

The UK Referendum on Membership of the European Union as a Trigger Event for Hate Crimes

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Abstract

Following the UK's referendum on membership of the European Union, the possibility that the referendum led to an increase in hate crimes dominated national debate. This paper studies this hypothesis by applying time series intervention models to daily data covering two years and monthly data covering four years. The results show that the referendum led to an increase in hate crimes of 19-23%, controlling for other interventions and the salience of immigration. The paper secondly applies vector autoregression models to the question of whether this was a product of media priming or a spiralling effect, where media coverage increases reports of hate crimes independent of any actual increase in crimes. There is no evidence for this hypothesis, providing a robustness test for the main inference. Aside from these important empirical findings, the paper contributes to the evidence base linking discrete events with hate crimes, and provides significant leverage in arbitrating between the causal mechanisms linking events with hate crimes.

Introduction

Following the UK's referendum on membership of the European Union, debate dominated political elites, national newspapers, and even the United Nations about the role of the referendum in increasing hate crimes. Labour MP Stella Creasy, a year after the referendum, argued that 'there can be no doubt the Brexit vote has had an impact' (Bienkov, 2017), whilst *The Spectator* acknowledged the increase in hate crimes, but that 'it's not fair to blame Brexit' (Goodenough, 2017). In October 2017, a formal inquiry was launched by the Home Office into the causes and consequences of hate crimes in response to this public concern, real or imagined. However, there was little rigorous empirical investigation of this claim or discussion about why one would expect a referendum, or any event, to trigger an increase in hate crime.

Previous research has found that discrete events have substantively important effects on the occurrence of hate crimes (King and Sutton, 2013; Borell, 2015; Disha *et al*, 2011), and has similarly investigated the underlying mechanisms driving hate crimes in general and how they are linked to events in particular (Hanes and Machin, 2014; Green *et al*, 2001; Craig, 2002). The evidence base for this is largely concentrated to the occurrence of terrorist attacks, though some also explore the effect of controversial court trials and rulings (King and Sutton, 2013). Building on this past research, this paper extends the evidence base to a very different type of event, namely, the UK's referendum on membership of the European Union. The paper uses time-series intervention models on daily and monthly data of hate crimes to gauge the causal impact of the referendum, controlling the salience of immigration and other shocks to the time series, such as terror attacks. The results indicate that the referendum result led to a statistically significant increase in hate crimes larger or equivalent to large terror attacks in the UK, representing approximately a 23% increase on the daily average or a 19% increase on the monthly average. I additionally provide evidence against a 'spiralling' or 'media priming' effect on the occurrence of hate crimes using a vector autoregression model on the quantity of media coverage of hate crimes and their occurrence, suggesting a valid causal inference on the effect of the referendum on hate crimes.

Aside from the empirical findings, this provides important theoretical insights for the literature on the causes of hate crimes. Given that much of the existing evidence comes from large-scale terror attacks, there has been debate about whether antecedent hate crimes are a 'backlash' from the victim group, or rather media coverage and framing of the event. The extension of these theories to the Brexit referendum allow us considerable leverage on arbitrating between these causal mechanisms. The data used in this paper does not include White or British victims, and the type of event is less likely to cause a backlash effect than terror attacks, which makes the backlash hypothesis less plausible and rather turns the focus on media coverage and framing of the event (Hanes and Machin, 2014; Legewie, 2013). The paper also extends the relationship to a distinctly new type of event, and one that is distinctly political. The analysis also includes numerous terror attacks and shows how hate crimes increase after these, strengthening the evidence on the link between discrete events and hate crime occurrence in general and attacks in particular (Borell, 2015). The methods used in the investigation also heed the call from Green and Spry (2014) to use quasi-experimental methods in the study of hate crimes.

The paper proceeds by laying out the context of the referendum vote and its campaign, before discussing the literature on the determinants of hate crime and their association with discrete events. The subsequent two sections develop the data, empirical strategy and results. I then provide evidence that media priming or a ‘spiralling’ effect did not play a role in the generation of hate crime reports. I conclude by discussing the paper in light of the wider theoretical relevance on the link between (political) events and the occurrence of hate crimes.

Context of the Referendum

The popular belief that the referendum could have led to hate crimes lay, firstly, in its highly divisive and partisan campaign and, secondly, that such a campaign revolved around immigration and its impact. Both sides of the debate – ‘Remain’ and ‘Leave’ – were criticised by independent organisations for misleading or factually incorrect campaign lines, reflecting their partisan biases. Whilst the most widely used examples of this dishonesty came from the Leave campaigns, such as Boris Johnson’s claim to seize back £350 million for the National Health Service, the Remain campaigns were in reality little better. George Osborne claimed an emergency budget of tax rises and spending cuts would be necessary in the event of a leave vote, and Alan Johnson argued that two thirds of manufacturing jobs were dependent on the EU (Stone, 2017), the former of which never materialised and the latter a spurious claim.

