Stockholm School of Economics
Department of Economics
5350 Master's Thesis in Economics
Academic year 2022–2023

Love Thy Neighbor: Hate Crime in a Time of Crisis

Anna Hagman (42 118) and Jessica Stefansson (24 311)

Abstract. This paper explores whether the increase in hate crime in Sweden following the Migrant Crisis in 2015 can be explained by changes in ethnic composition or income inequality. We use an event study methodology and data from Swedish municipalities to test how well these structural factors, supported by theory, can explain hate crime rates. Our analysis empirically confirms that hate crime per 10 000 citizens increased in 2015 in conjunction with the crisis, indicating that changes in ethnic composition played a role in the increase. However, our results do not show any evidence that more drastic changes in ethnic composition would lead to higher hate crime rates, as all municipalities in our sample initially respond the same to the inflow of immigrants. Instead, our findings suggest that greater changes in the share of foreign population have a dissipative effect on hate crime in 2016. When we split the sample based on the degree of income inequality, we discover that the increase in hate crime rates is only present among municipalities with high income inequality. This implies that income inequality could be considered one of the economic conditions conducive to hate crime. Our findings provide interesting insights regarding the complex relationship between ethnic composition, income inequality, and hate crime in Sweden, highlighting the need for further research in this area.

Keywords: hate crime, immigration, income inequality

JEL: J15, K42, R23, O15,

Supervisor: Ulrika Ahrsjö
Date submitted: 05-12-2023
Date examined: 05-23-2023
Discussant: Benjamin Leopardo
Examiner: Karl Wärneryd

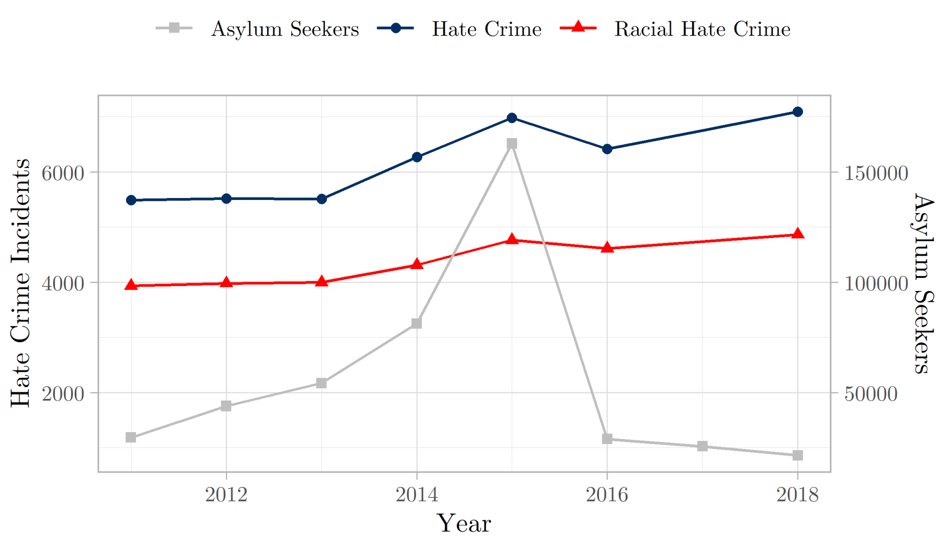
Acknowledgements

We would like to express our deepest appreciation to our thesis supervisor, Ulrika Ahrsjö, for her invaluable guidance, support, and feedback throughout the writing of this Master thesis. Furthermore, we are very grateful for the time and effort that Ulrika has devoted to reading and reviewing our work. Her mentorship has been invaluable and without her insight and knowledge, this thesis would not have been possible.

1 Introduction

The number of hate crimes reported in Sweden has increased in recent years (see Figure 1). In 2018, the Swedish National Council for Crime Prevention (BRÅ) estimated that 7 090 hate crimes were reported, 4 865 of which were deemed of a racist nature.[1] This constitutes a 29% increase from the number of reported hate crimes in 2011. Hate crimes have both social and economic effects, for example, by impairing immigrants' willingness to integrate and through the creation of social divide (Entorf & Lange, 2019, Dustmann et al., 2011). The social exclusion of minorities comes at the cost of a reduction in GDP and a loss of human capital (Noel et al., 2019, Friedberg, 2000). Furthermore, it contributes to social strain, which in the long run, can result in violence (Agnew, 2001). Thus, finding ways to prevent hate crimes should be on the policy agenda. Although a lot of research has been conducted on why people commit hate crimes and the characteristics of such perpetrators, the impact society has in forming such individuals remains ambiguous. What has changed in Swedish society, that lead to the observed increase in hate crimes?

Figure 1. Hate Crime and Asylum Seekers in Sweden



Note: The number of reported hate crimes, hate crimes of a racist nature and asylum seekers (not reported in 2017). Statistics taken from BRÅ's website https://bra.se/statistik/statistiska-undersokningar/hatbrottsstatistik/hatbrottsstatistik-2008-2018.html and the Swedish Migration Agency https://www.migrationsverket.se/Om-Migrationsverket/Statistik/Asyl.html [Accessed 2023-03-22]

[1] Statistics are taken from BRÅ's hate crime report 2018 https://bra.se/download/18.bbb8316de12eace227048/1614334407813/2019_13_Hatbrott%20_2018.pdf [accessed 2023-04-04]

In 2015 Europe experienced a large inflow of refugees, from here on referred to as the Migrant Crisis. The situation resulted in Sweden receiving twice the amount of requests for asylum in 2015 (162 877) compared to the previous year (81 301).[2] Furthermore, the Migrant Crisis had a polarizing effect on the political views on immigration in Sweden and made many people grow critical of Sweden's generous immigration policy (Esaiasson et al., 2016). As shown in Figure 1, hate crime began to rise in conjunction with the Migrant Crisis and reached a peak in 2015. We hypothesize the Migrant Crisis as a catalyst for the expression of greater anti-immigrant sentiment in Sweden, and ask ourselves to what extent hate crime depends on changes in the share of immigrants in the population?

This paper explores the link between hate crime and social and economic conditions in Sweden. Specifically, we examine whether the observed increase in hate crime following the Migrant Crisis can be explained by changes in ethnic composition or the degree of income inequality. For our analysis, we utilize municipality-level data on hate crime from BRÅ to capture the heterogeneity of hate crime rates throughout Sweden. An event study design is employed to estimate the effect of drastic changes in ethnic composition, or "immigrant shocks", on hate crime. Thereafter, we split the sample into two groups characterized by high and low income inequality and estimate two separate event studies. This split allows us to infer if economic conditions has any influence on inhabitants' reaction to the inflow of immigrants.

Our analysis can empirically confirm that hate crime increased, within the 59 Swedish municipalities in our sample, during the peak of the Migrant Crisis. This finding emphasizes the importance of changes in ethnic composition for explaining variations in hate crime rates. However, our results provide no evidence that it is the size of the compositional change, caused by the Migrant Crisis, that leads to the heightened hate crime rates. Instead, our event study shows the possible presence of a dissipative effect the year following the crisis for the municipalities that experienced the most significant compositional changes. Furthermore, our analysis indicates that income inequality impacts which municipalities displays higher hate crime rates during this period. We also find that the degree of income inequality may influence which municipalities experience the dissipative effect associated with the "immigrant shock" outlined above. Additional research is needed to determine whether our results are applicable across other Scandinavian or European countries, as its not certain that hate crimes are driven by the same structural factors across nations. There exists expected differences in the predisposition

[2] The number of total asylum seekers is found in Statistics Sweden's Database: https://www.statistikdatabasen.scb.se/pxweb/sv/ssd/START__BE__BE0101__BE0101P/AsylsokandeN/ [accessed 2023-03-17]

to use violence and in the history of ethnic oppression between countries. Hence, while there exist similar issues of higher hate crime rates following the Migrant Crisis within the European Union (European Union Agency of Fundamenal Rights, 2016), it is hard to argue that responses to the inflow of immigrants would closely resemble those of Sweden. Furthermore, there even exists uncertainties regarding if our results would generalize to the other Swedish municipalities omitted from our sample due to lack of data.

