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Article

The Effect of the Brexit Vote on the Variation in Race and Religious Hate Crimes in England, Wales, Scotland and Northern Ireland

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This paper examines possible mechanisms behind the spike in racially or religiously-aggravated (RR) offences after the Brexit vote. It adds to the current literature in five significant ways: (1) it provides the first Brexit-related RR hate crime comparison between England and Wales, Scotland and Northern Ireland; (2) it reports on results from a national-level panel model that adds to the debate in the literature on whether pro-leave or pro-remain areas saw greater increases hate crimes; (3) it assesses the role of demographic characterises on the variation in hate crime; (4) it compares the effect of the vote with other 'trigger events'; and (5) it uses social media data to control for variation in hate crime victim and witness reporting.

KEY WORDS: hate crime, Brexit, race, religion, European Union

INTRODUCTION

This paper examines the relationship between the 2016 referendum vote on the future of the United Kingdom in the European Union (the Brexit vote) and the rise of Race or Religious (RR) hate crimes at the national and regional (Police Force Area) level in England and Wales. The Brexit vote was linked by the Home Office to the largest increase in police recorded hate crime since records began. The UK Government's Hate Crime Action Plan (Home Office 2016) introduced after the vote, stressed the need to tackle Brexit-related hate crime by bringing together policymakers with academics to improve the analysis of the patterns and drivers of hate and how the latter can be addressed by police, courts and the third sector. However, significant questions remain over the short- and long-term causes of this rise in hate crime, what the implications are for the governance of this problem and the wider linked issues of segregation, community cohesion and fostering new shared principles of citizenship post-Brexit.

The reported stark increase in prevalence and change in the nature of hate crime after the Brexit vote requires the development of new governance models. The Home Office Hate Crime Action Plan (2016) included initiatives to increase reporting, secure places of worship and to develop our understanding of the 'drivers of hate'. These responses are clear indicators that governmental authorities across the United Kingdom recognise the changing nature of hate crime. Despite these efforts, fresh calls for government to re-examine how it deals with hate crime have been made in relation to improving reporting, victim services and community cohesion post-Brexit (Chakraborti 2017). The effectiveness of pre-Brexit hate crime governance models is being questioned, including traditional criminal justice interventions of expanding police recruitment, improving policing practice and harsher sentencing. However, these clearly have an important role to play, not least in symbolic terms. The design of new governance interventions requires robust analysis of all available data (not just police and survey data in isolation), linked in a way that allows longitudinal analysis by geography. Only with such data and methodological innovations can policymakers be made aware of the most significant driving factors of Brexit-related hate crime, and formulate new ways of tackling them. For example, isolated data sources cannot tell us if the rise in hate crime was due to increased reporting by victims and witnesses, better recording by police, an actual increase in perpetration related to the vote and leave campaigns, or a combination of multiple factors.

This paper reports on the use of innovative methods to explain variation in Brexit-related hate crime. A novel linked dataset consisting administrative, survey, traditional and new social media data was used to conduct temporal and spatial analyses to identify and explain possible mechanisms behind the spike (rapid increase and decrease) in RR offences in England and Wales after the Brexit vote. This paper adds to the current literature in five significant ways: (1) it provides the first Brexit-related RR hate crime comparison between England and Wales, Scotland and Northern Ireland; (2) it reports on results from a national-level panel model that adds to the current debate in the literature on whether pro-leave or pro-remain areas saw greater increases in RR hate crimes; (3) it includes the Index of Multiple Deprivation to assess the role of demographic characterises on the variation in Brexit related-RR hate crime; (4) it compares the effect of the Brexit vote with other 'trigger events' after the vote on the variation in RR hate crimes; and (5) it makes use of novel social media data to develop a proxy indicator to control for variation in hate crime victim and witness reporting.

CONTEXT

Legislation, and in turn the criminal justice system, recognise a limited number of identity characteristics in relation to hate crime. The Crown Prosecution Service (CPS) and National Police Chiefs' Council joint definition is perspective based: *'Any criminal offence which is perceived by the victim or any other person, to be motivated by hostility or prejudice, based on a person's disability or perceived disability; race or perceived race; or religion or perceived religion; or sexual orientation or perceived sexual orientation or a person who is transgender or perceived to be transgender.'* (CPS 2021). An array of acts covers the five protected characteristics, associated hostility and prejudice-based crimes. Racially and religiously aggravated crimes are specified under the Crime and Disorder Act 1998, with incitement laws introduced under the Public Order Act 1986.[1]

Hate crime scholarship is most developed in the context of race (Phillips and Bowling 2017) and religious hostility (Chakraborti and Zempi 2012; Taras 2012). The definitions of race and

1 An increase in sentences for aggravation related to disability, sexual orientation and transgender identity was introduced under the Criminal Justice Act 2003.

religious hate crimes remain contested among criminologists (Chakraborti and Garland 2009; Williams and Tregidga 2014). The empirical, policy and operational usefulness of concepts of hate crime that promote hierarchical notions of group dominance and subordination have been questioned (Garland 2012; Garland and Hodkinson 2014). In response, clarion calls have been made for hate crime scholarship to be organised around the notions of 'difference' and 'vulnerability', as opposed 'identity' and 'group membership' (Chakraborti and Garland 2012). Such an approach would refocus the analysis from an often empirically and theoretically problematic identity and group categories to the understanding of categories of risk, that are more sensitive to the inclusion of those victims on the margins and at the boundaries (Garland 2012).

Despite these calls for a more inclusive and reflexive approach to measuring hate crime, criminologists wanting to conduct national level analyses are limited by the lack of alternative data sources to state sponsored recording efforts. Looking at the period of interest, police recorded data showed there were 80,393 hate crime offences in 2016/17, compared with 62,518 the year before, a 29 per cent increase (Home Office 2017). Estimates from the Crime Survey for England and Wales, arguably a more reliable measure of hate crime victimisation due to its insensitivity to changes in police training/recording practices, show that hate crime, along with general crime, decreased between 2008 and 2015, before turning to show an increase in race and religious hate crime from 112,000 to 117,000 crimes (5 per cent) between 13/15 and 15/17 (ONS 2018). This is a significant turning point, as it reflects the first rise in hate crime recorded by the survey in 10 years. While the increase in police recorded crime can be partially attributed to greater perpetration, the Home Office at the time stated the rise around the EU Referendum was largely due to increased recording and reporting (Home Office 2017). This narrative was rapidly perpetuated by the right-leaning press, with the Daily Mail publishing the 'The Great British Hate Crime Myth' and the Spectator 'The Truth Behind the Brexit Hate Crime "Spike"'.

CONCEPTUAL FRAMEWORK

This paper builds on the growing body of work on the temporal dimension of crime and the role of 'trigger' events in shifting social norms. Notably, in the field of hate crime, King and Sutton (2014) found an association between terrorist acts and a rise in hate crimes in the United States, showing that 58 per cent of the 481 anti-Muslim hate crimes recorded in 2001 occurred in the two weeks following 9/11. Hanes and Machin (2014) found similar in the United Kingdom, with a rapid rise in anti-Muslim hate crimes in the immediate aftermath of 7/7. Both studies found a sharp decrease in hate crimes following an equally sharp increase following these events, indicating a potential causal association, although neither study's design allowed for a formal test. These findings were further replicated in research by Edwards and Rushin (2019) and Müller and Schwarz (2020) who identified sharp increases in anti-Muslim hate crimes in the United States during the campaign and the subsequent election of Donald Trump. Expanding this body of research to online hate crimes, Williams and Burnap (2016) found an association between the Woolwich terror attack and a rapid increase, and subsequent sharp decline, in online anti-Muslim hate speech.