The Remain and Leave campaigns took the economy and immigration as their key issues, with the latter dominating public media coverage: whilst the economy was technically the most covered issue – with immigration as a second – 47% of these economy-based articles mentioned immigration (Moore and Ramsay, 2017). Indeed, this was purposeful: the Campaign Director for Vote Leave told Leave-supporting politicians that to win they would need to “hit Cameron and Osborne over the head with a baseball bat with immigration written on it” (Evans and Menon, 2017, 53). It was thus this issue which dominated the news agenda, the dominance of which increased over the campaign (Evans and Menon, 2017). As a study by Moore and Ramsay (2017, 9) explains:

“Coverage of the effects of immigration was overwhelmingly negative. Migrants were blamed for many of Britain’s economic and social problems – most notably for putting unsustainable pressure on public services. Specific nationalities were singled out for particularly negative coverage – especially Turks and Albanians, but also Romanians and Poles.”

Quantifying this, they find that across all national newspapers, just 17 of 307 articles mentioned immigration and these nationalities were on average more positive than negative. In addition, there was not much room for cross-campaign compromise: outlets were highly partisan, aggressive and divisive (Moore and Ramsay, 2017).

Aside from media coverage, events during the campaign were equally divisive. Nigel Farage, leader of UKIP, was widely criticised and reported for inciting racial hatred after unveiling a poster which pictured migrants crossing the Croatia-Slovenia border in 2015. On the same day, Remain-supporting Labour MP Jo Cox was murdered in her

constituency in a politically-motivated attack. It is following these two events, coinciding on the same day, that the narrative changed towards hate crime and the consequences of the campaign. It is in this divisive atmosphere that referendum was held a week later on the 23rd of June.

Theory

However compelling this narrative, there is still considerable debate about whether, and how, events lead to hate crimes, and the debate on the impact of the referendum was never settled in the public sphere. For the claim to have credibility, the referendum must be linked with some causal mechanism leading to hate crimes. Previous research has shown how different types of events can lead to an increase in hate crimes or changing attitudes towards immigrants. This has largely been limited to terror attacks, but others have explored other contentious events such as trial verdicts (King and Sutton, 2013). However, the underlying causal mechanism is still contested. Here, I set aside the micro-level explanations (for an overview see Hanes and Machin, 2014; Green *et al*, 2001; Craig, 2002; Iwama, 2018) and focus on the macro-social explanations of relevance to political events. This is a reasonable omission, since hate crimes tend to be a form of group behaviour rather than isolated individuals (Craig, 2002).

One causal pathway is through the encouragement of elites. This may be through politicians or other elite actors directly supporting or condoning prejudicial views and actions, which can lead to hate crimes (Karapin, 1999). For example, Hall (2014) attributes a rise in hate crimes against disabled people to the rhetoric of the British government in relation to 'benefit cheats' or those faking disabilities, whilst Karapin (1999, 439) attributed the rise in anti-immigrant movements during the 90s to the rhetoric of 'sympathetic or blundering subnational elites'. A second form of elite encouragement lies in the media. 'Sensationalist coverage' of events, it is argued, can produce a 'hate crime contagion' (Green, McFalls and Smith, 2001) through 'perpetuating or legitimating stereotypes'. This is a significant contribution; whilst existing hate crime literature does not make the link, the ability of the media to drive support for anti-immigrant or far-right parties is well acknowledged within political science (Walgrave and Swert, 2004; Boomgaarden and Vliegenthart, 2009; Vliegenthart and Boomgaarden, 2010). Linking these two mechanisms to events in particular, it has been shown that the contagion effect and the dynamics of elite rhetoric are particularly potent following 'triggering events' (Hall, 2014).

An alternative current of research has suggested that it is rather the effects of *opportunity*. The argument is that hate crimes are variable depending on the available political opportunity structure, which is defined as 'the availability of channels to express grievances, the legitimacy of grievance within public and political discourse, and the likelihood of prevention or punishment of hate-motivated crimes' (Green, McFalls and Smith, 2001). The premise of this is that exogenous factors alter the probability of mobilisation of political activities, such as hate crimes or social movements (Meyer and Minkoff, 2004). As Meyer and Minkoff (2004) find in the US, people take advantage of 'a favourable climate of political support and public opinion' to advance their movements, of which 'the impression of greater political access' and perceived government support is a key factor. Seen from this theoretical perspective, the referendum may have been one such exogenous factor which altered the perception of the legitimacy of prejudicial views

and their prevalence amongst the public, leading to a rise in hate crime following the event.