But what drives a person to commit a hate crime? We focus our research on two theories that link structural factors such as, changes in ethnic composition and income inequality to hate crime. They are the defended neighborhoods theory and the strain theory. Green et al. (2001) examine both behavioral and contextual factors for why prejudice may erupt into hate crime. They highlight the defended neighborhoods theory as broadly applicable and capable to explain many of the surges in hate crime observed to date. According to this theory hate crimes are more prevalent when the ethnic composition of an area has changed drastically. As others in the literature, our analysis is designed to assess the role of changing ethnic composition and the plausibility of the defended neighborhoods theory explaining hate crime rates (Green et al., 1998b, Entorf & Lange, 2019, Grattet, 2009). Hate crime literature, at times, also attribute hate crime to social strain. The strain theory, originally presented by Merton (1938), describes how economic conditions can generate criminal incentives. Hate crime can in this context be explained by the inability to reach certain goals, created by social construct, being blamed on minority group members (Walters, 2011).

Our paper contributes to an area of research that, to the best of our knowledge, has not been broadly examined in a Swedish context. Namely, hate crime and the explanatory value of the structural factors specified by the defended neighborhoods theory and the strain theory. Furthermore, we can empirically confirm that there has been a rise in hate crime rates at the municipality-level, coinciding with the Migrant Crisis in Sweden. We can also add high income inequality to one of the economic conditions conducive to hate crime during the Migrant Crisis.

The structure of this paper is organized as follows; Section 2 discusses the related literature and the theory used; Section 3 presents the methodological approach; Section 4 contains the results of our empirical analysis; Section 5 discusses the implications of the results; Section 6 concludes.

2 Literature Review

Several researchers have explored the nature of criminal activity and the social structures conducive to it such as Merton (1938), Becker (1968), Kelly (2000) and Hipp (2007). Research has also been done on the case of Sweden by Nilsson (2004) and Lindgren (2019). However, hate crime is a far newer area of research. The term hate crime was, in fact, not coined until the 1980's (Green et al., 1998b). Furthermore, there exists no global definition of what constitutes a hate crime. As a result, studies made are not necessarily comparable across countries. In this section we explore existing research on the structural factors underlying hate crimes, and in Section 2.1 we discuss the theoretical models of hate crime considered when constructing our study.

Existing research on the relationship between ethnic composition and hate crime is largely unanimous that there is a connection between the two (Green et al., 1998b, Entorf & Lange, 2019, Grattet, 2009, Dustmann et al., 2011). However, there are many different theories and hypotheses underlying their correlation. Green et al. (1998b) explore these in their paper, ultimately finding evidence consistent with their hypothesis of a defended neighborhood. More specifically, they find hate crime to be increasing with the inflow of ethnically diverse people into Caucasian majority neighborhoods or what they refer to as "White strongholds", and to be falling where non-White has resided for some time. The idea is that hate crime is an attempt to keep these newcomers out and to preserve shared social identity which is threatened e.g. by the introduction of new social structures. Green et al. (1998b) performed their study on data from New York City but others such as Entorf & Lange (2019) and Grattet (2009) find comparable evidence of the defended neighborhoods theory from Germany and Sacramento, respectively. Similar to our paper Entorf & Lange (2019) explores the relation between demographic changes and surges in hate crime related to the Migrant Crisis. Entorf & Lange (2019)'s county-level analysis finds evidence that it is not simply the number of immigrants but the rapid compositional change of the residential population that drives the increase in hate crimes against asylum seekers. Grattet (2009) describes neighborhood defenses as something that doesn't arise in all communities, and similar to Green et al. (1998b) highlights the predisposition of predominantly Caucasian neighborhoods to have defended neighborhood responses. Related, but not connecting their results to the defended neighborhoods hypothesis, is Dustmann et al. (2011) on British data. They find that racial harassment is less common in areas of high ethnic concentration even though hostility on the side of the majority population is the same.

Within the social sciences, hate crime is commonly being attributed to competition for resources and economic downturns, but evidence linking the two phenomena is ambiguous. A large body of research has evaluated the connection between economic conditions and hate crime, such as Green et al. (1998a), Green et al. (1998b), Dustmann et al. (2011), Entorf & Lange (2019), and Gale et al. (2002), but results are mixed. Although both Green et al. (1998a) and Green et al. (1998b) conclude that economic conditions do not affect hate crime rates, other studies such as Entorf & Lange (2019), Dustmann et al. (2011), and Gale et al. (2002) suggest that poor economic conditions may be conducive to hate crime. All authors, except Gale et al. (2002), use unemployment rates as their main indicator of economic hardship. Although Green et al. (1998b) primarily use unemployment rates as their main indicator, they also examine some other measures such as poverty rates and median wages in an attempt to establish a connection, but ultimately cannot find a plausible relationship between the two. Gale et al. (2002) use a broader set of economic indicators in their analysis, additionally including per capita and relative income to unemployment rates. They find that the relative income of Blacks to Whites to be of importance for the prevalence of hate crime. Dustmann et al. (2011) find that poor economic conditions are conducive to instances of racial harassment. Entorf & Lange (2019) explores the effect of the inflow of immigrants during the Migrant Crisis on hate crime in areas that are already riddled with economic hardship. Their analysis shows that economic conditions may not be crucial in explaining hate crime, but may hold explanatory value in areas simultaneously experiencing a large inflow of immigrants.

These authors, with the exception of Gale et al. (2002), have focused on economic hardships but there are other economic conditions conducive to crime examined in the broad crime literature. Merton (1938) lays forth income inequality as a determinant of criminal activity, that is the whole income distribution rather than only the left tail considered above. This relationship has also been studied by Kelly (2000) and Hipp (2007), which both find a relationship between the two. Hipp (2007) does so even when controlling for economic hardship. There is also evidence from Sweden, Nilsson (2004) and Lindgren (2019) both find a connection between income inequality and different types of criminal activity. Gale et al. (2002)'s findings and the evidence from the crime literature motivates us to better understand the explanatory value of income inequality in a hate crime setting.

2.1 Hate Crime Theory

The classical model of crime prevention presented by Becker (1968) can be extended to also analyze hate crime. Becker models crime as a choice between legal and illegal activities based on the chance of financial gain and the risk of punishment. Individuals behave rationally and will thus only commit a crime if it pays off to do so in the context of their utility function. Perpetrators are in Becker's model indifferent towards the welfare of their victims. Here is where hate crime fundamentally differs from Becker's modeling of crime [3] Hate crime perpetrators do not necessarily only care for financial gain but also derive utility from inflicting harm upon their victims. The utility function of hate crime perpetrators are dependent on their victims welfare (Gale et al., 2002). The animosity that the offender has towards the victim has several potential causes and is constantly under debate in the literature. In this section we elaborate upon two theories that try to explain the occurrence of hate crime using structural factors.

