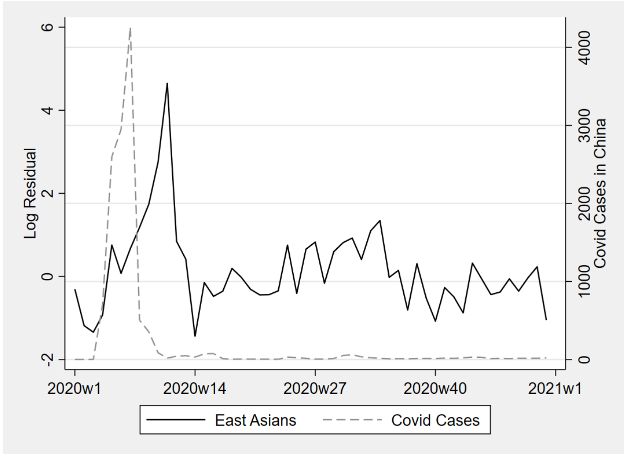

(d) Policy Indices

*Source*: Recorded crime data aggregated across the 10 reporting police force areas (PFA), collected using Freedom of Information (FOI) requests, OxCGRT systematic data set, Google mobility, World Health Organisation, and authors' own calculations.
*Notes*: Panel (a) plots (aggregated) monthly data on hate crimes against East Asians (left axis), Asians, Europeans, Black people as well as homophobic hate crimes (right axis) from 10 police forces. Panel (b) presents a plot of (aggregated) weekly data on hate crimes against East Asians (left axis), Asians, Europeans, Black people as well as homophobic hate crimes (right axis) from 10 police forces. Panel (c) contains plots of mobility data from Google, by type of destination. Panel (d) contains plots of the indices of government policy measures taken in response to COVID-19.

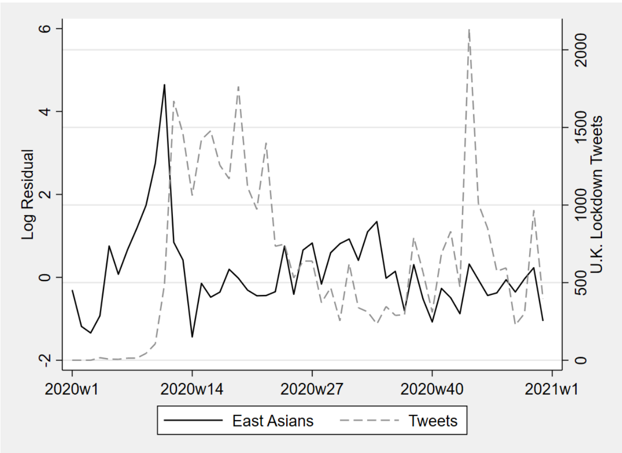
Figure 2: Descriptive plots of potential mechanisms