A third causal mechanism is through a 'backlash' effect following events, and in particular terror attacks. Drawing on group-threat theory, it is argued that terror attacks foster the perception of the out-group as threatening which directs attention to sources of intergroup conflict (Quillian, 1995; Legewie, 2013; Hopkins, 2010). These also have long-term effects on attitudes. For instance, surprisingly high levels of anti-Muslim attitudes were found in Spain, largely as a result of the Madrid bombings (Savelkoul *et al*, 2011). Since this requires a perception of threat (Schlueter and Scheepers, 2010), terror attacks are linked to the increase in hate crimes since they provide this perception of threat, and in particular towards the 'prevailing way of life or the foundation of national identity' (Ceobanu and Escandell, 2010). The response by the dominant group to such attacks is a 'backlash' or retaliation derived from this feeling of threat. It is not clear to what extent this is valid for other types of events.

Each of these mechanisms could plausibly link the referendum to an increase in hate crimes. As discussed, the referendum was conducted in an environment in which immigration and national sovereignty was a key concern, and in which attitudes, particularly on immigration, were both extreme and polarised. This lends itself easily to the interpretation that it was the role of media and elites in generating stereotypes and ill-feeling towards out-group members that led to the increase in hate crimes after the referendum. At the same time, however, a question could be raised about why hate crimes did not increase during the campaign, in which the coverage was of greater intensity. This shifts the perspective to the remaining two mechanisms. Firstly, the referendum *result* may have legitimated those already-existing narratives, leading to a perceived positive opportunity structure. Likewise, the result may have led to group conflict, leading to a backlash effect.

Of course, these mechanisms are clearly related. It is hard to imagine how the referendum altered the opportunity structure available to individuals without first having some form of media coverage surrounding it, as encouraged by elites or the media. Similarly, the framing of an event and who is seen as responsible has implications for the (extent of) group threat and the scale and longevity retaliation attacks. The events currently studied in the literature make these mechanisms difficult to disentangle. Whilst the aim of this paper is not to determine which of these is at work, the discussion section suggests the more likely of mechanisms. The extension of this theoretical literature to a new type of event allows considerable leverage over this theoretical debate.

Data and Method

Hate Crime Data

To test the hypothesis that the referendum led to an increase in hate crimes, I rely on official data from the UK government. Both daily and monthly data were taken from the UK Home Office's official statistics, updated to August 2017, which cover England and Wales. A 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' (O'Neill, 2017, 2). However, the data used in this analysis is

restricted to *racial* and *religious* hate crimes which includes ‘racially or religiously aggravated assault with injury, racially or religiously aggravated assault without injury, racially or religiously aggravated criminal damage, racially or religiously aggravated public fear, alarm or distress and racially or religiously aggravated harassment’. This means, for instance, it could extend from racist graffiti to religiously-motivated murder.

There are two important consequences to using this data. The first is that this encompasses a wide range of offences, from offensive graffiti to criminal damage or assault. I do not differentiate between type of severity of crime. The second is that this data is of *reported* hate crimes which do not require prosecution. This provides an obstacle to the relationship under investigation since it is entirely plausible that the referendum did not lead to an increase in the occurrence of hate crimes but only reporting of them. Since it is plausible that immigrants and ethnic or religious minorities felt threatened due to the referendum, they may have been more likely to report as crimes things they would not have reported otherwise. Similarly, this may have led to an increase in media coverage of hate crimes, which further primed individuals to report such crimes. Whilst this cannot be entirely ruled out, I address this by applying vector autoregression models to media coverage of hate crimes as well as the data used for the primary analysis. These analyses, presented later, show no evidence of this type of relationship. In addition, previous research has suggested there are significant attitudinal changes to minorities following events which can lead to hate crimes (Legewie, 2013; King *et al*, 2013). Finally, the Home Office conducted an investigation and validation of the data sources and confirmed its quality (O’Neill, 2017, 23-24).

In addition, this particular data set does not include attacks against White British people. Whilst these are identified as hate crimes, they are not within the remit of this particular data collection. This is quite useful as far as this analysis goes, since it rules out the possibility that hate crimes were driven by a ‘backlash’ effect on White British people from groups that felt under threat.

Model Specification

The data is modelled through the use of Box-Tiao time series intervention models, which is a quasi-experimental method that estimates the impact of ‘shocks’ on a time series. This method is effective at drawing inferences not only about the causal impact of particular events, but allows one to control for the dynamic components of a time series; in other words, the ‘inherent trends, persistence and random fluctuations of a variable over time’ (Jennings, 2010). Put formulaically, the basis of a time-series intervention model is:

$$Y_t = S(t) + I(t)$$

Where the outcome, Y_t is the time series at time t , $S(t)$ is an ARIMA model of the stochastic component, and $I(t)$ is the transfer function (i.e the intervention of interest) (Hay and Mcleary, 1979). Specifying these models in order to generate a causal interpretation requires a focus on both of these parts. The stochastic component can be modelled following identification of the statistical qualities of the time series using (partial-) autocorrelation plots, whilst the intervention is a matter of theory as well as post-estimation diagnostics of the model. This is the ‘identification/estimation/diagnoses’ approach proposed by Box and Jenkins (1970). I focus here on the specification of the

intervention, and leave discussion of the statistical identification and diagnostics for the Appendix, though I address some of them in the Results section.