2.1.1 Defended Neighborhoods Theory

The concept of a defended neighborhood was presented by Suttles (1972) and describes the collective action communities take to protect it from change. A defended neighborhood is a neighborhood which residents identify, through defined borders, as an area that is geographically separate and socially different to adjacent ones. Residents of a defended neighborhood will have a strong shared identity and take actions against perceived threats (Suttles, 1972). The shared identity of a defended neighborhood is the most critical part when it comes to explaining the occurrence of hate crime. Threats to the collective identity may be posed by newcomers entering the neighborhood challenging status quo and imposing new social structures. In an attempt to "defend" collective identity residents may commit hate crimes to ward off these newcomers. The notion of a defended neighborhood would lead to higher hate crime rates following times where the ethnical composition of an area has changed drastically. Neighborhoods of predominantly the same ethnicity are expected to respond more forcefully to newcomers as their race are more strongly connected to their identity. However, the heightened hate crime rates are expected to be short lived and eventually dissipate as residents gradually accepts the newcomers as a part of the community (Green et al., 1998b).

[3] Becker definition covers acts such as murder, robbery, assault, tax evasion, white-collar crimes, traffic violations, etc. (Becker, 1968).

2.1.2 Strain Theory

The connection to economic conditions is in the literature sometimes made by referencing parts of strain theory. Strain theory was presented by Merton (1938), elaborated upon by Agnew (2001) and is commonly used in criminology (Walters, 2011). In this framework individuals strive after cultural goals, which in western capitalist societies often are connected to material gains. According to Merton these social structures can pressure individuals into committing crimes. He argues that for some individuals, the means of achieving these goals are impaired, e.g. they have no access to education or job opportunities. Hate crimes can in this context be explained as stemming from the perceived socio-economic instability of the perpetrator's own life, believed to be the fault of the victim (Walters, 2011). The blame can be rooted in some of the common claims made about immigrants, such that they receive more social benefits and steal jobs from native citizens (Arbetsmarknadsdepartementet, 2013). When these minority group members enter perpetrators' social sphere they are seen as competition and become targets for pent up frustrations related to achieving these cultural goals. However, those already relatively well off may also target or discriminate against minority groups to ensure their socio-economic standing in the future (Walters, 2011). The perceived threat of newcomers will in the context of strain theory depend on the current income status and educational attainment of the majority group.

To conclude, the defended neighborhoods theory predicts that the inflow of immigrants during the Migrant Crisis would result in heightened hate crime rates, and that when the immigration wave has passed the levels should eventually dissipate. Moreover, the municipalities that experienced the greatest change in ethnic composition are expected to be the most hostile against the newcomers, as was found by Entorf & Lange (2019). We postulate income inequality as a source of strain, inducing differences in the perceived threat of newcomers. Following strain theory, we would thus expect municipalities with high income inequality to have higher hate crime rates.

3 Methodological Approach

In this section, we first present the data we use in our study, the sources, and how the different variables are constructed. We also elaborate on some of the shortcomings when it comes to our data collection. Second, we describe the empirical strategy and specification used for identification.

3.1 Data

For our analysis we utilize data on the number of hate crime incidents in Swedish municipalities. We chose municipality-level data as opposed to county-level data, because it allows us to highlight structural differences between municipalities with heterogeneous hate crime rates. The sample period for our analysis are the years 2011-2016 and 2018. The span of the sample is restricted due to availability of hate crime and income inequality statistics. The period does, however, still provide insight into recent developments of increasing hate crime as it includes some years before and after 2015. 59 out of Sweden's 290 municipalities enter into our dataset, these are chosen based on the availability of hate crime statistics. A list of the municipalities included in our sample is found in Table A.1 in the Appendix and their location together with the prevalence of hate crime are visualized in Figure A.1. Furthermore, lists of the municipalities with the highest and lowest hate crime per 10 000 citizens are provided in Table A.2.

3.1.1 Hate Crime

There are several difficulties associated with working with data on hate crime, e.g. difficulties with measurement and the absence of a universally accepted definition. To perform our analysis we need to comprehend what is considered a hate crime in Sweden. We turn to definitions provided by Swedish institutions to gain this understanding. BRÅ, which is the provider of hate crime statistics in Sweden, states their definition of hate crime as follows;

> *Crime committed against a person, group, property, institution or representative of these, which is motivated by fear of, hostility or hatred towards the victim based on race, skin color, nationality or ethnic origin, creed, sexual orientation and gender identity or expression that the perpetrator believes, knows or perceives that the person or group has.*[4] - BRÅ

Looking at the legislation, the Swedish judicial system defines hate crime as all crimes prosecuted as incitement to hatred (hets mot folkgrupp), illegal discrimination and other crimes such as abuse, unlawful threats, destruction of property etc., executed with a discriminatory intent (Swedish Penal Code 16 c. 8§ and 9§, and 29 c. 2§ 7p (SFS

[4] Citation is translated from the following technical report accompanying BRÅ's 2018 hate crime report: https://bra.se/download/18.7d27ebd916ea64de5309e71/1614334417855/2019_14_Hatbrott_2018_Teknisk_rapport.pdf [accessed 2023-04-04]

1962:700)).[5] Most hate crimes are of a racist nature and predominantly take the form of unlawful threats or abuse. Because hate crime is constituted of multiple crime categories and lacks a singular criminal classification code (brottskod), it is not included in Sweden's crime statistics system (RAR).[6] Instead, the number of reported hate crimes in each municipality is estimated by BRÅ, and provided in yearly hate crime reports. Since 2016 these reports are only produced biannually, hence 2017 is excluded from our sample. In the year 2020 the directives for hate crime reporting changed and the time series is no longer comparable. Furthermore, BRÅ does not report statistics publicly for municipalities with less than 20 hate crimes due to the risk of identification and the inherent uncertainty of those estimations (Forselius & Westerberg, 2019). The 59 municipalities that enter into our sample exceed 20 hate crime incidents for at least one of the years prior to 2015, and one of the years that follows. Since statistics on hate crime is not reported in RAR, BRÅ uses a random sample of half of all crime reports, which they search for words associated with hate crime. Thereafter, they use a sub-sample of the reports containing hits to manually verify that each report accurately describes a hate crime incident. Based on these figures an estimate is made. The exact process and words which BRÅ uses to identify hate crimes from police reports are thoroughly explained in the technical report accompanying the data on BRÅ's website.[7]

We divide the estimated number of reported hate crimes by census data to get a per 10 000 citizens measure that is equalized across municipalities. This is similar to what Gale et al. (2002) use as the dependent variable in their analysis. The yearly census data on municipalities is retrieved from Statistics Sweden's website. We take the natural logarithm of the per capita measure to improve upon the normality of the data which is heavily positively skewed (see Figure A.2 in the Appendix).

There are several shortcomings related to the use of this data. First, we note that an increase in reported hate crime does not necessarily imply an increase in the actual occurrence of hate crimes. However, self reported hate crime has during the period 2005-2016 developed in a similar manner to the estimates made by BRÅ (Westerberg & Faramarzi, 2018). This suggest that the trend is not driven by increased reporting.

[5] Including crimes of violence, (assault, murder, attempted murder, assault against officer), unlawful threats, abuse, defamation, agitation against an ethnic or national group, destruction of property, unlawful discrimination and others. A list of all criminal classification codes (brottskoder) considered is found in Appendix 2 of the following technical report: https://bra.se/download/18.7d27ebd916ea64de5309e71/1614334417855/2019_14_Hatbrott_2018_Teknisk_rapport.pdf [accessed 2023-03-18]

[6] Police must however mark incidents suspected to be hate crimes when submitting a report to RAR.