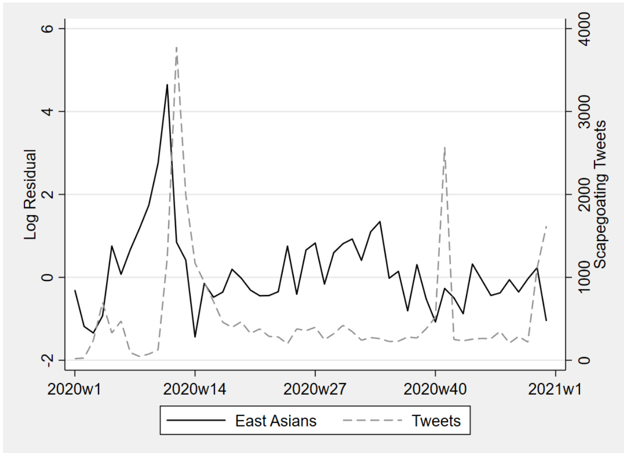

(a) UK COVID-19 cases



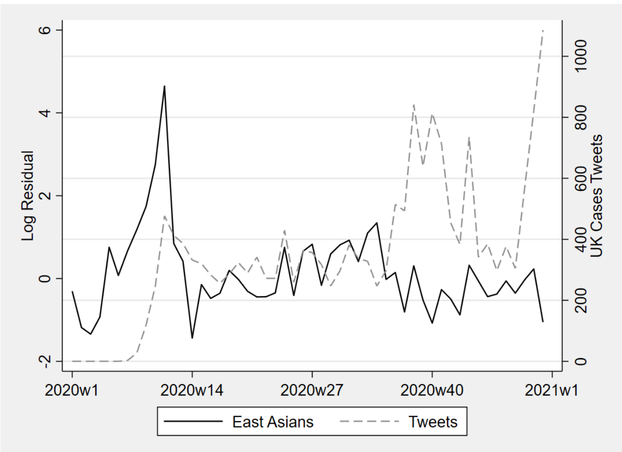

(b) China COVID-19 cases



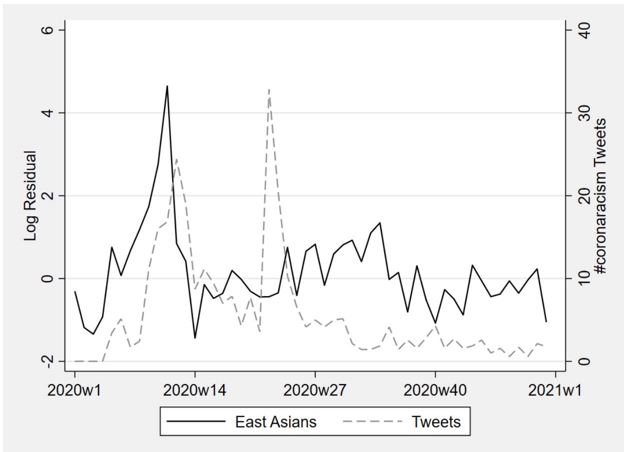

(c) Lockdown Tweets




(d) Scapegoating Tweets



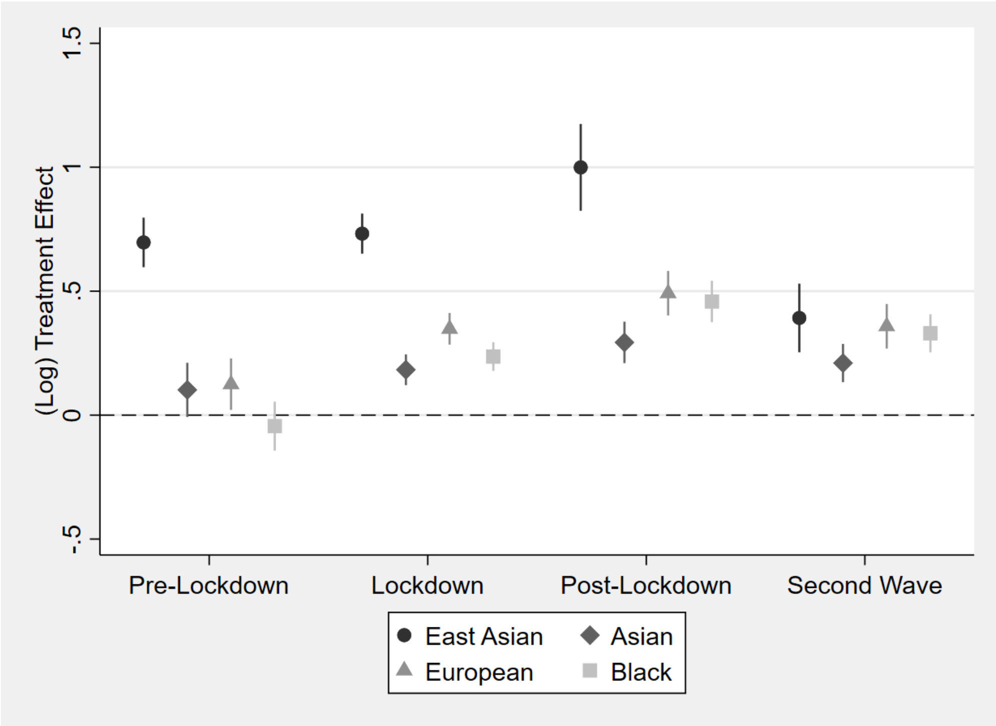

(e) UK COVID-19 Cases Tweets



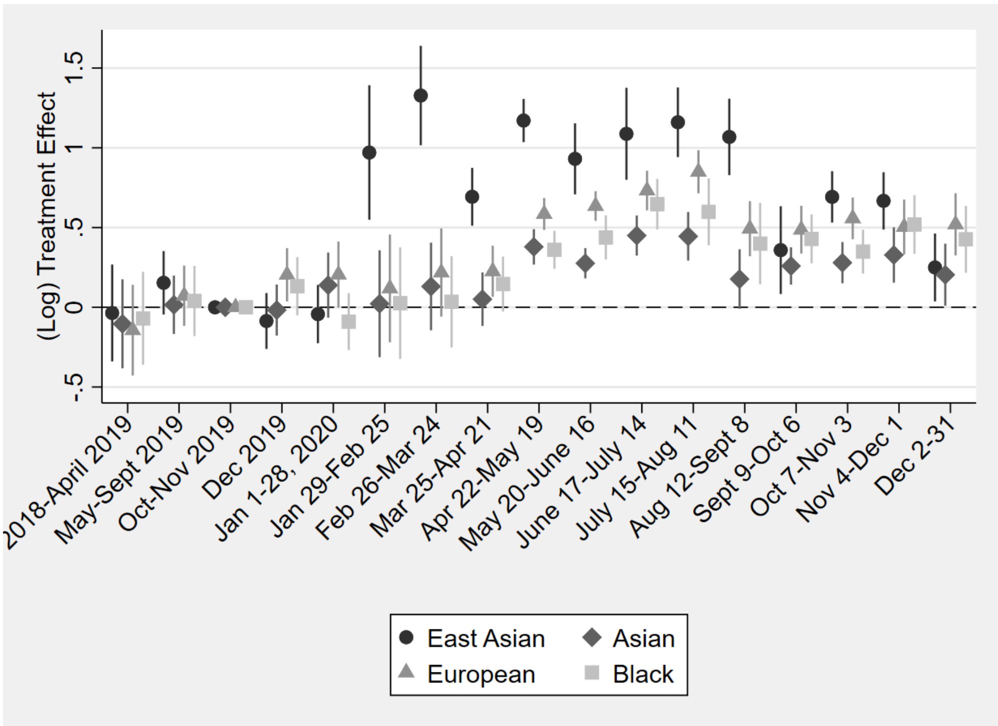

(f) #coronaracism Tweets

*Source*: Freedom of Information requests, World Health Organisation, Twitter and authors' own calculations.
*Notes*: In each panel we plot the count of hate crimes against East Asians in the reporting PFAs and one of the following: (a) the count of COVID-19 cases in the UK, (b) the count of COVID-19 cases in China, and weekly counts of tweets related to (c) lockdown, (d) scapegoating, (e) number of UK COVID-19 cases, and (f) coronaracism

Figure 3: Difference-in-difference and event study estimates of Covid-19 on racial hate crime by ethnic group