Several working-papers authored by economists have considered the Brexit vote as a likely trigger for a shift in social norms that resulted in an increase in hate crimes. Carr *et al.*'s (2020) analysis at the Community Safety Partnership and Police Force Area levels in England and Wales found a 35–39 per cent increase in the race and religious hate crime rate (per 100,000 inhabitants) in July 2016, with the greatest increases in majority leave voting areas, confirming an earlier study by Devine (2018). Conversely, Albornoz *et al.* (2020) found at the Community Safety Partnership level that the post-vote increase in hate crime was more pronounced in majority remain voting areas. Focusing on London and Manchester, Schilter (2020) estimated an increase of 21 per cent in the race and religious hate crime rate (per million of borough population) in July 2016, finding evidence that areas with a higher rate of hate crime had a larger proportion of recent immigrants and people with formal qualifications. These authors conclude hate crimes cluster in time and tend to increase, sometimes dramatically, in the aftermath of antecedent galvanising events that act as 'triggers' or 'releasers' of prejudice. This logic is premised on the idea that hate crimes are, in part, communicative acts, often provoked by events that incite a desire for retribution in the targeted group, towards a group that share similar characteristics to a wrongdoing outgroup (e.g. terrorist perpetrators, criminals on trial and immigrants).

The Justification–Suppression Model of the Expression and Experience of Prejudice (JSM) (Crandal and Eshleman 2003) and Integrated Threat Theory (ITT) (Stephan and Stephan 2000) offer possible mechanisms for the shift in social norms brought about by trigger events. JSM states that the expression of prejudice is marked by a psycho-social conflict between a desire to express an attitude, while simultaneously wishing to maintain a positive self-identity that conflicts with prejudice. Suppression forces include social norms, personal standards, beliefs and values, and when these forces dominate, socially and psychologically, they reduce the public expression of prejudice and its experience e.g. through hate speech and crimes. Conversely, justification processes facilitate the expression of genuine prejudice. Situational factors, such as individual and community experiences, exposure to ideological forces and interactions with others who hold particular attitudes, can liberate prejudice, leading to its public expression. Therefore, when justification forces are in abundance and there is a relative absence of suppression forces, then the correspondence between holding a prejudiced attitude and its public expression is high.

A well-documented justification force is perceived threat from an outgroup towards an ingroup (Crandal and Eshleman 2003). When an outgroup is perceived as threatening, and this threat can be communicated, it can serve as a powerful justification for prejudice. ITT delineates two kinds of threat: *realistic* (e.g. economic) and *symbolic* (e.g. cultural). Threats operate at the group and individual level: members of the ingroup can perceive that they as individuals, their group or both are being threatened by some outgroup. When a threat is perceived as emanating from a member of the outgroup, members of the ingroup can experience fear and frustration as a result, which in turn reduce the suppression and increase the justification of the expression prejudice. Threats do not have to be observable to have this effect. Simply the *perception* of threat, including the manufacturing of a sense of threat by public figures, can result in an increase in tensions between groups. It is possible that the Brexit vote, and the leave campaigns in the run up, increased the perception of threats, that in turn increased the justification for the expression of prejudice in the form of hate crimes.

HYPOTHESES

H1: The Brexit vote is statistically associated with an increase in RR hate crimes.

Previous research has indicated a statistical association exists between events and hate crimes (Hanes and Machin 2014; King and Sutton 2014; Williams and Burnap 2016; Edwards and Rushin 2019; Müller and Schwarz 2020, 2021). 'Trigger events', such as terror attacks, court cases and political votes, can act as vectors for the communication of novel information about group processes that can galvanise existing negative prejudices towards an outgroup. A host of working-papers have found an association between the Brexit vote and a rise in RR hate crimes in England and Wales (Devine 2018; Albornoz *et al.* 2020; Carr *et al.* 2020; Schilter (2020).

Building on this work, the first supposition in the present study is that the vote outcome represented a 'shock' that reduced the suppression and increased the justification for the expression of prejudice resulting in an increase in race and religious hate crimes targeting members of the 'outgroup' in an attempt to protect economic (e.g. threats to jobs, housing, NHS waiting times) and symbolic (e.g. threats to way of life) resources of the 'ingroup'.

H2: The Brexit vote effect on RR hate crime will be equal to, or greater, than the effect of other 'trigger events'.

Carr *et al.* (2020) considered the possibility that other events may have also caused a spike in hate crime, given their role in promoting a shift in social norms. They compared the effect of the Brexit vote to events that occurred *before* it, such as terrorist attacks in the United Kingdom and elsewhere, as well as elections, and found the magnitude of the Brexit vote effect on RR hate crime was roughly equal to the effect (in terms of magnitude and duration) of the Lee Rigby murder in 2013. This finding suggests that the effect of a terrorist attack and the public information shock of the referendum outcome are comparable. Our second supposition is the Brexit vote outcome effect on RR hate crime will be equal to, or greater, than 'trigger events' that took place *after* (between June 2016 and September 2017).

H3: There is geographical heterogeneity in the effect of the Brexit vote on an increase in RR hate crimes, which is a function of vote share and demographic differences at the regional (England and Wales compared to Scotland and Northern Ireland) and PFA level.

Prior studies agree that vote share is a key reason for geographical heterogeneity in the Brexit vote effect on hate crime, but disagree on the direction of influence. Carr *et al.* (2020) found that the post-vote increase in RR hate crime was greater in pro-leave areas. Conversely, Albornoz *et al.* (2020) at the national level, and Schilter (2020) at the city level (Manchester and London), found that the increase was more pronounced in pro-remain areas. Schilter also found that areas with a higher number of recent immigrants and people with formal qualifications were also associated with a higher RR hate offence rate post-vote. The third supposition in this study is that places with the largest increase in RR hate crime also have vote (pro-leave) and demographic (more deprived) characteristics that mean certain members of the 'in-group' are more susceptible to the associated divisive threat narratives pre-vote and the temporal shock of the result post-vote, leading to an increased justification for hate crimes.

H4: Variation by PFA in police social media communications encouraging hate crime reporting is not associated with recording rates of RR hate crimes in the wake of the Brexit vote.

The Home Office and many right-leaning press outlets were quick to explain the rapid rise in RR hate crimes following the Brexit vote as primarily a function of increased victim and witness reporting. In the past five years, social media has become the primary way police forces inform the public about unfolding crime trends (Schneider 2016). The fourth supposition in this study is that police forces that more frequently encouraged reporting on social media did not record more hate crime than those forces that encouraged reporting less frequently. We extend this supposition by claiming Crime Survey for England and Wales data do not show any significant variation in hate crime reporting rates before, during and after the study period.

DATA AND METHODS

Data

Police recorded crime data

Our hate crime data for England and Wales contains monthly counts of RR offences at the PFA level, covering 44 PFAs[2] from April 2012 to September 2017 (66 months). We also use data on monthly RR offences in Scotland from January 2015 to December 2018 (48 months) and in Northern Ireland from April 2014 to March 2020 (72 months); Scotland and Northern Ireland each have only one PFA. Data were obtained from the Home Office, Police Scotland and the Police Service of Northern Ireland. While there are known issues with police recorded hate crime data (Williams and Tregidga 2014), there are no alternative datasets available that allow for the longitudinal analysis required to address the specified hypotheses. The issue of variability in victim and witness reporting is addressed in H4. Another issue is the possible variability in recording hate crimes. Preliminary analysis not presented here[3] identified that 'tigger events', such as the Brexit vote, did not have an impact on the likelihood of a hate incident being 'crimed' (where hate incidents become recorded as hate crimes), indicating crime recording does not vary as a function of external forces in our analysis window.