[7] Search words are updated each year, the list for 2018 is found in Appendix 1 of the following report: https://bra.se/download/18.7d27ebd916ea64de5309e71/1614334417855/2019_14_Hatbrott_2018_Teknisk_rapport.pdf [accessed 2023-03-18]

Second, the preciseness of the hate crime estimates can be questioned as they are based on half of the available crime reports. How well this subset represents the total population could vary. Furthermore, the selection of hit words are subject to change, but how well these capture language used today is debatable with new derogatory slurs being invented ongoingly. Both are issues that impact the accuracy of the estimations made by BRÅ.

Third, the data is not exclusively on hate crimes that are racially motivated, though this is the main interest of this paper. If possible, we would have preferred to have municipality-level data on racial hate crime, to diminish the risk of capturing changes in hate crime unrelated to ethnic minorities such as sexual orientation. However, looking at the breakdown of hate crime motives on the national level during the period of interest, racially motivated, Islamophobic and other religious hate crime has shown the most growth Forselius & Westerberg (2019). These are all hate crimes that are closely related to ethnic minorities. Furthermore, there is a close relationship between the total hate crime and racial hate crime at a national level as seen in Figure 1.

Fourth and finally, BRÅ's censoring results in hate crime statistics being available for only 59 of Sweden's 290 municipalities. The municipalities that are omitted are mainly small and located in rural part of Sweden (see Figure A.1). This raises concerns about the representativeness of our data as well as the sample size. Additional evidence of the differences between our sample and the omitted municipalities is found in Figure A.3 in the Appendix. Access to quality data is something that is generally an issue in the field (Green et al., 2001), but if we compare our sample size to others it is still on the smaller side (Green et al., 1998b, Entorf & Lange, 2019). To make proper inference a larger sample size is favored, since a larger sample usually coincides with less margin of error and higher statistical power (Singh & Masuku, 2014). The censorship thus impacts both the precision of our estimates and the statistical power of our tests. Hence, it is clear that access to the uncensored hate crime data would have been preferred and improved upon the validity of our analysis.[8] However, the statistics provided by BRÅ are the best there are available for us to use. Thus, the issues highlighted above are all things that needs to be considered when interpreting our results.

[8] Professional researchers can access the uncensored data from BRÅ by paying for the production, passing a confidentiality review, gaining approval for their project from the Swedish Ethical Review Authority and complying with the rules of the General Data Protection Regulation. More about the requirements can be found here: https://bra.se/statistik/specialbestallningar.html [Accessed 2023-04-25]

3.1.2 Immigration

Our analysis is dependent on capturing the change in ethnic composition on the municipality-level associated with the Migrant Crisis. While Entorf & Lange (2019) use data on the assignment of asylum seekers to German counties to capture this element, there is no equivalent statistic available for Swedish municipalities. The asylum seekers in Sweden are not included in any municipality-level data until they are granted a residence permit and allocated to a municipality. More specifically, in the statistics on the number of immigrants welcomed by each municipality, reported by the Swedish Migration Agency. However, getting a residence permit is a lengthy process and does not necessarily capture the initial impact of the inflow of refugees that resided within Sweden during the Migrant Crisis. One could use the data on the distribution of residence permits, but that would not account for the internal migration that happens between different municipalities to live closer to family and friends. To measure the ethnic composition of the municipalities, we instead use census data on the number of individuals born to two foreign parents, which captures both new and migrating Swedish residents. We divide this number with the total population to get the share of foreign descent or immigrant share for each municipality. Both numbers can be found in the statistical database on Statistics Sweden's website. Since the degree of diversity before the inflow has also been shown to be of importance we include the immigrant share as a control in our regressions (Green et al., 1998b, Entorf & Lange, 2019). Note that only in two municipalities in our sample, Botkyrka and Södertälje, does immigrants constitute a majority share of the population. Hence, most of the Swedish municipalities can be seen as "White strongholds" were defended neighborhood responses are expected to be strong (Green et al., 1998b).

Some shortcomings with this measure of immigration is that it could capture individuals that immigrated a long time ago and that are already well integrated in terms of shared values and language. However, since this is not something that can be directly observed from the outside, we believe that it will still have an impact on the perceived threat to the shared identity of the defended neighborhood. Furthermore, since we cannot capture the movements of refugees with this measure it may introduce a discrepancy between the municipalities that had the most new immigrant residents and those where a lot of refugees resided during the crisis. This could lead us to potentially misinterpret which municipalities are subject to the the largest inflows of immigrants which is crucial for identification of the effect on hate crime from an "immigrant shock".

3.1.3 Income Inequality

The different measures for income inequality used in our analysis are taken from Statistics Sweden's database. The Gini coefficient measures income inequality by capturing how much the income distribution deviates from perfect equality using the Lorenz curve. A Gini coefficient with a value of 0 expresses perfect equality, while 1 describes full inequality of income. We chose to look at the Gini coefficient as our main variable of interest, instead of the poverty or unemployment rate, to grasp the impact that relative income differences have on hate crime rates. Another alternative, used in our robustness check is to look at the ratio of different income percentiles. We chose one of the pre-existing ratios in Statistics Sweden's dataset which is the ratio of the 90th to the 10th income percentile. These measures are both calculated using equivalized data[9] on a household's disposable income including income from capital.[10] The disposable income including capital of households is more representative of economic stature compared to using the disposable income excluding capital since it also captures individuals' return on investments. This data on equivalized disposable income only exist from 2011 and onwards. We cannot include years before 2011 in our sample due to this restriction.

3.1.4 Covariates

Election data for the municipalities are retrieved from the Swedish Election Administration for the years 2010, 2014, and 2018. The variable of interest is the vote support for the Swedish Democrats in the general election, as this is connected to support of radical right politics and hostile attitudes towards immigrants (Müller & Valdez, 2014). For the years in between elections we linearly predict the vote share. The vote share is seen as a proxy for the expression of anti-immigrant sentiment in society.

Data on the number of individuals that have pursued a higher education for the ages $16-74$, is retrieved from Statistics Sweden's database. We divide the number by census data on people aged $16-74$ to get a share of the adult population that have been enrolled in a post high school education. Using the logic from strain theory highly educated people should perceive the inflow of immigrants as less of a threat to their economic standing.

Next, we collect data on unemployment rates for each municipality from the Swedish Public Employment Services. The data includes both those who are openly unemployed and those in programs receiving development allowance. We would expect that high

[9] Meaning that data is adjusted for the number of children and members in the household such that disposable income can be compared across heterogeneous households.

[10] Disposable income including capital is calculated as follows; taxable income minus taxes and negative transfers plus real capital gains and or losses.

unemployment rates would lead to greater frustration towards the inflow of newcomers, as theorized in the literature (Green et al., 1998a,b, Dustmann et al., 2011, Entorf & Lange, 2019, Gale et al., 2002).

Lastly, we retrieve, from the same database as the measures of income inequality, data on poverty rates to use as a control. The poverty rate, as defined by Statistics Sweden, is the share of people living in a household below 60% of the median income value in Sweden. It is also calculated using equivalized data on disposable income including capital.

In Table 3.1 summary statistics for the presented variables, are reported for the 59 municipalities entering into our sample, without log transformations. Furthermore, a correlation table is provided in Table A.3 in the Appendix.