The simplest specification of an intervention is either as a 'step' or 'pulse' function. For a step function, the intervention is a binary variable, specified at 0 until the intervention and 1 thereafter; pulse functions, on the other hand, are 0 until the intervention and 1 for a specified, limited time after the event. The literature on discrete events and hate crime has argued that events cause 'an immediate increase in hate crimes [...] followed by a fast rate of decay' (King and Sutton, 2013), which recommends the use of a pulse intervention. In addition, a step function would also be unreasonable since it would imply that the referendum had a persistent, consistent effect throughout the remainder of the time series. However, beyond this guidance, the length of time for which a pulse function should be specified in this case is unclear. Since there is no theoretical reason to favour one over the other, I follow King and Sutton (2013) and consider a range of interventions. For the monthly data, I specify a pulse function in June (the month which the referendum occurs), the month after (July) and a two-month pulse (i.e June and July together). The daily intervention is more problematic since any number of days could be specified. The primary model presented is for a week following the referendum, from the 24th June until the 1st of July. The first day is the day after the referendum, since it is not expected that the mere holding of the ballot increased hate crimes, but the result. The alternative models range from two weeks after the referendum to just 5 days after the referendum. Whilst this is not a perfect identification strategy, there is no theoretical basis that seems more reasonable. Moreover, all of these interventions are presented in the results section to indicate how results may change with different specifications, increasing the transparency of the analysis and breadth of evidence.

For both the daily and monthly data, other events in the time series are also potentially relevant. These are included as additional independent variables. Informed by previous research on hate crimes, I create binary pulse variables of the same duration as the dependent variable for terror attacks throughout the time series: the murder of Jo Cox (16th June 2016), the Westminster attack (22nd March, 2017), the Manchester attack (22nd May, 2017), the London Bridge attack (3rd June, 2017) and the attack on the Finsbury Park mosque (19th June, 2017). In other words, for the daily data, this is a week from the data specified; for the monthly data, it is the month in which the attack occurred (so, for instance, the London Bridge and Finsbury Park attacks take the same variable as they both occur in June). In addition, since the monthly data is over a longer period of time, I control for the additional events of the murder of Lee Rigby, the Charlie Hebdo attacks and the Paris attacks. Though clearly different to these events, I also include the Israel-Gaza conflict, since previous research has indicated this increased anti-Semitic attacks (Jacobs *et al*, 2011). I also control for the salience of immigration using Ipsos Mori's 'issue index', which is the percentage of respondents identifying immigration as the most important issue facing the country in any given month. There is evidence that media effects and coverage can drive support of right-wing parties and anti-immigrant rhetoric which are quite plausibly related to hate crimes (Esser and Brosius, 1996; Walgrave and Swert, 2004; Boomgaarden and Vliegenthart, 2009; Vliegenthart and Boomgaarden, 2010). Finally, in the daily data, I also include a control for the month since hate crime (and crime more generally) is seasonal. In the monthly data, this is adjusted through a dummy variable for each month and taking the first difference of the dependent variable.

Results

Daily Data

Turning to the results, Figure 1 plots the descriptive time series of the available daily hate crime data, where the vertical dashed line represents the occurrence of the referendum. There is a clear, contemporaneous impact of the referendum result on the time series, equivalent to those of the Manchester and Finsbury Park terrorist attacks, the two peaks in the final quarter of the time series. In between the referendum and the terror attacks, hate crimes fall to pre-referendum levels and lower.