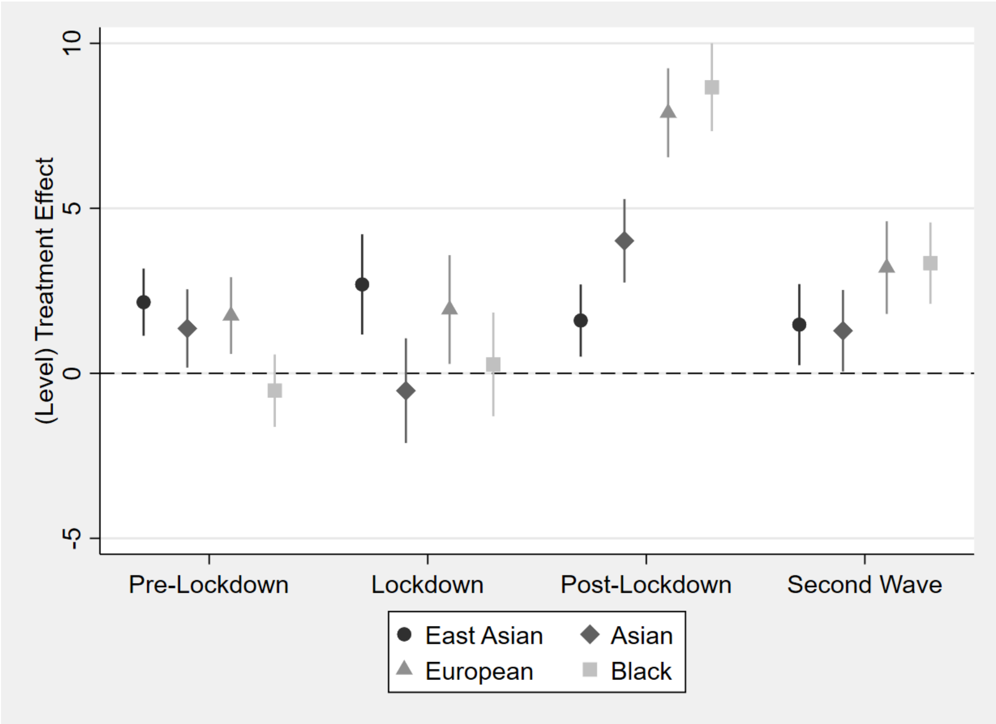

(a) Diff-in-Diff



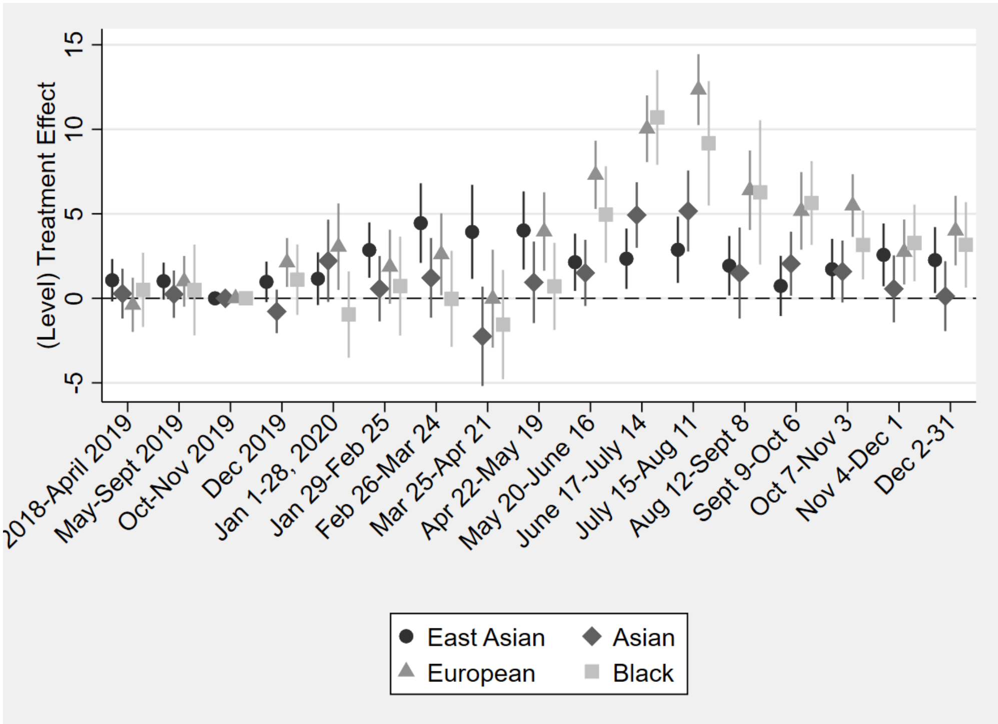

(b) Event Study

*Source*: Recorded crime data aggregated across the 10 reporting police force areas (PFA), collected using Freedom of Information (FOI) requests and authors' own calculations.
*Notes*: Outcome variable in each panel is defined as the logarithm transformed crime count. Panel (a) gives the treatment effect of each stage of the pandemic for each of the ethnic groups, separately, using other hate crime biases as a control with 2018 and 2019 acting as the pre-treatment period. Panel (b) displays the treatment effect for the ethnic groups using an event study setting. Baseline period for the event study is Oct-Nov 2019. For all panels the coefficients presented are the treatment effect for racial hate crime relative to the control group. Control group consists of homophobic, transphobic, and disability hate crime. Standard errors are clustered at crime-year. Bars represent 95% confidence interval around

Figure 4: Difference-in-difference and event study estimates of Covid-19 on racial hate crime by ethnic group: robustness estimates using crime counts




(a) DD: Crime Count

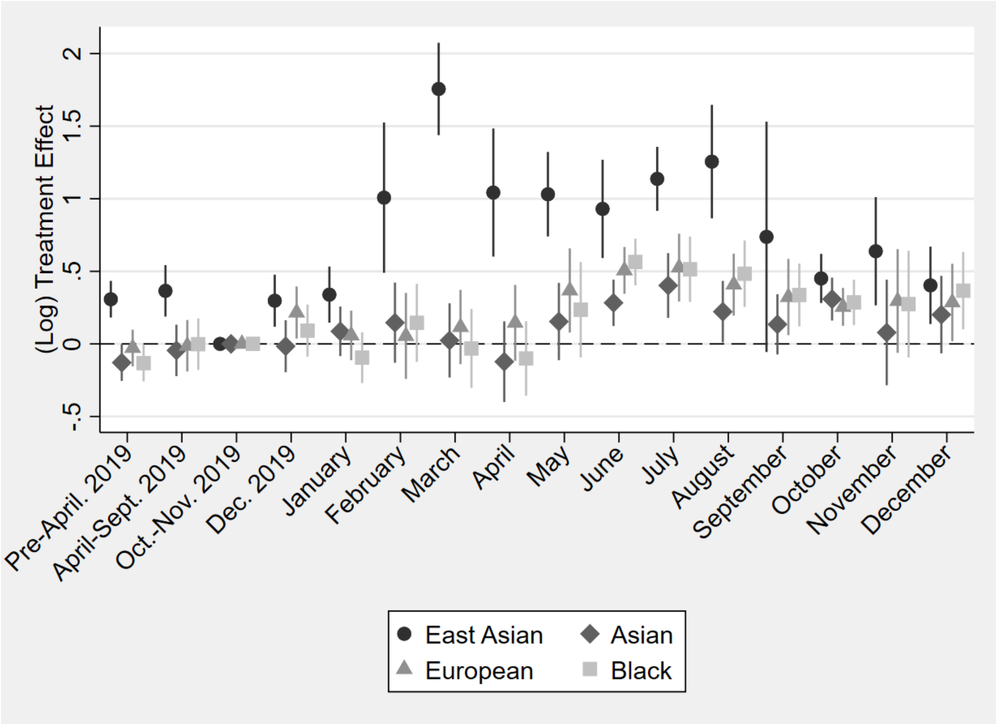



(b) ES: Crime Count

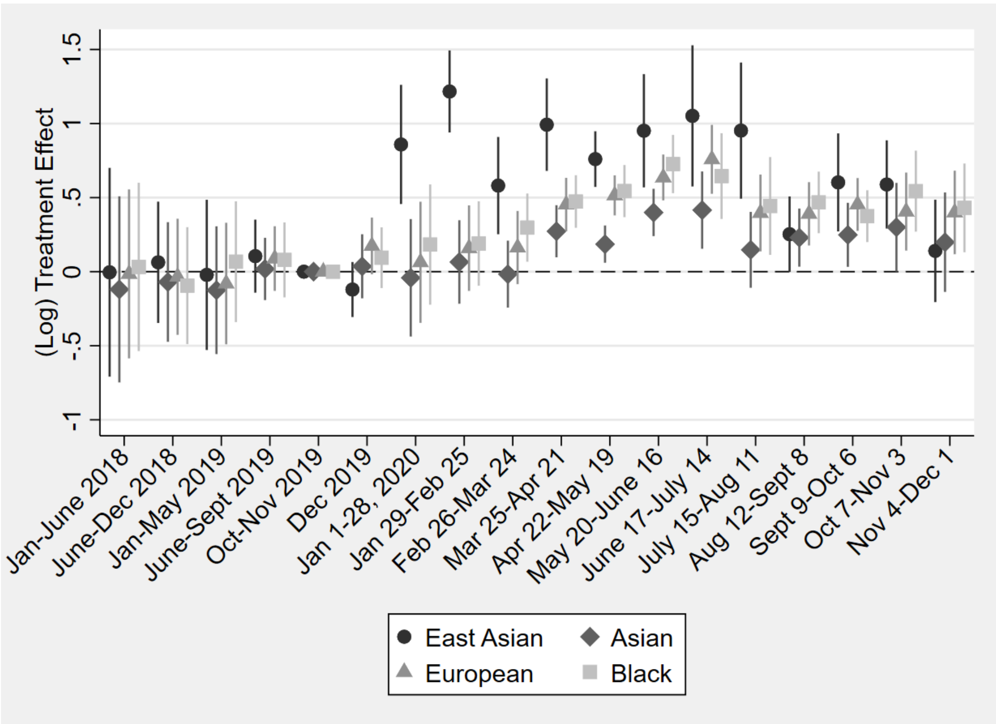
*Source*: Recorded crime data aggregated across the 10 reporting police force areas (PFA), collected using Freedom of Information (FOI) requests and authors' own calculations.