Table 3.1
Summary Statistics

Variable	Min	Median	Mean	Max	N
Hate Crime	2.600	5.781	6.258	29.345	353
Immigrant Share	0.059	0.204	0.228	0.593	413
Gini	0.228	0.277	0.282	0.402	413
Poverty Rate	0.047	0.135	0.139	0.237	413
Vote Share	0.032	0.117	0.124	0.307	413
Unemployment Rate	0.026	0.081	0.083	0.170	413
Education	0.221	0.327	0.347	0.620	413

Note: This table presents summary statistics of the variables used in our analysis. Statistics are taken on a pooled sample of the 59 municipalities in our dataset for the period 2011-2016 and 2018. Data on hate crime is missing where a municipality did not exceed the reporting requirement of 20 hate crime incidents set by BRÅ.

3.2 Empirical Strategy

This paper aims to test the effect of changes in ethnic composition and degree of income inequality on hate crime. We want to know if these factors can explain the observed increase in Swedish hate crime rates during the Migrant Crisis. That is, we want to test the explanatory value of the structural factors specified by the defended neighborhoods theory and the strain theory on hate crime. In Section 3.2.1 and 3.2.2 we develop the empirical strategy that will attempt to estimate the effect on hate crime from changes in ethnic composition and income inequality, respectively.

3.2.1 Hate Crime and Changes in Ethnic Composition

Since all Swedish municipalities received some refugees during the Migrant Crisis there are no municipalities that can represent the true counterfactual outcome of no inflow of immigrants. We instead evaluate whether receiving a larger proportion of immigrants during the Migrant Crisis result in higher hate crime rates. Hate crime and ethnic composition is however interlinked by unobserved factors such as the innate level of acceptance toward minority groups. Hence, changes in the ethnic composition are not inherently random and using OLS for identification would lead to endogeneity. We would expect that the omission of the innate level of acceptance to lead to an underestimation of the effect of the "immigrant shock", i.e. a negative bias. Other authors have addressed the issue of identification by utilizing first-differences (Entorf & Lange, 2019) or by employing count data poisson models (Green et al., 1998b, Grattet, 2009). Our approach differs from both because we do not use count data and want to capture the development of the treatment effect over time.

Instead of OLS, we use an event study approach which will reduce the possible omitted variable bias and control for time trends, by comparing the outcomes between the treatment and control group. Additionally, an event study will control for the factors that relate to hate crime but are uncorrelated with changes in ethnic composition. We also include time and municipality fixed effects in the event study. The municipality fixed effects allows for different baseline outcomes across units, which in effect removes any time invariant heterogeneity, again addressing issues concerning omitted variable bias. The current time period will by construction be correlated with treatment, which is why time fixed effects are included. Municipalities are assigned to the treatment group on the basis of having what we call a "immigrant shock" during the Migrant Crisis. More specifically, we look at the change in the immigrant share, the fraction of the population born to two foreign parents, from 2014 to 2015. This is the period when Sweden experienced the most rapid compositional change. Municipalities that had a change in immigrant share above the median are assigned to the treatment group. The change is given in percentage points. We let 2014 be the year of reference.

Equation (1) aims to test the role of changes in ethnic composition and the plausibility of the defended neighborhoods theory explaining hate crime. We estimate the following event study with municipality and time fixed effects;

$$Hate\ Crime_{it} = \alpha_i + \sum_{t=2011, t\neq 2014,2017}^{2018} \beta_t \cdot Immigrant\ Shock_i + \delta \cdot X_{it} + \lambda_t + \epsilon_{it} \tag{1}$$

where $Hate\ Crime_{it}$ is the log of reported hate crime per 10 000 citizens. $Immigrant\ Shock_i$ is an indicator variable equal to 1 if municipality i experienced an above median change in the immigrant share from 2014 to 2015. α_i and λ_t are municipality and time fixed effects respectively. X_{it} is a vector of time-varying, municipality-level covariates including; Gini coefficients (*Gini*), vote support for the Sweden Democrats (*Vote Share*), the share of immigrants (*Immigrant Share*), unemployment rates (*Unemployment Rate*), share of adults with a higher education (*Education*) and poverty rates (*Poverty Rate*). These covariates are all fractions and are transformed in the following way $log(1 + x_{it})$. Both dependent and independent variables in our model are positively skewed, and we use log transformations to induce normality and address the skewness. The normality of the data is important to ensure the validity of t-tests and estimators. Since 2014 is omitted, that means estimates are relative to the year before the crisis ensued.

To properly identify any effect of treatment (β_t) there needs to be a common trend in the untreated outcomes. That is, in the absence of treatment both treatment and control group would have followed the same trend. Given the log transformation of the number of hate crime incidents per 10 000 citizens, the underlying assumption is that the two group would have had similar percentage changes in hate crime rates, in the absence of treatment. We believe it reasonable to assume that the hate crime rates would develop with the same growth rates across municipalities and not in absolute values. It is easier to justify a common trend in percentage changes than in absolute values because the former is more comparable across different levels of hate crime rates. Parallel trends is necessary to ensure that differences in hate crime rates between the treatment and control group can be attributed to the "immigrant shock" and not pre-existing differences between the two groups of municipalities. Furthermore, there should be no anticipatory effect of being treated, allowing us to use the period preceding treatment as a reference without bias. Event studies such as equation (1) are frequently used to test for a common pre-trend of the treatment and control group. Such a test can be a good indicator of whether parallel trends is a plausible assumption to make. However, it is often done in combination with a graphical interpretation to not only illustrate any differences in trends between the series but their levels as well.

Pre-trends testing using an event study can have low statistical power, making it harder to detect indications of a possible failure of the parallel trends assumption. Yet, with a test of higher statistical power the opposite may happen, where one potentially identifies differing pre-trends even though they may be small and unimportant for proper identification (Roth, 2022). In attempt to assess the robustness of the parallel trends assumption and to be diligent about any omitted variables we also condition hate crime rates on several observable differences between the treatment and control group (see Table A.4 in the Appendix). These are the municipality-level time-varying covariates (X_{it}) mentioned above. Conditionality is often needed in a non-experimental setting to control for membership of the treatment group.

To avoid bias from any residual correlation that may occur within the treatment and control group, cluster robust standard errors are employed to compute t-statistics in our analysis. Clustering at the level of treatment is used, since observations from the same municipality are likely correlated over time. The rule of thumb for using clustered errors suggest 30-50 clusters are needed for reliable calculations. We have, as aforementioned, 59 municipalities with data in our sample and hence 59 clusters as well. The choice of the level of clustering comes at a bias-variance trade-off, larger and fewer clusters have less bias but more variability and vice versa. The recommendation is to be conservative and choose more aggregate clustering when possible to avoid bias. However, in our case clustering at the county-level does not make sense as this is not the level of treatment and because we choose to include municipality fixed effects. Furthermore, using county-level clustering would result in only 21 clusters which is less than what is suggested by the rule of thumb.[11]

By estimating β_t we try to capture whether the observed increase in hate crime can be explained by changes in the ethnic composition and the defended neighborhoods theory. The theory suggests there should be a spike in the hate crimes rates in 2015, corresponding to the peak of the Migrant Crisis, for both treatment and control group. In other words, we would expect a positive and significant λ_{2015}. Furthermore, We would also anticipate that those who experienced larger changes in ethnic composition during this period to have higher animosity against the newcomers and thus higher hate crime rates, i.e. a positive β_{2015}, in line with findings of Entorf & Lange (2019).