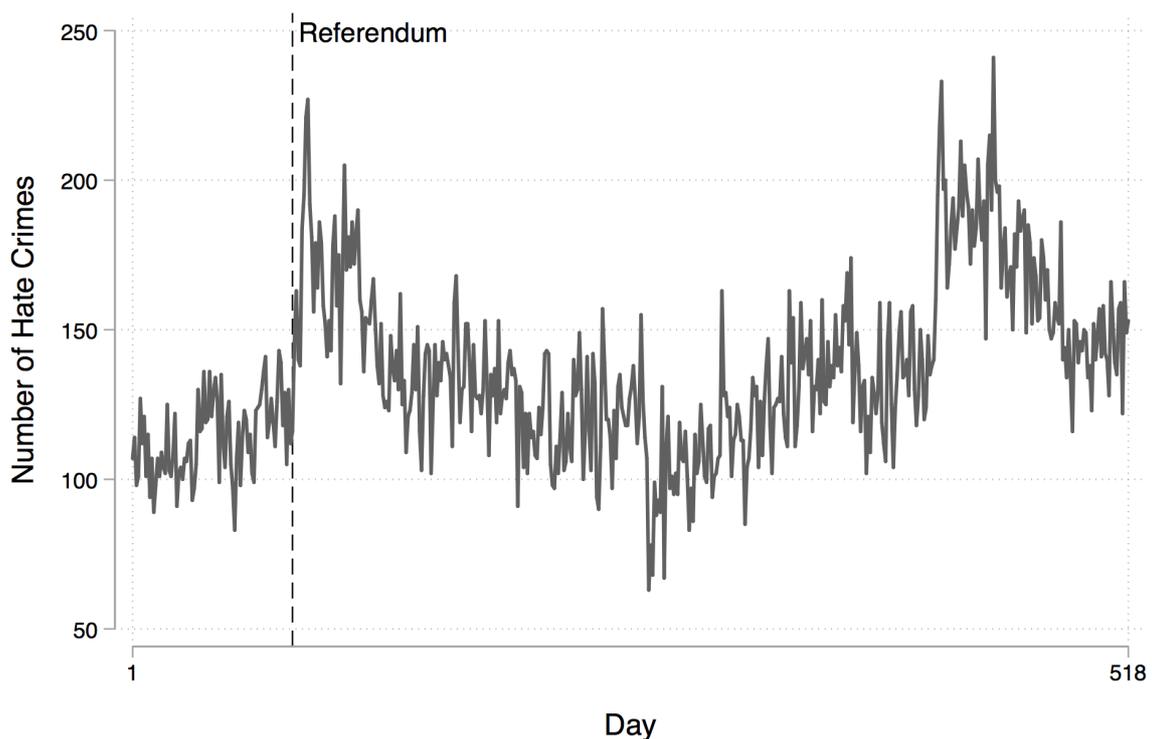


Figure 1: Time Series Plot of the Daily Data on Hate Crimes

Table 1 displays the coefficients of the significant interventions on the daily hate crime time series as well as the noise components and diagnostics. A full model containing the non-significant events and diagnostics is in the Appendix. A Dickey-Fuller test indicated that there was no unit root process, so the time series is unadjusted and is straightforward to interpret. The model shows that the referendum, coded as a week-long pulse, led to an increase of approximately 31 hate crimes a day. This is an increase of 23% on the average of 125 a day. What is striking about this is that this is a *larger* increase than even the Manchester and Finsbury Park terror attacks, of 21 and 23 respectively. This is quite surprising given what we know about the significant increases in hate crimes in the wake of terror attacks (Hanes and Machin, 2014; Legewie, 2013). Yet, research into the effect of the election of Trump indicates that his election increased hate crimes 25 times more than terror attacks in the US, except for 9/11 (Rushin and Edwards, 2018).

Table 1: Estimates and Diagnostics from a Time Series Intervention Model on Daily Data

X	Date	Hate crimes (Y)
Referendum	24/06/2016- 01/07/2016	31.11*** (7.68)
Finsbury Park Attack	19/06/2017- 26/06/2017	23.67*** (8.84)
Manchester Attack	22/05/2017- 29/05/2017	21.14*** (6.7)
<i>Noise Components and Diagnostics</i>		
ARIMA		(2,0,1)
Autoregressive (L1)		1.17*** (0.069)
Autoregressive (L2)		-0.18*** (0.064)
Moving Average (MA)		-0.78*** (0.047)
Mean		133.23 (9.94)
Sigma		16.17 (0.47)
Durbin-Watson d Statistic		1.997
Observations		518

*** p<0.01, ** p<0.05, * p<0.1

Table 2 presents a comparison of the different referendum interventions using the same model as presented in Table 1, along with the AIC/BIC values. This adds some nuance to the story. All specifications of 5 days and under are insignificant (with the possible exception of a 3-day pulse, which is significant at the 10% level). This is consistent with the delay between events and increases noted in other research. However, it is worth noting that the longer interventions of eight days and two weeks have a lower AIC and BIC value, indicating that statistically they are a better fit than the more instantaneous intervention, and that these coefficient estimates are in agreement with the week-long intervention.

Table 2: Intervention Comparison for Daily Data

X	Date	Hate crimes (Y)	AIC/BIC
Week Intervention	24/06/2016- 01/07/2016	31.11*** (7.68)	4382/4416
5 Day Intervention	24/06/2016- 29/06/2016	4.64 (5.00)	4382/4416
6 Day Intervention	24/06/2016- 30/06/2016	17.13*** (5.23)	4379/4413
8 Day Intervention	24/06/2016- 02/07/2016	32.06*** (7.96)	4369/4403
2 Week Intervention	24/06/2016- 07/07/2016	30.02*** (6.82)	4370/4404

*** p<0.01, ** p<0.05, * p<0.1

Monthly Data

Figure 2 plots the monthly data. Since there is clear seasonality, and a Dickey-Fuller test indicates that there is non-stationarity, I take the first difference of the time series and include dummies for the months in the analysis (not presented). A Dickey-Fuller test on the differenced variable confirms stationarity.