Demographic data

To explore geographical heterogeneity in the increase in hate crime following the Brexit vote, we derive PFA-level demographic characteristics from the 2011 Census (ONS 2017), mid-year population estimates (ONS 2020*b*), local area migration indicators (ONS 2020*a*), referendum vote results (Electoral Commission 2019), and the 2015 English indices of deprivation (MHCLG 2015). Our first set of covariates are demographics chosen based on past research relating hate crime to area characteristics (Ivandic *et al.* 2019; Schilter 2020), namely the remain vote share, unemployment rate, share of residents without formal qualifications and average migrant inflow rate. Secondly, we use the seven components of the 2015 Index of Multiple Deprivation (IMD), namely: income; employment; education, skills and training; health deprivation and disability; crime; barriers to housing and services; and living environment. We convert each IMD component into percentile ranks, from a value of 0 for the least deprived (lowest deprivation score) to 1 for the most deprived PFA for the given IMD component.

Social media police communications

Facebook and Twitter pages of the 43 forces in England and Wales were web scraped to gather all posts sent by police related to raising awareness about hate crime, including encouraging reporting. In total, 2,520 messages between 1 July 2015 and 31 December 2017 were collected. The utility and shortcomings of using social media data in criminological research, including sample limitations, have been well documented (see Williams *et al.* 2013; 2017*a*; 2020; Chan and Bennett Moses 2017). We followed established guidelines to ensure our use of these novel data was ethical (Williams *et al.* 2017*b*).

Table 1 presents descriptive statistics on the outcome and covariates (omitting IMD components that we defined as percentile ranks).

2 These are the 43 territorial PFAs in England and Wales, plus the British Transport Police (BTP). We include the BTP data when estimating the national increase in reported hate crimes after Brexit and comparing Brexit to other key events, but omit it when analysing geographical heterogeneity in the Brexit vote effect with PFA-level covariates on demographics and indices of deprivation.

3 Available upon request.

Table 1. Descriptive statistics for the outcome and covariates in our analysis of geographical heterogeneity in the Brexit vote effect in England and Wales. Data covers 43 territorial PFAs and 1 special PFA (the British Transport Police, omitted from analyses with covariates on demographics and indices of deprivation). Data on RR offences covers 60 months from October 2012 to September 2017 (months for which we can define six lags of the outcome). LA-level covariates aggregated to PFA level. Each PFA's migrant inflow rate is averaged over the six years in the data, weighted by the number of months in the year. Social media data covers 30 months from April 2015 to September 2017

Variable	Observation level	*N* unique obs.	Mean	SD	Min	Max
Outcome						
Count of RR offences, seasonally-adjusted	PFA-month	2,640	76.3	135.0	−0.3	1,200.5
With log(x + 1) transformation	PFA-month	2,640	3.8	1.0	−0.3	7.1
Covariates						
Demographics						
Remain vote share	PFA	43	45.4	7.0	34.7	75.3
Unemployment rate	PFA	43	6.0	1.5	3.8	10.3
No qualifications share	PFA	43	18.3	3.3	5.9	23.2
Average migrant inflow rate	PFA	43	1.0	1.8	0.3	12.5
Count of hate crime-related social media posts	PFA-month	1,320	1.5	3.9	0.0	53.0

Method of estimation

We analyse the impact of the Brexit vote on RR offences using a panel autoregression model estimated at the PFA-monthly level:

$$y_{it} = \sum_{j=1}^{6} \rho_j y_{i,t-j} + \sum_{v=1}^{V} \tau_v v_i + \lambda I_t^{\text{Brexit}} + \sum_{v=1}^{V} \beta_v v_i I_t^{\text{Brexit}} + \gamma t + \epsilon_{it}; \;\; \epsilon_{it} \overset{i.i.d.}{\sim} N\left(0,\ \sigma^2\right) \quad (1)$$

where the dependent variable y_{it} is the log(x + 1)-transformed, seasonally adjusted count of RR offences in PFA i and month t. We use the log(x + 1) transformation rather than the raw count itself to normalise the distribution of RR offences (see Figure A1 in Appendix).

We model y_{it} as depending on its values in the previous 6 months, $\sum_{j=1}^{6} \rho_j y_{i,t-j}$. This component of the model allows each PFA's RR offences to follow a flexible, short-term, non-linear time trend. We also add a linear time trend t to account for the long-run increase in RR offences seen in the data.[4] The combination of these two terms crucially means that we estimate how much RR offences increased following the Brexit vote independently of pre-existing trends. In other words, we estimate the impact of the Brexit vote as the difference between what would have been expected to happen to RR offences given pre-existing trends, and what actually happened. I_t^{Brexit} is an indicator variable equal to 1 if month t is one month after the Brexit vote, i.e. July 2016; its coefficient λ thus estimates the Brexit vote effect on RR offences.

4 We perform a stationarity check on the resulting panel data using an augmented Dickey-Fuller test with an exogenous trend; this rejects the null hypothesis of non-stationarity, suggesting that the addition of t to the regression is useful for the validity of our inference.

The ν_i are V covariates that are generally defined at the PFA level, except police social media posts defined at the PFA-monthly level. The interaction term $\nu_i I_t^{\text{Brexit}}$ examines whether cross-PFA differences in the increase in RR offence rates one month after the Brexit vote can be explained by PFA characteristics given by ν_i. Finally, ϵ_{it} are the residual errors, assumed to be independent and identically distributed (i.i.d.) with mean 0 and variance σ^2.

Given the log specification used for y_{it}, we can generate more easily interpretable results by examining $100\left(e^{\beta}-1\right)$, which approximates the percentage change in RR offences associated with a one-unit increase in the corresponding independent variable from a baseline of zero. Another interpretation of this quantity is the approximate increase in proportion for a large baseline $\left(\text{where } \frac{e^y}{e^y-1} \approx 1\right)$ associated with a one-unit increase in the independent variable.

To explain geographical heterogeneity in the Brexit vote effect, we model three sets of covariates separately: (1) demographics; (2) the English indices of deprivation; and (3) police social media outreach. Covariates (1) and (2) are defined at the PFA level, aggregated from the local authority (LA) level as a weighted average by LA population; whereas (3) is defined at the PFA-monthly level. We model these three covariate sets separately as the relatively small geographical cross-section available for the analysis (N = 44 territorial PFAs in England and Wales) limits our ability to include multiple covariates simultaneously. Therefore, our results should be treated as descriptive (e.g. 'PFAs with higher in-migration rates tended to have smaller increases in hate crimes') rather than causal (e.g. 'higher in-migration rates created environments that were conducive to smaller increases in hate crimes'). We focus on England and Wales because we have only one PFA for each of Scotland and Northern Ireland, making aggregated covariates less useful for those nations.

RESULTS

Estimating the national increase in reported RR hate crimes after Brexit

In this section we report on results from a model estimated using PFA-monthly hate crime data for England and Wales, with full panel autoregression results in Table 2. This model includes indicator variables covering the three months before and after the Brexit vote and is highly effective in explaining the variation in seasonally adjusted RR offences (Adj R^2 = 0.93). Based on this model, Figure 1 visualises the Brexit vote effect.

The impact of the vote on RR hate offences appears to have started in the month of the vote and reached its apex in July 2016. It then rapidly dissipated in autumn. We estimate that RR offences were around 29 per cent higher in England and Wales in July 2016 than they would have been otherwise; this translates to roughly 1,100 additional hate crimes in total. In the raw data, RR offences increased by around the same number from April to July.