3.2.2 Hate Crime and Income Inequality

In the second part of the analysis we explore the link between income inequality and hate crime to assess the explanatory value of strain theory. The municipalities in our sample are split into two groups based on the degree of income inequality in the year prior to the Migrant Crisis. The level in 2014 is chosen since the inflow of immigrants will likely impact those in 2015. The selected measure of income inequality is the Gini coefficient. We assign the municipalities above the median Gini coefficient to the high

[11] Many of the municipalities are from within the same county resulting in far fewer counties, most prevalent are municipalities from Stockholm County (15) followed by Östra Götaland County (7) and Skåne County (7)

income inequality group and those below to the low income inequality equivalent. We then estimate equation (2), which is in essence the same as equation (1), but with separate estimates for the municipalities with high (h) and low (l) income inequality ($g = \{h, l\}$).

$$Hate\ Crime_{g,it} = \alpha_{g,i} + \sum_{t=2011,t\neq 2014,2017}^{2018} \beta_{g,t} \cdot Immigrant\ Shock_{g,i} + \delta \cdot X_{g,it} + \lambda_{g,t} + \epsilon_{g,it} \tag{2}$$

This is done to determine the role of strain in generating the high hate crime outcomes during the Migrant Crisis. It is Entorf & Lange (2019) that highlights the combination of poor economic conditions and changes in ethnic composition as possibly conducive to hate crime. We extrapolate this finding to other economic conditions and propose that it may also be true for economic indicators of income inequality. We believe that the relative success of others within the municipality will impact the goals that individuals in that municipality strive for. Thus, measures like the Gini coefficient may be better at capturing the type of strain that we hypothesize underlies hate crime, compared to unemployment rates. By splitting the dataset in this manner, any difference in response to the inflow of immigrants, between the high and low income inequality groups will be apparent. Strain theory suggests some of the same findings as the defended neighborhoods theory, such as a positive and significant $\lambda_{g,2015}$ and $\beta_{g,2015}$. However, from strain theory we would also expect that the high income inequality coefficients ($\lambda_{h,2015}$ and $\beta_{h,2015}$) to be of either greater magnitude or significance or both.

Some questions can be raised with regards to our empirical strategy. For instance, if there exists sufficient logical and empirical evidence to support the assumption of a parallel trend? Additionally, one may question if included covariates are adequate to rule out omitted variable bias? There is a clear risk that the data may not exhibit parallel trends and that we in our analysis could have omitted municipality characteristics important for identification. Additionally, the fixed effects in our model may capture something entirely unrelated to the inflow of immigrants during the Migrant Crisis, especially considering the data collection issues. Nonetheless, we believe the chosen method is suitable given the data at hand and the inferences that we want to make. However, we cannot deny that there may exist approaches better suited to estimate the effect of changes in ethnic composition and income inequality on hate crime.

4 Empirical Analysis

In this section the results of the empirical analysis are presented. In Section 4.1 the results with regards to hate crime and changes in ethnic composition are reported. In Section 4.2 we present the result regarding hate crime and its relationship to income inequality. We conclude the analysis by evaluating the robustness of our results in Section 4.3.

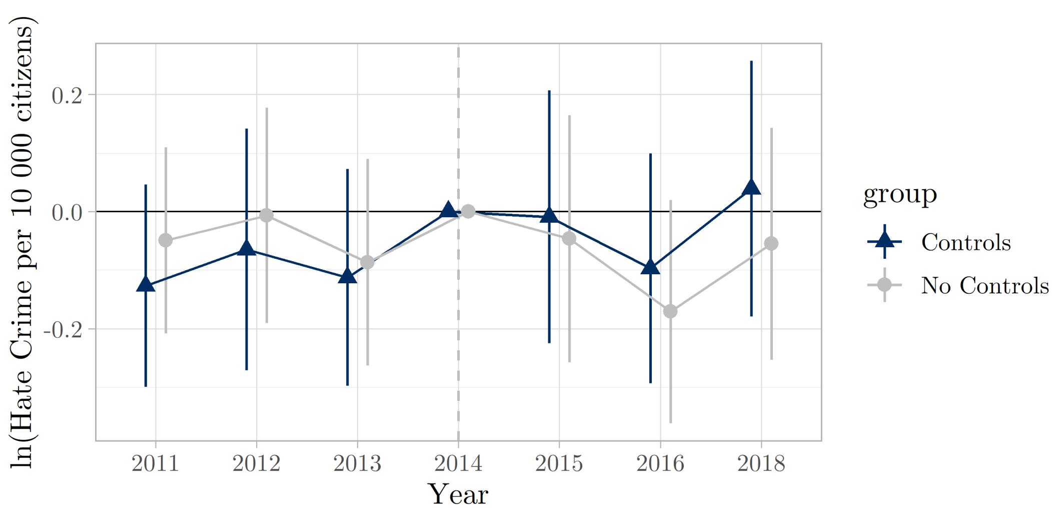
4.1 Hate Crime and Changes in Ethnic Composition

We begin our analysis by estimating equation (1) for the 59 municipalities in our sample. Results are reported in Table 4.1. Before dissecting the results we need to assess the validity of the parallel trends assumption, which is needed to interpret estimates as causal. This is, as mentioned, often done through pre-testing in combination with a graphical interpretation. However, we want to highlight that these tests are not evidence enough for parallel trends to be taken as true, it is still an assumption we make regarding the nature of the data. We provide further assessment of the robustness of the parallel trends assumption in Section 4.3. In Table 4.1 there are no significant treatment effects preceding the Migrant Crisis in any of the specifications. This suggest that the pre-trends are statistically indistinguishable. This should be seen as support for the plausibility of the parallel trends assumption. In Figure 2 we illustrate the treatment effect from column (1) and (3) from Table 4.1 and their 95% confidence intervals. We also provide more graphical support for the parallel trends assumption by plotting the average hate crime rates for the treatment and control group (see Figure A.4 in the Appendix). Furthermore, a list of the municipalities in each group can be found in Table A.5 and a map of the municipalities by treatment status in Figure A.5. Although trends are similar between the treatment and control group there is a difference in the level of hate crime between the two. The treatment group has on average higher hate crime rates than the control group (see Figure A.4). This could potentially be explained by the treatment group having lower education rates, higher immigrant share, and higher poverty rates, which are all associated with higher hate crime rates (see Table A.3 and Table A.4 in the Appendix).

In column (1) of Table 4.1 we display the regression results from equation (1) without any controls. The positive and significant time fixed effect for 2015 (λ_{2015}) is indicative that all municipalities on average experienced higher hate crime rates during the peak of the Migrant Crisis compared to the baseline year of 2014. More specifically, λ_{2015} implies, on average, 14% higher hate crime rates in 2015 compared to 2014. λ_{2015} is significant at the 10% level. We cannot find any evidence suggesting that the change in ethnic composition, or "immigrant shock", would induce higher hate crime rates during the crisis. In other words, we do not observe a significantly positive β_{2015}. The point estimate is -0.05 and the p-value is far greater than any conventional level of significance. Irrespective of the change in ethnic composition, the municipalities in our sample exhibit similar initial responses. This result is robust to restricting the sample to only include the municipalities with an average immigrant share below 25% (see Table A.6 in the Appendix). These places of low ethnic diversity, or "white strongholds", are the areas found by Green et al. (1998b), Grattet (2009), Dustmann et al. (2011) to have the strongest response to an inflow of newcomers. The results from the regression using the restricted sample are very similar to those in Table 4.1. There are no divergent treatment effects in Table A.6, but coefficients are generally of a slightly greater magnitude.

Instead of the expected positive treatment effect in 2015 our results indicate that the "immigrant shock" has a dissipative effect on hate crime rates the year following the Migrant Crisis. β_{2016} is equal to -0.17 and is significant at the 10% level. However, when we add the municipality-level time-varying covariates the coefficient grows smaller and errors larger, loosing the initial significance.