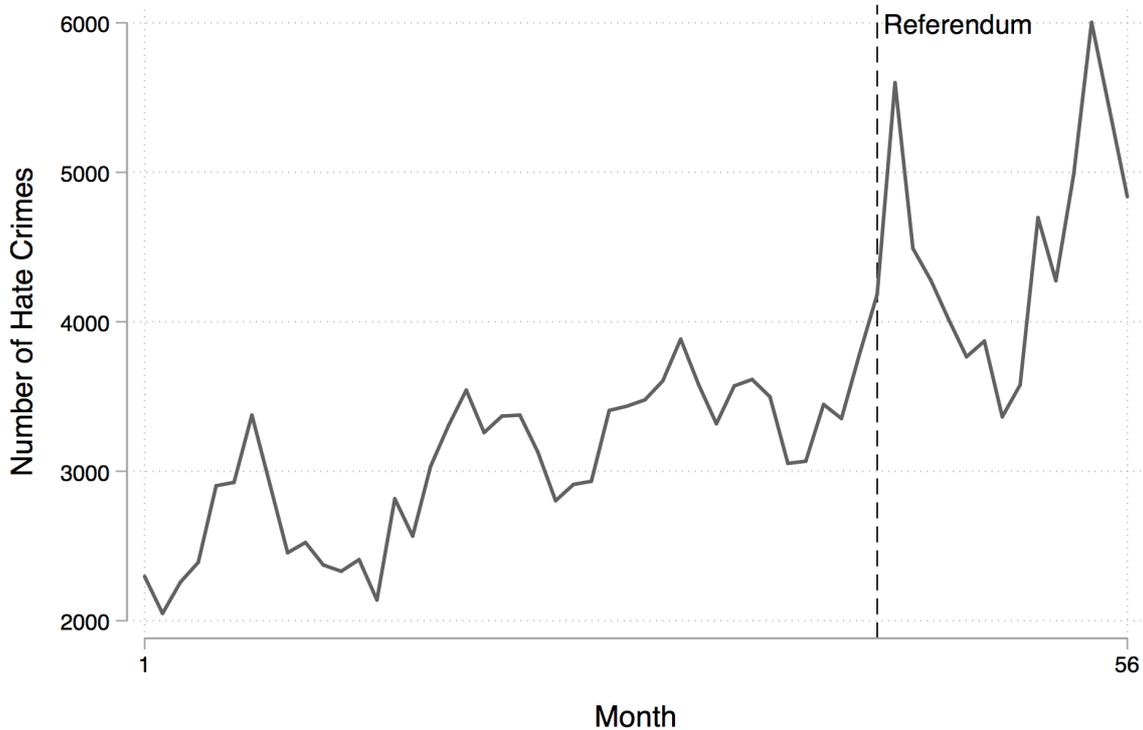


Figure 2: Time Series Plot of the Monthly Data on Hate Crimes

Results of the significant interventions are presented in Table 3. However, a note of caution should be made. All of the significant interventions except the referendum occurred in the last five months of the time series, a time series which is relatively short

(N = 55), and the last months of the time series are months in which we would expect higher hate crimes given the seasonal trend. As is indicated in the residual analysis in the Appendix, this poses problems for interpretation of these last interventions. However, the intervention of interest, the referendum, occurs 14 months before the end of the time series, and can be interpreted with more confidence.

The results are consistent with the daily data with regard to the referendum intervention, showing a positive and highly significant effect at the $p < 0.01$ level. Substantively, the model estimates that the referendum led to an increase of 638 hate crimes, approximately a 19% increase on an average of 3425 hate crimes in a month. A similar effect is found for the Finsbury Park and London Bridge attacks, with 740 and 831 crimes respectively. Unlike the analysis of the daily data, however, the Westminster attack is significant whilst the Manchester attack is not. Until the release of more data, this should be interpreted with caution given the limited amount of observations following these attacks. Overall, even controlling for the seasonality and trend of hate crime occurrence, the month in which the referendum was held is a significant shock on the time series to a similar magnitude as terror attacks, which are commonly linked in the literature to hate crime occurrence. An additional, full model in the Appendix shows these coefficients alongside other events and the salience of immigration which were not significant.