In a second version of the model, we include national data from Scotland and Northern Ireland alongside the PFA-level data from England and Wales. We use these series to ask whether the increase in hate crimes after Brexit was bigger or smaller in Scotland and Northern Ireland (both pro-remain) than in England and Wales—in essence to provide a comparison. In the raw data, it is more difficult to visually identify any increase in RR offences following the vote in either Scotland or Northern Ireland compared to England and Wales (Figure 2). While point estimates from the model suggest roughly no increase in either Scotland or Northern Ireland (Table A1 in Appendix), the confidence intervals on both estimates are extremely wide because we only have one geographical unit in each case, meaning the model cannot estimate the differences across nations with much precision. We are therefore unable to conclude that there was a *statistically* significant difference in the Brexit vote effect across nations of the United Kingdom. Piatkowska and Lantz (2021) similarly find no evidence of a Brexit vote effect in Scotland; however, their interrupted time-series analysis at the monthly level is also constrained by a small sample size (43 months of hate crime data in Scotland).

Table 2. Panel autoregression results for RR offence counts at the PFA-month level, modelling the effects of the 3 months before and after the Brexit vote. Covariates are indicator variables for the three months before and after the Brexit vote, six lags of the outcome and a linear time trend. Sample is 44 PFAs in England and Wales across 60 months (October 2012–September 2017) for which the six lags are available

Log(x + 1) seasonally-adjusted RR offences	
(Intercept)	−0.331* (0.174)
Lag 1	0.342*** (0.020)
Lag 2	0.196*** (0.021)
Lag 3	0.147*** (0.021)
Lag 4	0.106*** (0.021)
Lag 5	0.128*** (0.021)
Lag 6	0.070*** (0.020)
Linear time trend	0.001** (0.000)
3 months before vote	0.010 (0.041)
2 months before vote	−0.078* (0.041)
1 month before vote	0.045 (0.041)
Month of vote (June 2016)	0.110*** (0.042)
1 month after vote	0.254*** (0.042)
2 months after vote	0.041 (0.042)
3 months after vote	0.059 (0.042)
Observations	2,640
R^2	0.930
Adjusted R^2	0.929

Notes: *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

Comparing the Brexit vote to other key events

To model the effects of other key events that may have impacted hate crime and to provide a benchmark for further comparison, we adapt Equation (1) by adding an indicator variable for each event that takes the value 1 if month *t* is the month of or the month after the event, and 0 otherwise. This means that the estimated coefficients measure the average increase in RR offences over a two-month period for the Brexit vote and other key events as listed in Table A2 (see Appendix).[5]

Figure 3 illustrates the results of this analysis, showing the nine events that had the largest absolute estimated effect on RR offences. We do not report a table with full regression results here due to the large number of indicator variables in the model, but the main results of interest are visible from the figure. We estimate that the increase in RR offences following the Brexit vote is smaller than that following the later Manchester Arena terror attack, but a little larger than that following the Westminster Bridge attack. All other events we tested were associated with statistically insignificant or negative (in the case of the Parsons Green terror attack) changes in RR offences.

5 Some events occur in the same month, meaning that we cannot separate their effects using monthly hate crime data.

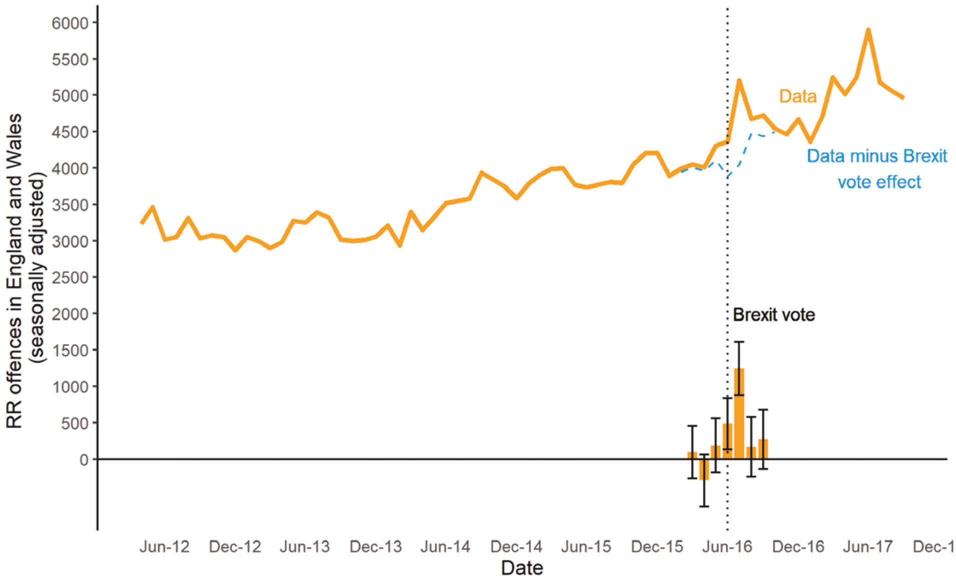

Fig. 1. Increase in hate crimes in England and Wales after the Brexit vote. Brexit vote effect calculated based on panel autoregression results in Table 2. The bars show estimated excess RR offences in the months surrounding the vote. The dotted line simply subtracts those bars from the data for illustrative purposes.

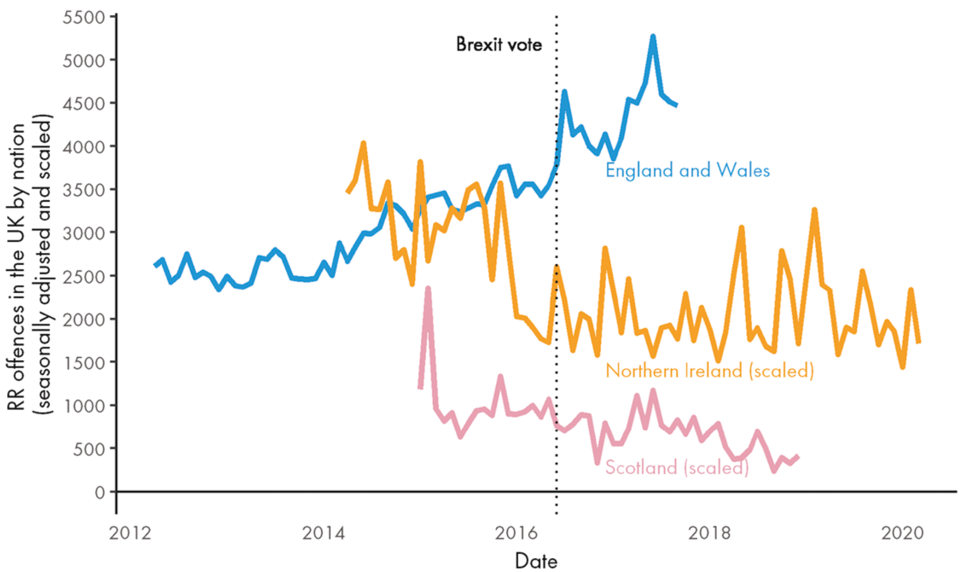

Fig. 2. RR offence counts over time in nations of the United Kingdom.