*Notes*: Outcome variable is defined as the average daily crime count by week. Panel (a) displays the treatment effects across the four COVID-19 periods, using a difference-in-differences model with 2018 and 2019 acting as the pre-treatment period. Panel (b) uses an event study design, controlling for other hate crime biases. Baseline period for the event study is Oct-Nov 2019. For all panels the coefficients presented are the treatment effect for racial hate crime relative to the control group. Control group consists of homophobic, transphobic, and disability hate crime. Standard errors are clustered at crime-year. Bars represent 95% confidence interval around each estimate.

Figure 5: Difference-in-difference and event study estimates of Covid-19 on racial hate crime by ethnic group: robustness estimates using monthly data and alternative periods



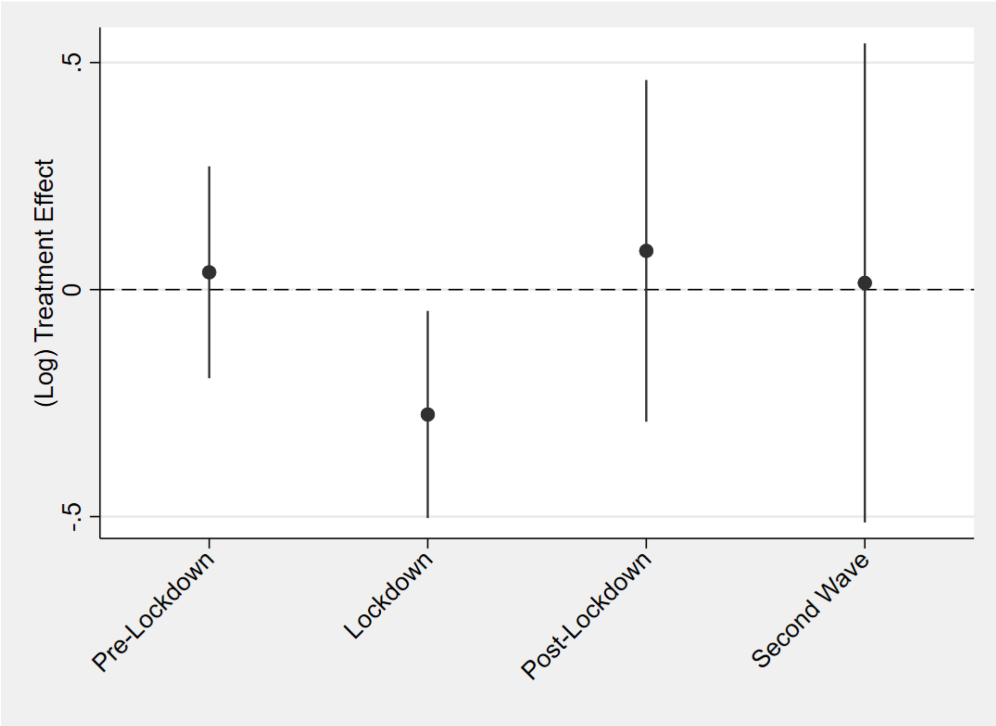

(a) ES: Monthly data



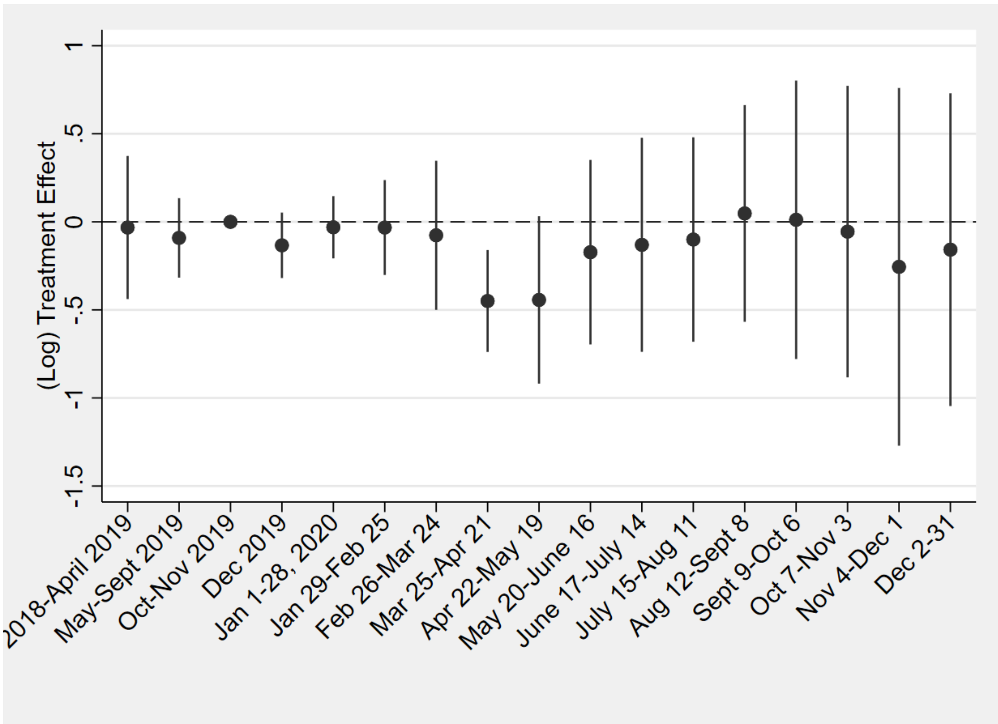

(b) ES: other pre-treatment periods

*Source*: Recorded crime data aggregated across police force areas (PFA), collected using Freedom of Information (FOI) requests and authors' own calculations.
*Notes*: Outcome variable is defined as the logarithm transformed crime count. Panels (a) and (b) use an event study design, controlling for other hate crime biases with Oct-Nov 2019 acting as the pre-treatment period. Panel (a) uses monthly data from 23 police force areas. Panel (b) uses weekly data with different division of the pre-treatment periods. Standard errors are clustered at crime-year. Bars represent 95% confidence interval around each estimate.

Figure 6: Difference-in-difference and event study estimates of Covid-19 on other hate crimes