Table 3: Estimates and Diagnostics from a Time Series Intervention Model on Monthly Data

X	Date	Hate crimes (Y)
Referendum	June 2016	638.32*** (243)
Finsbury Park/London Bridge Attack	June 2017	740.31** (344.41)
Westminster Attack	March 2017	831.85** (434.20)
<i>Noise Components and Diagnostics</i>		
ARIMA		(1,0,0)
Autoregressive (L1)		-0.41** (0.19)
Mean		-605.68
Sigma		237.10 (28.10)
Durbin-Watson d Statistic		2.04
Observations		55

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

As in the previous analysis, alternatives to this intervention are specified in Table 4. I show the June 2016 intervention (the one in the presented model), one for the following month, and one for the two-month pulse. These comparisons show that the model specified is a conservative estimate of the impact of the referendum, and that other sensible interventions point in the same direction. To test to make sure this was not a statistical artefact, I included an intervention in the month before the referendum, which was insignificant.

Table 4: Intervention Comparison for Monthly Data

X	Date	Hate crimes (Y)	AIC/BIC
June Intervention	June 2016	638.32*** (243)	803/849
July Intervention	July 2016	1266.19** (515.51)	783/829
Two Month Intervention	June and July 2016	707.99*** (125.32)	788/835

*** p<0.01, ** p<0.05, * p<0.1

Overall, the results provide strong evidence that the referendum led to a statistically significant increase in hate crimes, largely irrespective of the length of intervention chosen and controlling for other significant events, the salience of immigration, and the stochastic components of time series. What is most striking is that, in the daily data, this is an even larger increase than significant terror attacks, though that is not borne out in the monthly data. Alongside this, the analysis shows that the Finsbury Park attack led to a significant increase in hate crimes, whilst there is conflicting evidence regarding the Westminster and Manchester terror attacks which should become clearer upon the release of new data.

Addressing the media-hate crime nexus

The dangers to causal inference noted here are twofold: since these data are reported hate crimes, it is possible that media priming leads people to report more often or perceive attacks as hate crimes where they otherwise would not; this could then lead into a spiralling effect, where reporting and media coverage reinforce each other. In other words, media coverage surrounding hate crime may have led to an increase in reported hate crimes independent of any actual increase in offences.

To address this, I use a vector auto-regression (VAR) analysis with Granger causality tests on the *media coverage* of hate crimes and the daily data used in the previous analysis. The coverage of hate crimes is collected through a Nexis search of “hate crimes” in all UK national newspapers a month either side of the referendum, meaning the time series extends from 23rd of May to 24th of July. This provides a daily time series of the number of articles mentioning hate crime, which can be matched with the daily time series of reported hate crimes. Whilst there has been some criticism of measuring media coverage in this way, it is a valid measure of the general contours of media coverage (Althaus *et al*, 2001), particularly since the objective here is to measure the quantity of coverage rather than content (Boomgaarden and Vliegenthart, 2007). VAR analysis with Granger causality tests is also ideal for this purpose. VAR models disentangle the causal ordering of two time-series variables (in this case, coverage and hate crimes). This analysis provides evidence for the hypothesis that coverage leads to reported hate crimes or of a spiralling effect if it is found both time series are mutually reinforcing.

The optimal lag length is VAR(4), as determined by the Akaike Information Criterion for the dependent variable. Whilst a Portmanteau test for white noise indicates that errors are not serially correlated, a White test for residual heteroskedasticity indicates that a log transformation of the variables is necessary to reduce heteroskedasticity. The diagnostics on the final models indicate appropriate model fit and that there is also no evidence of temporal instability. The results of these VAR models are presented below. Since the variables needed to be log-transformed, the coefficients are now interpreted as elasticities: in other words, the coefficients represent the expected change in the dependent variable from a one percent change in the independent variable. Along with the two dependent variables, I include two exogenous predictors: a variable indicating the week of the referendum and the week of the murder of Jo Cox.

Table 5: Vector Autoregressions for coverage of hate crime and reported hate crimes

X	Δ Hate Crimes	Δ Articles
Δ Hate Crimes _{t-1}	0.32** (0.13)	0.28 (0.95)
Δ Articles _{t-1}	-0.014 (0.02)	0.62*** (0.14)
Δ Hate Crimes _{t-2}	0.08 (0.14)	-0.46 (0.99)
Δ Articles _{t-2}	0.038 (0.024)	-0.11 (0.16)
Δ Hate Crimes _{t-3}	0.08 (0.13)	0.79 (0.94)
Δ Articles _{t-3}	0.007 (0.75)	0.06 (0.17)
Δ Hate Crimes _{t-4}	0.13 (0.12)	0.87 (0.85)
Δ Articles _{t-4}	0.02 (0.22)	-0.07 (0.15)
Referendum	0.15*** (0.04)	0.58* (0.33)
Jo Cox	-0.11** (0.05)	0.23 (0.39)
Observations	59	59
R ²	0.77	0.59