Explaining geographical heterogeneity in the increase in RR hate crimes following the vote in England and Wales

As described in Equation (1), we explore geographical heterogeneity in the Brexit vote effect by including interaction effects between the month after the Brexit vote (July 2016) and covariates of interest. We focus on England and Wales, and model three sets of covariates

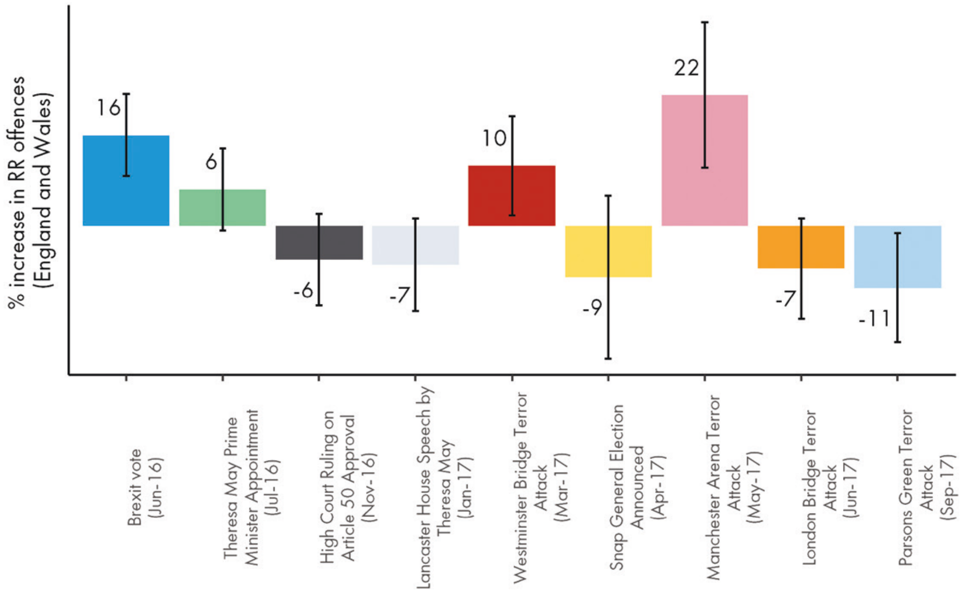

Fig. 3. Comparison of increases in RR offences following various key events in the United Kingdom. The estimated effect is the average increase in RR offences over the month of the event and the month after it.

Table 3. Panel autoregression results for RR offence counts at the PFA-month level, focusing on the interaction effects between the month after the vote and demographic characteristics. Covariates are six lags of the outcome, a linear time trend and constituent terms of the interactions shown here, i.e. an indicator variable for the month after the Brexit vote (July 2016) and these demographics. Sample is 43 territorial PFAs in England and Wales across 60 months (October 2012–September 2017) for which the six lags are available

Log(x + 1) seasonally-adjusted RR offences	
Month after vote	
×Remain vote share	–0.019* (0.011)
×Unemployment rate	–0.014 (0.041)
×No qualifications share	–0.027 (0.026)
×Migrant inflow rate	0.001 (0.035)
Observations	2,580
R^2	0.928
Adjusted R^2	0.927

Notes: *p < 0.1; **p < 0.05; ***p < 0.01. Standard errors in parentheses.

separately: (1) demographics; (2) the English indices of deprivation; and (3) police social media outreach.

Table 3 shows the results of the model on demographic characteristics, focusing on the interaction effects between the month after the Brexit vote (July 2016) and these demographics. These interactions ask whether cross-PFA differences in the increase in hate crimes one month after the Brexit vote can be explained using demographic characteristics of PFAs. Figure 4 shows how the impact of each variable on the increase in hate crimes after Brexit changes over its range

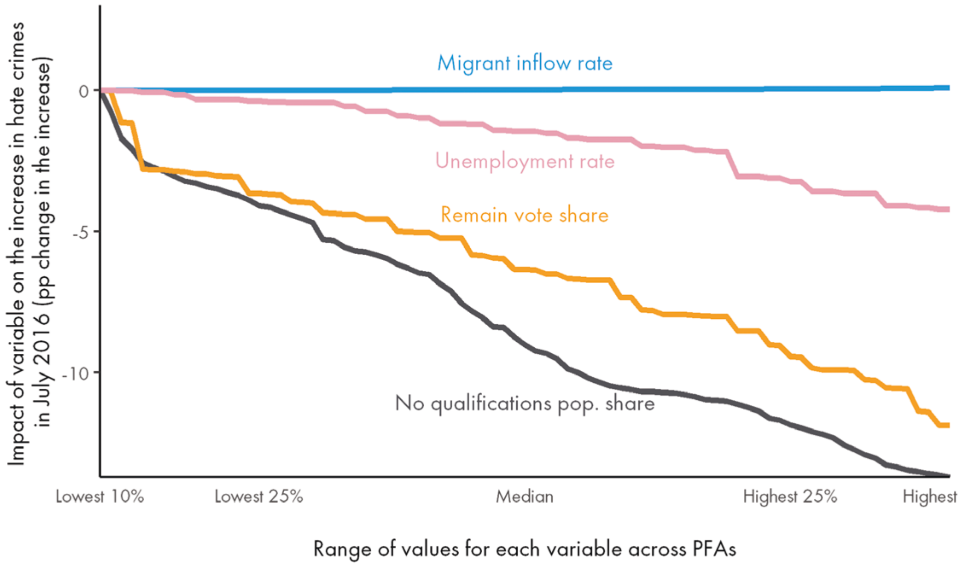

Fig. 4. Relationship between key demographic variables and the increase in RR offences after the Brexit vote, based on the model in Table 3.

in the data, starting at the 10th percentile for each variable and ending at the 90th (we exclude the most extreme values because estimates at these extremes are generated with considerable uncertainty due to the small number of relevant PFAs). In other words, going from left to right on the chart for a given variable shows how the increase in RR offences varies in the data across values of that variable (relative to what it is at the 10th percentile for that variable). For example, the relationship between the migrant inflow rate and the increase in RR offences is statistically insignificant, matching the fact that the line is close to horizontal (higher migrant inflow rates were not associated with a change in the increase in hate crimes).

Importantly, we find that PFAs with greater remain vote shares tended to have smaller increases in hate crimes after the Brexit vote. To give an illustration, moving from a remain vote share of 38 per cent at the 10th percentile (Essex) to the 90th percentile value of 52 per cent (Surrey) implies a circa 12 percentage point reduction in the increase in hate crimes. The demographic covariates do not have statistically significant relationships with the increase in RR offences after the Brexit vote. This may be partly explained by the small cross-section of 43 PFAs used in the analysis.

We also estimate an alternative model in which we replace the previous set of covariates with components of the 2015 IMD, though retaining the remain vote share so it does not confound the results. We do not find evidence that any of the IMD components bear a relationship with the increase in hate crimes after the Brexit vote (Table A3). That being said, it is worth reiterating that the standard errors on these interaction terms are large due to the small cross-section of 43 PFAs (see Limitations section).