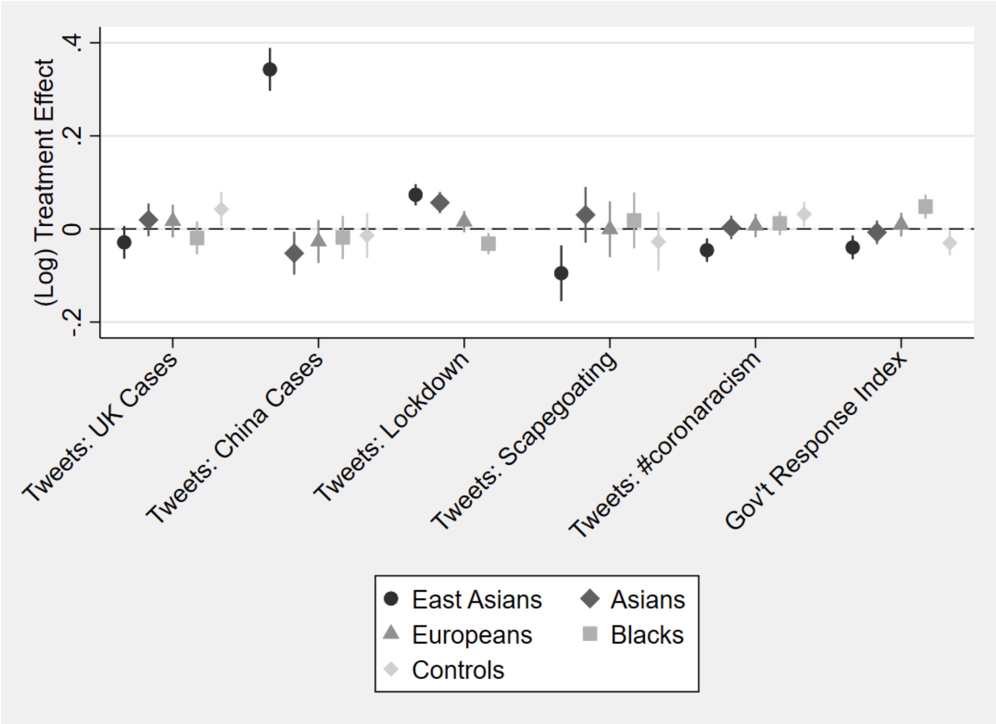

(a) DD: Weekly



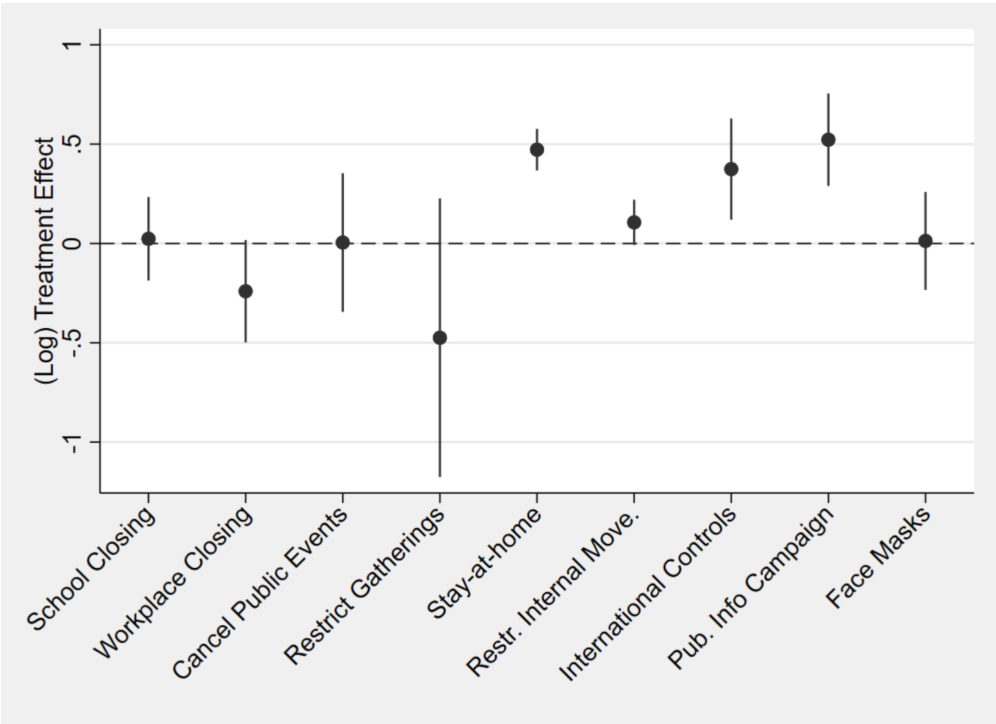

(b) ES: Weekly

*Source*: Recorded crime data aggregated across the 10 reporting police force areas (PFA), collected using Freedom of Information (FOI) requests and authors' own calculations.
*Notes*: Outcome variable in each panel is defined as the logarithm transformed crime count, based on control hate crimes. Control hate crimes consist of homophobic, transphobic, and disability hate crime. Panel (a) displays the effects across the four COVID-19 periods, using a difference model with 2018 and 2019 acting as the pre-treatment period. Panel (b) uses an event study design with a baseline period of Oct-Nov 2019. Standard errors are clustered at crime-year. Bars represent 95% confidence interval around each estimate.