Figure 2. Event Study Plot of Immigrant Shock on Hate Crime



Note: Event study plot of β_t from column (1) and (3) in Table 4.1 displaying results from the following regression: $Hate\,Crime_{it} = \alpha_i + \sum \beta_t \cdot Immigrant\,Shock_i + \delta \cdot X_{it} + \lambda_t + \epsilon_{it}$.

Table 4.1
Hate Crime and Change in Ethnic Composition

	Dependent Variable: *Hate Crime*		
	(1)	(2)	(3)
2011	0.05 (0.06)	−0.08 (0.10)	−0.08 (0.11)
2012	−0.03 (0.06)	−0.13 (0.08)	−0.12 (0.08)
2013	−0.05 (0.05)	−0.10 (0.07)	−0.10 (0.06)
2015	0.14* (0.07)	0.18** (0.08)	0.18** (0.08)
2016	0.05 (0.07)	0.16* (0.09)	0.16 (0.10)
2018	0.05 (0.07)	0.27** (0.13)	0.27* (0.16)
Gini		−0.62 (2.85)	−0.80 (2.93)
Vote Share		−0.36 (1.75)	−0.50 (2.02)
Immigrant Share		−9.08** (4.03)	−9.11* (4.74)
Unemployment Rate			−1.25 (3.15)
Education			−0.45 (5.66)
Poverty Rate			1.40 (3.72)
2011 × *Immigrant Shock*	−0.05 (0.07)	−0.12 (0.08)	−0.13 (0.08)
2012 × *Immigrant Shock*	−0.01 (0.09)	−0.06 (0.10)	−0.06 (0.10)
2013 × *Immigrant Shock*	−0.09 (0.08)	−0.11 (0.08)	−0.11 (0.09)
2015 × *Immigrant Shock*	−0.05 (0.10)	−0.01 (0.10)	−0.01 (0.10)
2016 × *Immigrant Shock*	−0.17* (0.09)	−0.10 (0.09)	−0.10 (0.09)
2018 × *Immigrant Shock*	−0.06 (0.09)	0.04 (0.10)	0.04 (0.10)
Time Fixed Effects	✓	✓	✓
Municipality Fixed Effects	✓	✓	✓
Clustered Standard Errors	✓	✓	✓
N	353	353	353
R^2	0.66	0.67	0.67

Note: This table presents the results from the following event study: $Hate\ Crime_{it} = \alpha_i + \sum \beta_t \cdot Immigrant\ Shock_{it} + \delta \cdot X_{it} + \lambda_t + \epsilon_{it}$. $Hate\ Crime_{it}$ is the log of hate crime per 10 000 citizens. *Immigrant Shock* is an indicator variable equal to 1 for each year t, if the municipality i experienced an above median change in immigrant share from 2014 to 2015. X_{it} is a vector of covariates. Covariates are all transformed as follows log(1+x). α_i and λ_t are municipality and time fixed effects respectively. The sample consists of yearly observations for the period 2011-2016 and 2018. 2014 is omitted. The panel contains 59 municipalities with data before and after 2015. T-statistics are computed using clustered standard errors at the municipality level which are reported in parentheses. Statistical significance is attributed based on p-values as follows: *p<0.1; **p<0.05;***p<0.01.

In column (2) we control for the degree of income inequality (*Gini*), vote support for the Sweden Democrats (*Vote Share*), and the share born to two foreign parents or the initial ethnic composition (*Immigration Share*). Looking at the time fixed effects in column (2) we find that the increase in hate crime rates previously only seen in 2015 is not isolated. The regression displays positive and significant coefficients in 2016 and 2018 as well. The point estimates for λ_{2016} and λ_{2018} are 0.16 and 0.27 and they are significant at the 10 and 5% level, respectively. The inclusion of the covariates has also led to an increase in magnitude and significance of λ_{2015}. λ_{2015} is now 0.18 and significant at the 5% level. When unemployment rates (*Unemployment Rate*), share of adults having pursued higher education (*Education*), and poverty rates (*Poverty Rate*) are added, λ_{2015} and λ_{2018} remain significant but not λ_{2016}.

4.2 Hate Crime and Income Inequality

To understand the relationship between income inequality and hate crime during the Migrant Crisis, we split the municipalities into two groups based on the degree of income inequality and estimate equation (2). Results are reported in Table 4.2. The selected measure of income inequality is the Gini coefficient in 2014. As aforementioned, we assign the municipalities with a Gini coefficient above the median to the high income inequality group and those below to the low income inequality equivalent. The municipalities in each group are found in Table A.7 in the Appendix and their location in Figure A.6. The omitted year is still 2014. Before interpreting the results, we again assess the plausibility of the parallel trends assumption. As there are no significant differences in trends preceding the Migrant Crisis the parallel trends assumption is believed plausible. A graph of β_t from column (2) and (4) in Table 4.2 is found in Figure 3. When we look at the average hate crime rates there are also satisfying evidence of the existence of a parallel trend (see Figure A.7 in the Appendix). In Table 4.2, columns (1) and (2) show the findings for the low income inequality group. For this group no surge in hate crime rates in 2015 is observed, which was the case for the whole dataset. The value of $\lambda_{l,2015}$ in column (1) is 0.03 and its p-value 0.7310. In column (1), we see a significant drop in hate crime in 2013. $\lambda_{l,2013}$ is -0.15 and significant at the 5% level. This result is however, not robust to adding covariates. The high income inequality event studies, in column (3) and (4), show no drop in 2013 and have a spike in hate crime rates in 2015. The estimated increase in hate crime in column (3) is greater than the equivalent estimate from Table 4.1, $\lambda_{h,2015}$ is 0.24 compared to λ_{2015} which is 0.14 (no controls). Both estimates are significant at the 5% level. Figure A.8 in the Appendix plots $\lambda_{g,t}$ from column (2) and (4).