Finally, we explore whether geographical heterogeneity in the Brexit vote effect within England and Wales can be explained using data on hate crime-related social media posts by police forces. On a national level, there does not appear to be a clear temporal link between hate crime-related social media posts and the increase in hate crimes after Brexit (Figure 5). Indeed, the number of posts made by police forces across the country remained relatively stable through the period. Nonetheless, that does not rule out the possibility that differences across PFAs in levels of police engagement could be a predictor of the differences in the Brexit vote effect. For example, the following Facebook post made by Northamptonshire Police on 28 June 2016 has the potential to increase hate crime reporting rates:

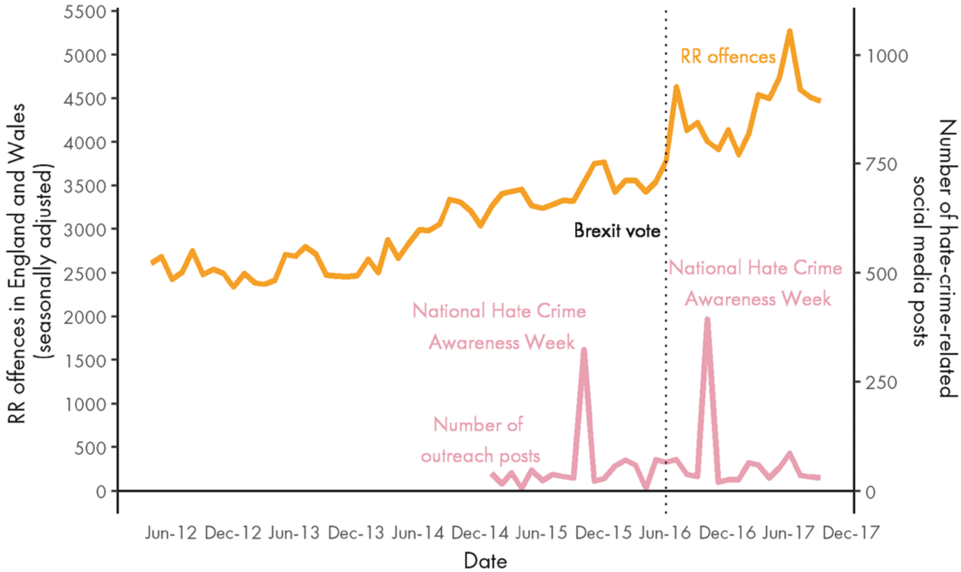

Fig. 5. Monthly counts of RR offences and police social media posts related to hate crime.

> Police in Northamptonshire are urging people to report hate crimes after a rise in incidents across the country at the weekend.

We do not find evidence that differences in social media activity can help explain differences in the increase in hate crimes across PFAs in England and Wales (Table A4). That is particularly surprising because we also include social media activity in July 2016 itself. The nature of police hate crime and social media data means we cannot completely rule out an effect due to sample limitations, however, complementary CSEW analysis presented in the discussion favours the argument that changes in reporting had a marginal effect on variability in recorded hate crime counts.

DISCUSSION

Supporting the first hypothesis, we found that following the referendum vote on the future of the United Kingdom in the EU, there were an additional c.1,100 RR hate crimes (a 29 per cent increase on the month prior) in England and Wales than there would have been in the absence of the vote. This is slightly smaller than Carr *et al.*'s (2020) estimate of a 35–39 per cent increase in England and Wales at the PFA level, but consistent with all previous work that has examined the effect of the Brexit vote on RR hate crimes (Albornoz *et al.* 2020; Schilter 2020; Piatkowska and Lantz 2021). This finding is also consistent with the conceptual work on the temporal patterning of hate crime which is largely driven by 'trigger events' (Hanes and Machin 2014; King and Sutton 2014; Williams and Burnap 2016; Edwards and Rushin 2019; Müller and Schwarz 2020). While we were unable to directly test the theory due to the lack of relevant variables at the PFA level, like the studies before ours, we interpret the dramatic spike in RR hate crimes by considering the vote outcome as a 'shock' that reduced suppression and increased justification forces for the expression and experience of prejudice in the form of hate crimes (Crandal and Eshleman 2003). One such justification force that has been associated with hate crime is the perception of realistic and symbolic threat (e.g. threats to jobs, housing, etc. and threats to a way of life, respectively) (Stephan and Stephan 2000). Such threats were increasingly portrayed as emanating from

EU migrants by the Vote Leave, Leave.EU and UKIP campaigns in the weeks running up to the vote. A study on the press in the weeks leading up to the vote found immigration and the economy were the two most-covered issues in reportage described as acrimonious and divisive, with particular groups (Turkish and Polish) receiving negative treatment (Moore and Ramsay 2017).

Supporting the second hypothesis we found that the vote effect on RR hate crimes was equal to, or greater than, 'trigger events' that took place after, between June 2016 and September 2017. The only 'trigger event' likely to exhibit a greater effect was the Manchester Arena terror attack in May 2017. This finding extends the work of Carr *et al.* (2020) who compared the effect of the events on RR hate crime before the vote, concluding that the effect of a terrorist attack and the public information shock of the referendum outcome are comparable. Similar to the vote, terror attacks prompt mass news coverage that have the potential to increase perceived threat, not only to life, but to a way of life, which in turn can increase the justification for the expression and experience of prejudice. Our finding resonates with that of Legewie (2013) who established a significant association between anti-immigrant sentiment and the Bali and Madrid terrorist bombings using Eurobarometer data. The Bali attack was the cause of a significant worsening in attitudes towards all immigrants in Portugal, Poland and Finland. The strength of the effect of the attack was enhanced if the person lived in an area with high unemployment—both Poland and Portugal showed the highest increase in unemployment in 2001–02. The effect was also stronger on people who did not have immigrants as friends or co-workers but who lived in areas with high immigrant numbers. These findings were replicated in relation to the Islamist terrorist bombing in Madrid in 2004. The proportion of the Spanish population that thought immigration was one of the most important issues the country was facing rose from 8 per cent to 21 per cent immediately after the attack, with the effect being strongest in areas with high unemployment. This study supports the finding that terror attacks cause a worsening in attitudes towards immigrants, especially in people who live in areas characterised by high unemployment, high immigration and low contact with the outgroup.

To identify if similar demographic characteristics played a role in shaping the RR hate crime rate post vote, we hypothesised that areas with the highest spike in Brexit-related hate crime also had high migration, unemployment and leave vote share. The demographic portion of our supposition was based on research that shows some areas that voted overwhelmingly to leave the EU saw inward migration rise from 1 in 50 to 1 in 4 the decade before. These places also suffered some of the biggest cuts in jobs and services in the United Kingdom. Migration to these areas is largely comprised of younger non-English speaking low-skilled workers (ONS 2017). The combination of unemployed locals and an abundance of employed migrants, competing for scarce resources in a time of recession and cutbacks, creates a greater feeling of 'Us' and 'Them' resulting in a process of 'Othering'. A lack of interaction between the local and migrant population results in rising tensions, due to a lack of inter-cultural transmission and understanding. Research shows that Polish migrants felt more at risk in deprived areas with a high white working-class presence, than in more affluent areas (Rzepnikowska 2019). We hypothesised that the combined temporal shock of the vote outcome interacting with demographic factors associated with increased threat perception and ingroup preferences would result in reduced suppression and increased justification for hate crimes. Our hypothesis was only partially supported, with only vote share emerging as significant, suggesting the demographic characteristics above played a non-existent or limited role when analysed alongside each other and other factors at the PFA level. However, the lack of association may be a result of the small cross-section of 43 PFAs used in the analysis, resulting in reduced statistical power.

The third hypothesis was however partially supported, as those PFAs with greater remain vote shares tended to have smaller increases in hate crimes after the Brexit vote. In addition, Scotland and Northern Ireland, with largely remain voting PFAs, also showed smaller increases. These results are supported by Carr *et al.* (2020) but are in contrast to Albornoz *et al.* (2020) and Schilter (2020) who find a greater increase in hate crimes in remain areas. Albornoz *et al.*, using data from the British Election Study, found that a 1 per cent increase in the proportion voting remain in each CSP increased the level of hate crime by 0.5 per cent. They explained their finding by arguing that vote outcome shifted the dominant worldview by exposing that anti-immigrant attitudes were more widespread than was previously believed, and therefore justifying them, more so in remain areas. This is because the behavioural adjustment was larger in these areas—prejudiced individuals living in remain areas that are pro-diversity will likely have a history of supressing prejudice, against their preference, but when they discover (or perceive) that the country at large shares their views, the justification for the expression of prejudice increases. However, like Carr *et al.*, we advance a more straightforward argument to explain our finding that hate crimes tended to be higher in leave voting PFAs in England and Wales—in leave areas there are more people with existing prejudiced attitudes towards racial and religious minorities, who felt justified to express these attitudes verbally and violently, following the shock outcome of the vote that indicated they were not alone in their thinking.