Figure 7: Fixed effects estimates of various mechanisms



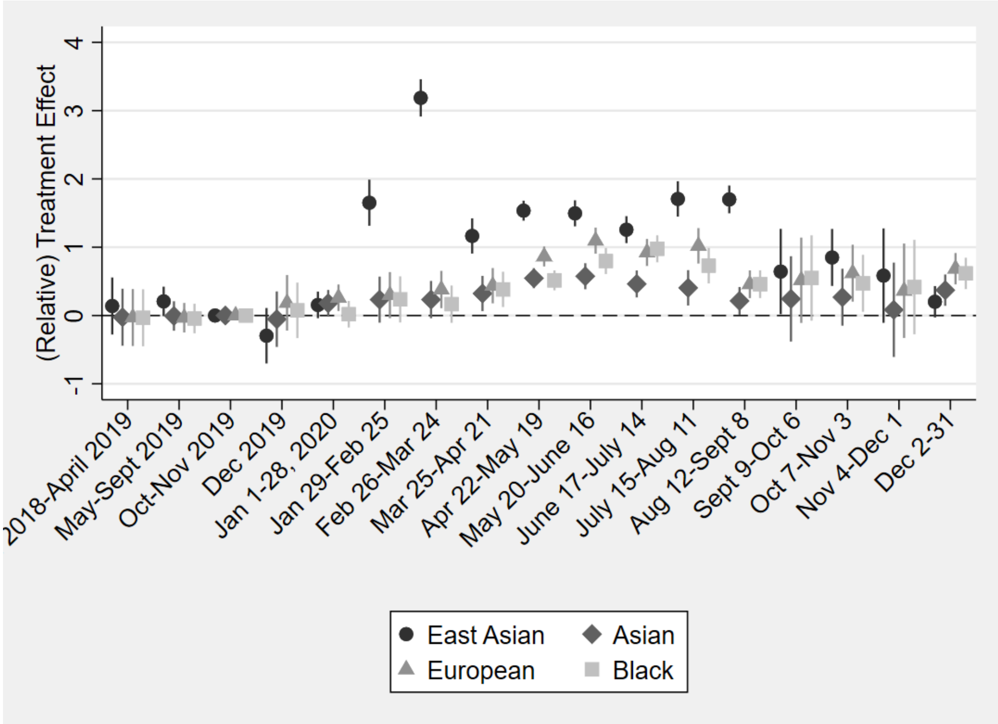

(a) Cases, Tweets, Policy Index



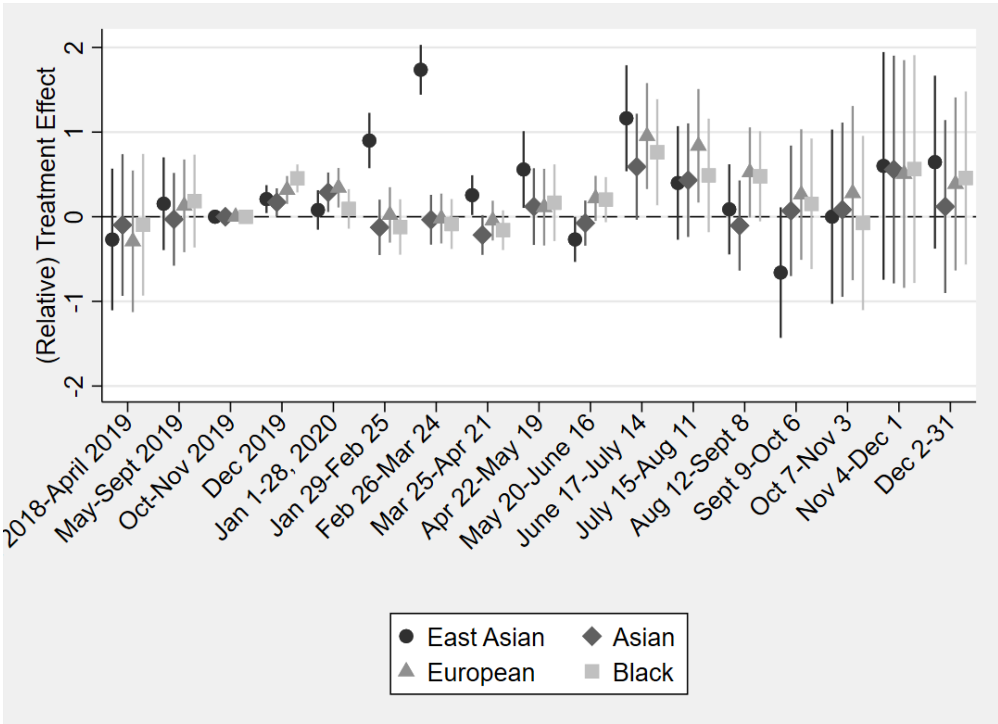

(b) Policies

*Source*: Recorded crime data aggregated across the 10 reporting police force areas (PFA), collected using Freedom of Information (FOI) requests and authors' own calculations.
*Notes*: Outcome variable in each panel is defined as the logarithm transformed crime count. Coefficients can be interpreted as the differential impact of the variables relative to the control group. Control group consists of homophobic, transphobic, and disability hate crime. Panel (a) gives the correlation between measures for cases, tweets, and policy indices and racial hate crime by ethnic group. Panel (b) gives the correlation between disaggregated policy measures and racial hate crime by ethnic group. Panels (a)-(b) use a fixed effects panel model. Standard errors are clustered at crime-year. Bars represent 95% confidence interval around each estimate.

Figure 8: Estimates of Covid-19 on racial hate crime by ethnic group: London vs. other police force areas




(a) MPS (London)

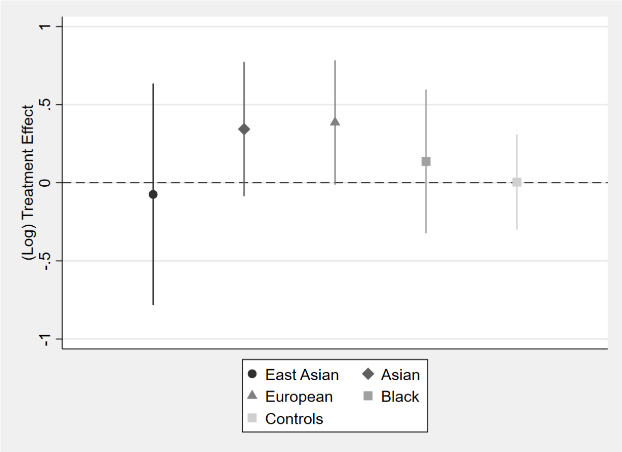



(b) Other Areas

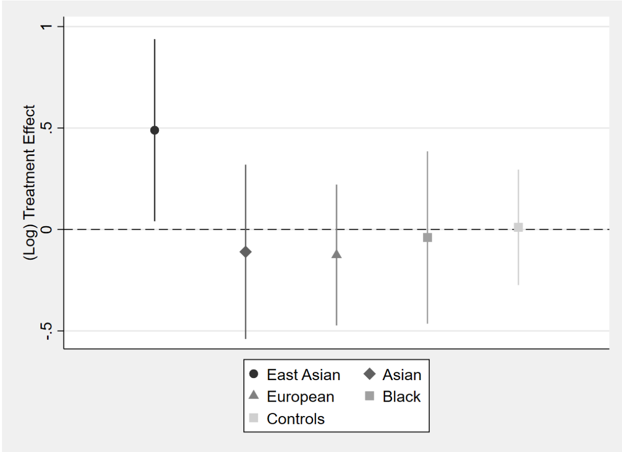
*Source*: Recorded crime data aggregated across the 10 reporting police force areas (PFA), collected using Freedom of Information (FOI) requests and authors' own calculations.

*Notes*: Outcome variable in each panel is defined as the percent deviation from the pre-COVID average. Control group consists of homophobic, transphobic, and disability hate crime. Panel (a) provides results for Metropolitan Police Service (London) only. Panel (b) provides the results for the other 9 police forces, aggregated. Standard errors are clustered at crime-year. Bars represent 95% confidence interval around each estimate.

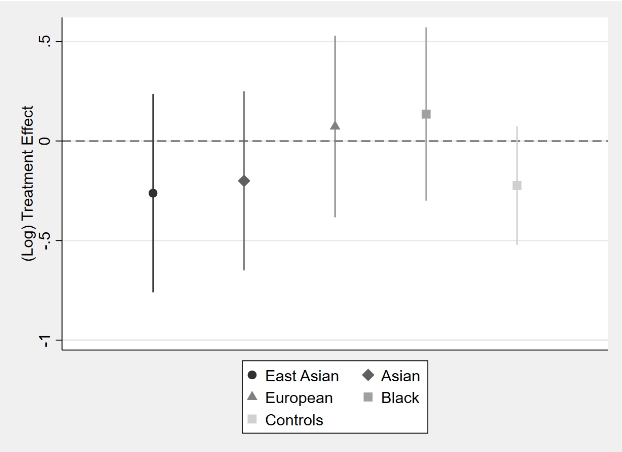
Figure 9: Regression Discontinuity in Time estimate of the impact of various policies on racial hate crime



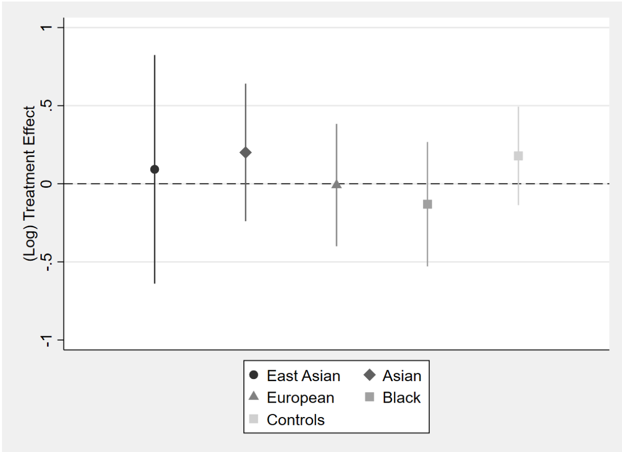

(a) WHO Announcement




(b) Travel Restrictions and First Cases




(c) National Lockdown




(d) International Travel Restrictions

*Source*: Recorded crime data aggregated across the 10 reporting police force areas (PFA), collected using Freedom of Information (FOI) requests and authors' own calculations.
*Notes*: Outcome variable in each panel is defined as the logarithm transformed crime count. Coefficients can be interpreted as the differential impact of the variables relative to the control group. Control group consists of homophobic, transphobic, and disability hate crime. Panels (a)-(d) show differential RDiT results, each at a different discontinuity: (a) WHO announcement of a novel virus, (b) imposition of travel restrictions and first COVID-19 cases in the UK, (c) first national lockdown, and (d) introduction of international travel restrictions. Bars represent 95% confidence interval around each estimate.