*** p<0.01, ** p<0.05, * p<0.1

The results show no evidence that changes in hate crime coverage are related to reported hate crimes, nor that hate crimes are related to hate crime coverage. As in the first set of analyses, the week of Brexit is still a highly significant predictor of hate crime, and significant at the 10% level for media coverage of hate crime, providing some evidence that the referendum increased hate crime coverage in national newspapers. The Granger causality tests reflect this interpretation, with neither time series Granger-causing the other. Interestingly, the results indicate that the murder of Jo Cox reduced hate crimes by 11%, and this is significant at the 5% level. Whilst this was not the case in the first analyses, it is notable that the direction of the coefficient is the same. I also ran the models excluding the murder of Jo Cox, which may have potentially masked other effects due to its occurrence just a week before the referendum. The results did not change on this exclusion. These results provide strong evidence that hate crime coverage did not lead to

an increase in reported hate crimes and, therefore, nor that there was not a mutually reinforcing effect between coverage and reporting. Orthogonalised impulse response plots of the results are presented in the Appendix, along with diagnostic plots.

Discussion

Did the referendum on membership of the European Union lead to an increase in hate crimes? This was debated across the political spectrum, in national newspapers, the United Nations, and the Houses of Parliament. This paper has provided an important and rigorous empirical answer by applying time series intervention models to daily and monthly data. The results indicate that the referendum acted as a 'trigger event' for hate crimes in both daily and monthly data, perhaps even more than the Manchester and London terror attacks. Since the dependent variable is *reported* hate crimes, the paper also examined whether the media played a role in artificially increasing reporting independent of actual attacks. It was found that the increase in hate crimes is not reducible to media coverage of hate crimes, with vector autoregression models providing no evidence for a spiralling or media priming effect. The results also show that terror attacks have a large and significant impact on hate crimes.

These results provide valuable theoretical insights into the causes of hate crimes. The literature has struggled with the causal mechanisms linking events, such as terror attacks, with an increase in hate crimes. Primarily, this difficulty has been about whether there is a backlash effect following these events or whether it is the role of media and elites in either generating negative stereotypes or legitimating views following an event. Since the data used in this paper does not include attacks against White British people, and the event itself is less likely to cause a backlash effect than a terror attack, it reduces the plausibility of the backlash hypothesis. Rather, the focus turns to the role of media coverage and elite behaviour following an event. This is consistent with research from Hanes and Machin (2014). However, whether it is about a legitimisation of views following an event or 'shifts in underlying bigotry from attitudinal change following events' (Hanes and Machin, 2014, 263) is hard to say, since both are plausible mechanisms in this case.

Nonetheless, the extension of these theories to a new case is independently important. For one, it provides significant evidence to the literature that focuses on the role that discrete events play in the causation of hate crimes, even outside of terror attacks. Secondly, it helps arbitrate between the theoretical mechanisms linking these events with hate crimes. Finally, there is an ongoing committee in the UK Home Office dedicated to exploring 'hate crime and its violent consequences', and so this analysis provides a robust analysis of a topic of ongoing concern.

The next step for research in this area should be to extend the evidence base on the underlying mechanisms. The academic literature on events and hate crimes has studied elections (King and Brustein, 2006; Rushin and Edwards, 2018), terror attacks (Hanes and Machin, 2014; King and Sutton, 2013), and actions of political elites (Karapin, 1999), and has done so with robust methods for causal inference (Green and Spry, 2014; King and Sutton, 2013; Borell, 2015; Disha *et al*, 2011). As such, the evidence that discrete events are linked to an increase in hate crimes is broad and generalisable. The missing step is an extensive analysis of the underlying mechanisms, and whether they are the

same between events. For instance, it is quite plausible that whilst elections, referendums and terror attacks lead to a similar increase in hate crimes, the mechanism is different.

What are the wider implications of this paper? Firstly, if future research shows that the mechanism is media or elite coverage of the event, there should be more critical attention paid to the role of the media in society. This is particularly urgent if one considers these findings in the context of the vast literature on the role of the media in shaping support for far-right parties or bolstering prejudicial attitudes (Walgrave and Swert, 2004; Boomgaarden and Vliegenthart, 2009; Vliegenthart and Boomgaarden, 2010). Secondly, the paper sheds some light on the social consequences of the referendum. Whilst academic work has explored a diverse range of topics in relation to Brexit, such as its psychological underpinnings (Swami *et al*, 2018), individual-level drivers (Goodwin and Milazzo, 2017; Clarke *et al*, 2017), and long-term socio-economic causes (Jennings and Stoker, 2017), the consequences have largely been restricted to technical issues or the explicitly 'political' ramifications, such as the unity of the United Kingdom (McEwen, 2017) or the European Union (Hobolt, 2016). One exception is Menon (2018), who addresses other social consequences.

This paper has provided a significant contribution to the evidence base on political events and the occurrence of hate crimes. It seems now to be a key turning point in which the unpacking of the underlying causal mechanisms is necessary to advance the field. This paper has suggested some of the more plausible avenues of further research whilst contributing to a relevant and contemporary problem of social significance.

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