The differences between studies that find increased hate crime rates in Leave areas, and those that show increased hate crime rates in Remain areas, are best explained by methodological factors, such as statistical model specification, study design and spatial and temporal scales of the data used in estimation. For example, Schilter only included London and Manchester in their models, and Carr *et al.* note that the determinant in Albornoz *et al.* is the interaction of 'post-Brexit' with a share of the vote for Remain in the Community Safety Partnership area, but because of fixed effects the vote share cannot be estimated, and the post-Brexit dummy is not included in the regression. When the dummy is included, the effect in Remain areas disappears.

As our analysis is dependent on police recorded crimes, we wanted to rule out the role of more reporting in the increase found in RR hate crimes. As no conventional measure on variability in reporting by PFA was available, we innovated by collecting police social media communications. Over the past decade, social media has become the most used method for engaging with the public, as it benefits from mass reach and immediacy of communication (Schneider 2016). We found that variation by PFA in police social media communications encouraging hate crime reporting was not associated with recording rates of RR hate crimes in the wake of the Brexit vote, supporting the last hypothesis (sample limitations accepted). If we accept that these communications are a viable proxy measure for variability in reporting, we can *tentatively* conclude that the rise in RR hate crimes following the vote was driven primarily by an increase in actual perpetration rather than reporting. This argument is supported by data from the Crime Survey for England and Wales that shows the two-year average reporting rate for RR hate crime in the 2011–12 to 2014–15 and the post vote period was near identical (50.6 per cent and 50.5 per cent respectively). However, we accept that the two-year averaged CSEW measures are too blunt a tool to identify changes in reporting behaviour in the month after the vote. Carr *et al.* use more granular data from the CSEW in a formal test of any change in the probability of reporting RR hate crimes, relative to other crimes, before and after the referendum vote. They found that the probability of reporting rose by 5.5 per cent post vote, concluding that around one-third of the increase in RR hate crimes is due to an up-tick in reporting behaviour. However, this is likely to be a significant overestimate given the CSEW does not include a large proportion of aggravated intentional harassment, alarm or distress offences[6] that made up the majority of RR hate crimes post vote.

Limitations

There are several limitations to this work which future studies might seek to address. Utilising PFA at the spatial level and month at the temporal level precluded a more nuanced analysis of the role of demographic factors. Future work should consider lower spatial and temporal scales,

6 Importantly, CSEW counts of intentional harassment, alarm, or distress offences exclude crimes where no individual victim can be identified or interviewed, such as some public order offences (e.g. racial slurs that do not target an individual but a group).

such as lower or middle-super output area and week, in order to increase statistical power. This may be done in tandem with a more focussed analysis on metropolitan areas, such as London, that have their own unique set of demographic and voting patterns, to identify any variation to the national picture. A lack of data on variation in the activity of local online leave campaigns by PFA meant that we could not isolate their effect on RR hate crime. Previous work has found a link between traditional media output and variation in Brexit-related RR hate crime (Carr *et al.* 2020) and future work may seek to explore if a similar link exists with online output should geo-tagged data become available. In addition, the hate crime data supplied to us did not provide a breakdown of RR offences by crime type across all areas. Future work may wish to identify if particular crime types (e.g. intentional harassment, alarm or distress) were more or less prone to a rate change post the Brexit vote. Finally, a lack of data on hate crime police training over time and PFA meant that we could not isolate the effect of changes in police recording practice. Future work may obtain such data via freedom of information requests, or develop alternative proxy measures.

CONCLUSION

This paper provides the first Brexit-related RR hate crime comparison between England and Wales, Scotland and Northern Ireland, showing that remain areas saw smaller increases, but that theoretically relevant demographic factors did not emerge as predictive. Additionally, our models found that, bar one event, the Brexit vote had the greatest effect on RR hate crime in the analysis period. Finally, using a novel proxy measure for variation in hate crime reporting, we found no association indicating the rise in Brexit-related RR hate crimes was more likely a function of increased perpetration. Our analysis lends some support to the Justification–Suppression Model of the Expression and Experience of Prejudice (Crandal and Eshleman 2003). It seems reasonable to assume the Brexit vote acted as a 'trigger event', communicating novel information about group processes that galvanised existing negative prejudices towards outgroups. The vote outcome therefore likely represented a 'shock' that reduced the suppression and increased the justification for the expression of prejudice resulting in an increase in RR hate crimes. However, we found no support for Integrated Threat Theory (ITT) (Stephan and Stephan 2000), as our demographic factors that acted as proxies for economic threat (e.g. threats to jobs, housing, NHS waiting times) and symbolic threat (e.g. threats to way of life) did not reach statistical significance (although this is possibly due to limits to statistical power which our future work seeks to remedy).

There seems to be no slowing in the rise in police recorded hate crime, and in the regularity of trigger events (e.g. Brexit, COVID-19, Russia-Ukraine conflict) that seem to have powerful observable positive associations with the hardening of prejudiced attitudes and in turn the expression of identity-based hostility. Significant questions remain over the short- and long-term governance of hate crime. The Government's continued reliance on traditional criminal justice interventions of more or better policing and harsher sentencing must remain under question. That hate crime is so dependent on temporal forces clearly suggests a reassessment of the utility of these governance models, designed in response to less retaliatory and defensive crimes, is in order.

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DATA ACCESSIBILITY

The social media data used in this research are available from the Cardiff University data repository at http://doi.org/10.17035/d.2022.0218582866.