Table 1: FOI requests: summary statistics

| Panel A: police force response statistics | | | |
|---|---|---|---|
| | n | mean | st.dev. |
| Responded | 45 | 0.96 | 0.21 |
| Responded late | 45 | 0.27 | 0.45 |
| Refused data | 45 | 0.09 | 0.29 |
| Provided data | 45 | 0.78 | 0.42 |
| Provided right data | 45 | 0.20 | 0.40 |

| Panel B: Characteristics of police forces and areas by response | | | | | |
|---|---|---|---|---|---|
| | Did not provide data | | Provided data | | Difference t-stat |
| | mean | st.dev. | mean | st.dev. | |
| Total police force size | 7,114 | 2,683 | 7,647 | 9,461 | -0.17 |
| Police force administration size | 1,876 | 756.1 | 1,805 | 1,718 | 0.12 |
| Police force total funding (£m) | 295.40 | 108.80 | 324.20 | 497.00 | -0.17 |
| Police force funding (£m) per 100 residents | 0.004 | 0.002 | 0.025 | 0.123 | -0.50 |
| Total police force per 100 residents | 0.10 | 0.05 | 0.68 | 3.36 | -0.51 |
| Hate crimes per 100k population | 164.80 | 43.03 | 143.90 | 60.15 | 0.98 |
| % population aged 16-64 | 63.36 | 1.77 | 63.27 | 2.83 | 0.09 |
| % population non-UK born | 7.81 | 2.61 | 12.69 | 16.76 | -0.91 |
| GDHI per capita in 2018 | 19,461 | 2,932 | 24,638 | 30,290 | -0.51 |
| Unemployment rate | 4.87 | 1.47 | 4.52 | 1.20 | 0.74 |

*Source*: FOI data – own collection; GDHI (Gross Domestic Household Income) data from 2018 and unemployment rate among 16-64 year olds in 2020 from NOMIS (National Online Manpower Information System) data base; Police force characteristics data from data.police.uk; Police force population characteristics from 2011 Census.

Table 2: FOI requests: determinants of folice force response

| Dependent variable | Any Data | | | | | Right Data | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Total police size per 100 population | 0.159 | | | | 0.384 | -0.139 | | | | -0.097 |
| | (0.148) | | | | (0.601) | (0.185) | | | | (0.777) |
| Administration per 100 police force | -0.013 | | | | -0.021 | -0.005 | | | | -0.028 |
| | (0.026) | | | | (0.023) | (0.011) | | | | (0.022) |
| Funding per 100 police force | -0.048 | | | | -0.105 | -0.009 | | | | -0.024 |
| | (0.092) | | | | (0.132) | (0.015) | | | | (0.151) |
| Hate crimes per 100 000 residents (in 2020) | | -0.001 | | | -0.001 | | 0.001 | | | 0.001 |
| | | (0.001) | | | (0.001) | | (0.001) | | | (0.001) |
| Percent population aged 16 to 64 (in 2011) | | | -0.013 | | -0.018 | | | 0.013 | | 0.004 |
| | | | (0.023) | | (0.026) | | | (0.022) | | (0.023) |
| Percent non-UK born population (in 2011) | | | 0.013* | | 0.027** | | | -0.000 | | -0.000 |
| | | | (0.008) | | (0.010) | | | (0.003) | | (0.003) |
| GDHI per capita (in 2018) | | | | -0.000 | -0.000 | | | | 0.000 | -0.000 |
| | | | | (0.000) | (0.000) | | | | (0.000) | (0.000) |
| Unemployment rate (in 2020) | | | | -0.041 | -0.023 | | | | 0.011 | -0.02 |
| | | | | (0.055) | (0.035) | | | | (0.048) | (0.048) |
| n | 45 | 43 | 45 | 40 | 39 | 45 | 43 | 45 | 40 | 39 |
| pseudo R-squared | 0.066 | 0.023 | 0.042 | 0.014 | 0.15 | 0.058 | 0.024 | 0.006 | 0.005 | 0.068 |

*Source*: Data come from own collection through FOI, police.data.uk, Census 2011 and NOMIS (National Online Manpower Information System).
*Notes*: Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3: FOI requests: baseline analysis excluding London-based police forces

| Dependent Variable | Any Data | | | | | | Right Data | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Total police size per 100 population | 0.527 | | | | 0.400 | -0.547 | | | | -0.116 |
| | (0.675) | | | | (0.625) | (0.635) | | | | (0.688) |
| Administration per 100 police force | -0.039 | | | | -0.022 | -0.011 | | | | -0.009 |
| | (0.028) | | | | (0.024) | (0.019) | | | | (0.021) |
| Funding per 100 police force | -0.150 | | | | -0.110 | -0.027 | | | | -0.015 |
| | (0.138) | | | | (0.139) | (0.084) | | | | (0.141) |
| Hate crimes per 100 000 residents (in 2020) | | -0.001 | | | -0.001 | | 0.001 | | | 0.001 |
| | | (0.001) | | | (0.001) | | (0.001) | | | (0.001) |
| Percent population aged 16 to 64 (in 2011) | | | -0.016 | | -0.019 | | | -0.001 | | -0.009 |
| | | | (0.026) | | (0.029) | | | (0.018) | | (0.020) |
| Percent non-UK born population (in 2011) | | | 0.010 | | 0.028** | | | -0.002 | | -0.003 |
| | | | (0.013) | | (0.012) | | | (0.004) | | (0.006) |
| GDHI per capita (in 2018) | | | | -0.000 | -0.000 | | | | -0.000 | -0.000 |
| | | | | (0.000) | (0.000) | | | | (0.000) | (0.000) |
| Unemployment rate (in 2020) | | | | -0.060 | -0.025 | | | | -0.042 | -0.047 |
| | | | | (0.058) | (0.039) | | | | (0.048) | (0.048) |
| n | 43 | 42 | 43 | 39 | 38 | 43 | 42 | 43 | 39 | 38 |
| pseudo R-squared | 0.050 | 0.032 | 0.025 | 0.027 | 0.14 | 0.017 | 0.007 | 0.004 | 0.033 | 0.055 |

*Source*: Data come from own collection through FOI, police.data.uk, Census 2011 and NOMIS (National Online Manpower Information System).
*Notes*: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 4: FOI requests: determinants of non-response and refusal to provide data

| Dependent variable | Late Response or No Response | | | | Refused to provide data | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Total police size per 100 population | -0.000 | | | | 0.032 | | | |
| | (0.001) | | | | (2.858) | | | |
| Administration per 100 police force | 0.000 | | | | -0.727 | | | |
| | (0.002) | | | | (10.881) | | | |
| Funding per 100 police force | 0.001 | | | | 3.061 | | | |
| | (0.010) | | | | (9.037) | | | |
| Hate crimes per 100 000 residents (in 2020) | | -0.000 | | | | -0.000 | | |
| | | (0.001) | | | | (0.001) | | |
| Percent population aged 16 to 64 (in 2011) | | | -0.018 | | | | 0.000 | |
| | | | (0.027) | | | | (0.010) | |
| Percent non-UK born population (in 2011) | | | 0.003 | | | | -0.004 | |
| | | | (0.004) | | | | (0.006) | |
| GDHI per capita (in 2018) | | | | 0.000 | | | | 0.000 |
| | | | | (0.000) | | | | (0.000) |
| Unemployment rate (in 2020) | | | | -0.017 | | | | -0.028 |
| | | | | (0.066) | | | | (0.027) |
| n | 45 | 43 | 45 | 40 | 45 | 43 | 45 | 40 |
| pseudo R-squared | 0.100 | 0.002 | 0.016 | 0.002 | 0.010 | 0.001 | 0.020 | 0.027 |

*Source*: Data come from own collection through FOI, police.data.uk, Census 2011 and NOMIS (National Online Manpower Information System).
*Notes*: Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5: Description of Twitter keywords

| Topic | Keywords used in search | Mechanism(s) |
|---|---|---|
| Sinophobia | “chink” or “yellowman” or “chinky” or “chinazi” | Substitution |
| Scapegoating | “chinavirus” or “kungflu” or “yellow fever” or “Wuhan virus” or “chinaliedpeopledied” or “fuckchina” or “CCPvirus” | Retaliation |
| UK Covid | “covid” or “coronavirus” + “cases” or “infections” + locations | Protectionism/Retaliation |
| China Covid | “covid” or “coronavirus” + “cases” or “infections” + “China” | Protectionism/Retaliation |
| UK Lockdown | “covid” or “coronavirus” + locations | Retaliation |
| Corona Racism | “#coronaracism” | Reporting |
| Employment | “unemployment” + locations | Retaliation |
| Recession | “recession” + locations | Retaliation |

*Notes*: Locations include “United Kingdom”, “UK”, “England”, “Wales”, “London”, “Manchester”, “Liverpool”, or “British”. Data is scrapped using the Python package from https://github.com/JustAnotherArchivist/snscrape. This code allows researchers to collect a dataset of tweets containing specified keywords (including combinations) during a chosen period. Search by geolocation or user location is not possible with Twitter data.

Table 6: Summary Statistics

| Variable | Obs | Mean | Std. dev. | Min | Max |
|---|---|---|---|---|---|
| *Hate Crime* | | | | | |
| Racial: East Asians | 156 | 1.42 | 0.81 | 0.43 | 6.57 |
| Racial: Asians | 156 | 14.6 | 2.78 | 7.57 | 24.3 |
| Racial: Europeans | 156 | 14.4 | 3.50 | 7.38 | 28.0 |
| Racial: Black people | 156 | 16.3 | 3.76 | 10.0 | 30.6 |
| Homophobic | 156 | 13.0 | 3.15 | 6.14 | 22.1 |
| Transphobic | 156 | 1.72 | 0.672 | 0.286 | 4.14 |
| Disability | 156 | 4.79 | 1.24 | 2.29 | 8.57 |
| *New COVID-19 Cases* | | | | | |
| UK | 156 | 2205 | 6365 | 0 | 42083 |
| China | 156 | 79 | 465 | 0 | 4259 |
| East Asia | 156 | 344 | 825 | 0 | 4616 |
| Asia | 156 | 11441 | 24388 | 0 | 97429 |
| Europe | 156 | 21494 | 58359 | 0 | 275265 |
| Africa | 156 | 2500 | 5158 | 0 | 23858 |
| *Tweets* | | | | | |
| COVID-19 in UK | 156 | 117 | 214 | 0 | 1086 |
| COVID-19 in China | 156 | 211 | 425 | 0 | 2136 |
| Scapegoating | 156 | 186 | 454 | 1.43 | 3777 |
| UK unemployment | 156 | 82 | 41 | 32 | 228 |
| UK recession | 156 | 86 | 151 | 14 | 1814 |
| #coronaracism | 156 | 1.90 | 4.72 | 0 | 33 |
| Sinophobia | 156 | 21.6 | 33.8 | 2.57 | 220 |
| *Policy Measures* | | | | | |
| Gov't Response Index | 156 | 18.3 | 29.5 | 0 | 75.6 |
| School Closing | 156 | 0.497 | 0.928 | 0 | 3 |
| Workplace Closing | 156 | 0.606 | 1.038 | 0 | 3 |
| Cancel Public Events | 156 | 0.524 | 0.879 | 0 | 2 |
| Restrict Gatherings | 156 | 1.033 | 1.750 | 0 | 4 |
| Stay-at-home Order | 156 | 0.270 | 0.584 | 0 | 2 |
| Restrict Movement | 156 | 0.400 | 0.746 | 0 | 2 |
| Restrict Int'l Travel | 156 | 0.188 | 0.390 | 0 | 1 |
| Income Support | 156 | 0.523 | 0.880 | 0 | 2 |
| Public Info Campaign | 156 | 0.620 | 0.920 | 0 | 2 |
| Face Coverings | 156 | 0.481 | 0.996 | 0 | 3 |
| *Mobility* | | | | | |
| Retail and Recreation | 156 | -11.0 | 21.0 | -74.6 | 2.5 |
| Grocery and Pharmacy | 156 | -4.16 | 8.58 | -36.1 | 11.7 |
| Parks | 156 | 4.76 | 15.7 | -26.2 | 71.8 |
| Transit Stations | 156 | -11.4 | 19.75 | -66.3 | 0 |
| Workplaces | 156 | -10.4 | 18.3 | -65.7 | 0.729 |
| Residential | 156 | 3.62 | 6.51 | 0 | 22.7 |
| *Weather* | | | | | |
| Rain | 156 | 2.83 | 2.30 | 0.040 | 11.7 |
| Mean temperature | 156 | 106.1 | 48.2 | -10.7 | 212.4 |
| Min. temperature | 156 | 66.1 | 41.2 | -37.7 | 156.1 |
| Max. temperature | 156 | 146.1 | 57.1 | 16.3 | 273.6 |

*Source*: Freedom of Information requests, OxCGRT systematic dataset, Google mobility, World Health Organisation, and authors' own calculations.
*Notes*: All variables are measured by the daily average count for that week.

Table 7: Difference-in-difference estimates of Covid-19 on racial hate crimes

| | (1) East Asian | (2) Asian | (3) European | (4) Black |
|---|---|---|---|---|
| Treatment Effect | 0.919*** | 0.193* | 0.281** | 0.111 |
| | [0.001] | [0.076] | [0.030] | [0.224] |
| Parallel Trends F-stat | 0.00 | 0.39 | 0.63 | 0.01 |
| | [0.971] | [0.578] | [0.485] | [0.929] |
| N | 624 | 624 | 624 | 624 |
| Groups | 4 | 4 | 4 | 4 |

*Source*: Recorded crime data aggregated across the 10 reporting police force areas (PFA), collected using Freedom of Information (FOI) requests and authors' own calculations.
*Notes*: Outcome variable in each panel is defined as the logarithm transformed crime count. Baseline period is 2018 and 2019. Coefficients are the treatment effect for racial hate crime against the given ethnic group (East Asian, Asian, European or Black) relative to the control group. Control group consists of homophobic, transphobic, and disability hate crime. p-values in squared brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.