APPENDIX

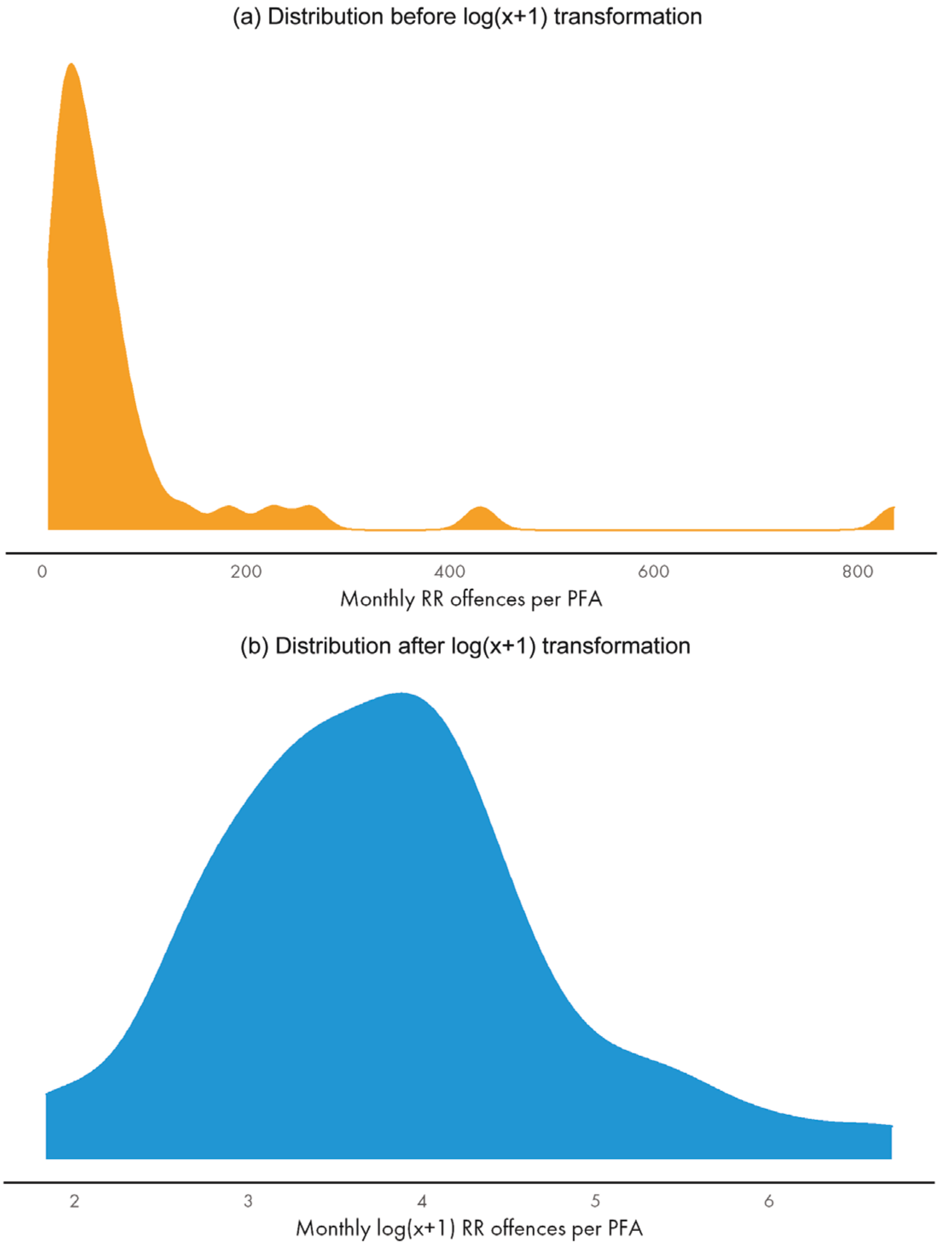

Figure A1. Distributions of monthly RR offence counts across PFAs, before and after $\log(x + 1)$ transformation. Applying the $\log(x + 1)$ transformation yields an approximately normal distribution of monthly offence counts across PFAs. While the dependent variable is at the PFA-monthly level, we plot its average value at the PFA level here for illustrative purposes.

Table A1. Panel autoregression results for RR offence count at the PFA-month level, comparing the Brexit vote effect (month after vote) across countries in the United Kingdom. Data on England and Wales for 44 PFAs across 60 months (October 2012–September 2017), Scotland across 42 months (July 2015–December 2018) and Northern Ireland across 66 months (October 2014–March 2020) for which the six lags are available

Log(x + 1) seasonally-adjusted RR offences	
(Intercept)	–0.410** (0.162)
Lag 1	0.344*** (0.020)
Lag 2	0.197*** (0.021)
Lag 3	0.148*** (0.021)
Lag 4	0.100*** (0.021)
Lag 5	0.125*** (0.021)
Lag 6	0.074*** (0.020)
Linear time trend	0.001*** (0.000)
Month after vote	0.249*** (0.041)
Scotland	–0.026 (0.044)
Northern Ireland	–0.061* (0.034)
Month after vote	
×Scotland	–0.288 (0.273)
×Northern Ireland	–0.182 (0.271)
Observations	2,748
R^2	0.934
Adjusted R^2	0.934

Notes: *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$. Standard errors in parentheses.

Table A2. Key events that may have impacted hate crime

Date	**Event**
7 Oct. 2015	Conservative Party Conference Keynote by David Cameron
12 Oct. 2015	Hate Crime Awareness Week
13 Nov. 2015	Paris Terror Attack
20 Feb. 2016	Referendum Announced
21 Mar. 2016	International Day for the Elimination of Racial Discrimination
22 Mar. 2016	Brussels Terror Attack
17 May 2016	International Day Against Homophobia, Transphobia and Biphobia
16 Jun. 2016	Murder of Jo Cox
23 Jun. 2016	Referendum
24 Jun. 2016	David Cameron Announces Resignation
13 Jul. 2016	Theresa May Prime Minister Appointment
14 Jul. 2016	Nice Terror Attack
5 Oct. 2016	Conservative Party Conference Keynote by Theresa May

Table A2. Continued

Date	Event
8 Oct. 2016	Hate Crime Awareness Week
3 Nov. 2016	High Court Ruling on Article 50 Approval
19 Dec. 2016	Berlin Terror Attack
17 Jan. 2017	Lancaster House Speech by Theresa May
21 Mar. 2017	International Day for the Elimination of Racial Discrimination
22 Mar. 2017	Westminster Bridge Terror Attack
29 Mar. 2017	Article 50 Invocation
18 Apr. 2017	Snap General Election Announced
17 May 2017	International Day Against Homophobia, Transphobia and Biphobia
22 May 2017	Manchester Arena Terror Attack
3 Jun. 2017	London Bridge Terror Attack
8 Jun. 2017	General Election
19 Jun. 2017	First Round of Brexit Negotiations
19 Jun. 2017	Finsbury Park Terror Attack
15 Sep. 2017	Parsons Green Terror Attack
22 Sep. 2017	Florence Speech by Theresa May
4 Oct. 2017	Conservative Party Conference Keynote by Theresa May
14 Oct. 2017	Hate Crime Awareness Week
8 Dec. 2017	Joint EU/UK Brexit Report on Future Divorce Terms

Table A3. Panel autoregression results for RR offence counts at the PFA-month level, focusing on the interaction effects between the month after the vote and components of the 2015 Index of Multiple Deprivation. Covariates are six lags of the outcome, a linear time trend, Remain vote share and constituent terms of the interactions shown here, i.e. an indicator variable for the month after the Brexit vote (July 2016) and these IMD components. Sample is 43 territorial PFAs in England and Wales across 60 months (October 2012–September 2017) for which the six lags are available

Log(x + 1) seasonally-adjusted RR offences	
Month after vote	
×Income	−0.178 (0.262)
×Employment	0.208 (0.319)
×Education, Skills and Training	−0.018 (0.083)
×Health Deprivation and Disability	−0.007 (0.110)
×Crime	−0.002 (0.060)
×Barriers to Housing and Services	0.056 (0.045)
×Living Environment	−0.025 (0.048)
Observations	2,580
R^2	0.928
Adjusted R^2	0.927

Notes: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Standard errors in parentheses.

Table A4. Panel autoregression results for RR offence counts at the PFA-month level, focusing on the interaction effects between the month after the vote and police social media posts related to hate crime. Covariates are six lags of the outcome, a linear time trend and constituent terms of the interactions shown here, i.e. an indicator variable for the month after the Brexit vote (July 2016) and these PFA-monthly counts of social media posts. Sample is 44 PFAs in England and Wales across 30 months (April 2015–September 2017)

Log(x + 1) seasonally-adjusted RR offences	
Month after vote	
×Count of posts in July 2016	−0.027 (0.024)
×Count of posts in June 2016	−0.004 (0.020)
×Count of posts in May 2016	0.008 (0.010)
×Count of posts in April 2016	0.005 (0.037)
Observations	1,320
R^2	0.940
Adjusted R^2	0.939

Notes: $^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Standard errors in parentheses.

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