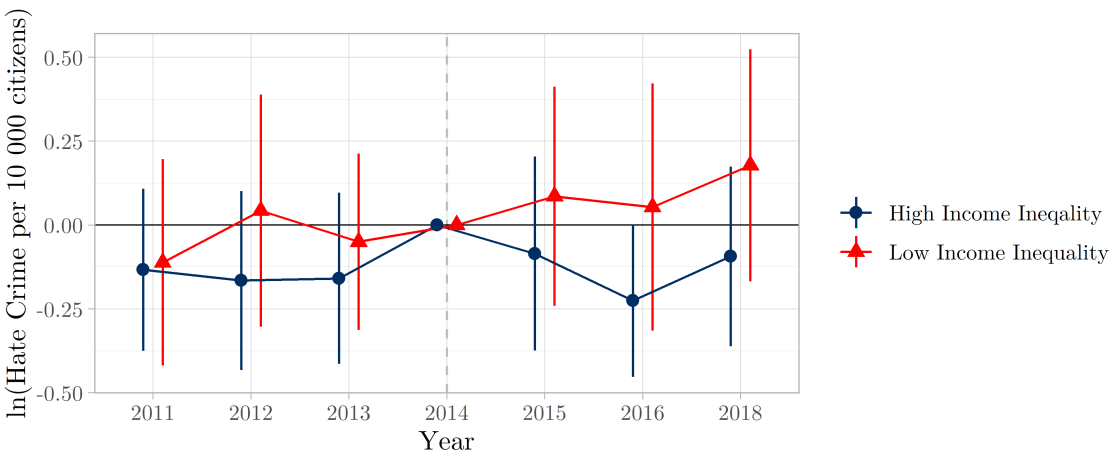
Table 4.2

Hate Crime, Ethnic Composition and Income Inequality

	Dependent Variable: *Hate Crime*			
	Low Income Inequality		High Income Inequality	
	(1)	(2)	(3)	(4)
2011	0.01 (0.08)	−0.03 (0.24)	0.10 (0.08)	0.02 (0.12)
2012	−0.08 (0.09)	−0.10 (0.15)	0.03 (0.08)	−0.03 (0.10)
2013	−0.15** (0.07)	−0.14 (0.10)	0.04 (0.08)	0.01 (0.08)
2015	0.03 (0.10)	0.04 (0.12)	0.24** (0.10)	0.27** (0.10)
2016	0.01 (0.13)	0.05 (0.19)	0.09 (0.07)	0.17* (0.10)
2018	−0.07 (0.09)	0.02 (0.27)	0.17 (0.11)	0.34* (0.20)
Unemployment Rate		−5.67 (5.73)		1.19 (4.28)
Vote Share		0.76 (4.03)		−0.001 (2.08)
Immigrant Share		−9.05 (7.59)		−6.53 (5.42)
2011 × *Immigrant Shock*	0.003 (0.12)	−0.11 (0.14)	−0.08 (0.10)	−0.13 (0.11)
2012 × *Immigrant Shock*	0.13 (0.14)	0.04 (0.16)	−0.13 (0.11)	−0.17 (0.12)
2013 × *Immigrant Shock*	−0.02 (0.11)	−0.05 (0.12)	−0.14 (0.11)	−0.16 (0.12)
2015 × *Immigrant Shock*	0.02 (0.15)	0.09 (0.15)	−0.11 (0.13)	−0.09 (0.13)
2016 × *Immigrant Shock*	−0.07 (0.16)	0.05 (0.17)	−0.26** (0.10)	−0.23** (0.11)
2018 × *Immigrant Shock*	0.06 (0.14)	0.18 (0.16)	−0.15 (0.12)	−0.09 (0.12)
Time Fixed Effects	✓	✓	✓	✓
Municipality Fixed Effects	✓	✓	✓	✓
Clustered Standard Errors	✓	✓	✓	✓
N	159	159	187	187
R^2	0.67	0.69	0.67	0.67

Note: This table presents the results from: $Hate\ Crime_{g,it} = \alpha_{g,i} + \sum \beta_{g,t} \cdot Immigrant\ Shock_{g,i} + \delta \cdot X_{g,it} + \lambda_{g,t} + \epsilon_{g,it}$ estimated separately for the municipalities with above (high) and below (low) median Gini coefficients. $Hate\ Crime_{it}$ is the log of hate crime per 10 000 citizens. *Immigrant Shock* is an indicator variable equal to 1 for each year t, if the municipality i experienced an above median change in immigrant share from 2014 to 2015. X_{it} is a vector of covariates. Covariates are all transformed as follows log(1+x). α_i and λ_t are municipality and time fixed effects respectively. The sample consists of yearly observations for the period 2011-2016 and 2018. 2014 is omitted. The panel contains 59 municipalities with data before and after 2015. T-statistics are computed using clustered standard errors at the municipality level which are reported in parentheses. Statistical significance is attributed based on p-values as follows: *p<0.1; **p<0.05;***p<0.01.

Figure 3. Event Study Plot of Immigrant Shock on Hate Crime by Income Inequality



Note: Event study plot of β_t from column (1) and (3) in Table 4.2 displaying the result from the following event study estimated separately for the high and low income inequality groups: $Hate\,Crime_{g,it} = \alpha_{g,i} + \sum \beta_{g,t} \cdot Immigrant\ Shock_{g,i} + \delta \cdot X_{g,it} + \lambda_{g,t} + \epsilon_{g,it}$. *N=159* in the low income inequality group and *N=187* in the high income inequality group.

Looking at the treatment effects ($\beta_{g,t}$) we still do not find any significant increase in hate crime from the "immigrant shock" in any of the specifications. $\beta_{l,2015}$ has the right sign but is insignificant, and $\beta_{h,2015}$ is both negative and insignificant. However, there is a treatment effect in 2016 for the high income inequality group. Those in the high income inequality group that had the largest change in ethnic composition also experience the dissipative effect discussed previously. The combination of being in the high income inequality group and the treatment group leads to a 26% or 23% decrease in the average hate crime per 10 000 citizens in 2016, depending on the included covariates. The effect is greater in absolute values than the equivalent coefficient from Table 4.1. $\beta_{h,2016}$ is -0.26 compared to β_{2016} which is equal to -0.17 (without covariates). Furthermore, $\beta_{h,2016}$ is still significant when adding the selected covariates. This was not the case in Table 4.1. Lastly, we note that the coefficient on $ImmigrantShare_{it}$ that was significant in Table 4.1, is no longer so when splitting the sample based on the Gini coefficient. This is likely a result of their moderate correlation.

4.3 Robustness of Empirical Results

In this section we assess and comment on the robustness of the empirical results. First, we gauge the importance of the selected transformations and errors for the results and parallel trends assumption. Second, we look at other cutoffs than the median for our first event study (equation (1)). Third and finally, we look at a different measure for income inequality to see if our results are replicable for other income inequality benchmarks.

We estimate equation (1) without log transformations on the included variables. Results are reported in Table A.8 in the Appendix. Column (1) in Table A.8 displays coefficients of similar sign and significance to those in column (1) in Table 4.1. The results are less similar for column (2) and (3), where additional covariates have been added. From column (2) and (3), it becomes evident that the assumption of parallel trends is sensitive to the functional form. Parallel trends can be sensitive to the functional form if treatment is not as-if randomly assigned or the distribution of the dependent variable is not stable over time and treatment. The assumptions needed for insensitivity to the functional form can often be quite restrictive, especially in the first case mentioned (Roth & Sant'Anna, 2023). If, as we do, one observes differing levels of the dependent variables between treatment and control group, parallel trends often cannot hold simultaneously for both level and log transformations (Kahn-Lang & Lang, 2020). We believe the log transformation is a more reasonable assumption about the development in trends.

We employ clustered standard errors in our analysis to protect inference against within-cluster correlation. Another alternative to address this issue is to use Newey-West or heteroscedasticity and autocorrelation robust (HAC) standard errors. The computations needed for calculation of HAC errors are described in Whitney K. Newey (1987). Results from equation (1) with HAC standard errors are reported in Table A.9 in the Appendix. The lag length is chosen using the rule of thumb, that is $m = 0.75N^{1/3}$, where $N = 353$. The reported HAC errors are smaller or equal to the clustered standard errors, resulting in higher significance levels and a less conservative analysis. With the smaller errors there is a significant treatment effect in 2011, which is prior to treatment. This finding challenges the validity of the parallel trends assumption. Additionally, some coefficients become more significant with smaller errors. For example, λ_{2013} is equal to -0.10 and now significant at the 10% level in specification (2), possibly due to the drop in hate crime rates in 2013 observed in municipalities with low income inequality. However, this result is not robust to adding the last covariates. There is a preference to go with the more conservative errors in the analysis and the standard in event studies is to cluster errors at the cross-sectional source of variation. This robustness check highlights the sensitivity of error selection for the plausibility of the parallel trends assumption.

Next, we look at the robustness of our results when increasing the cutoff for assignment to the treatment group and consequently lowering that of the control group. Results from equation (1) using the 50th (original), 60th, 70th and 80th percentiles as cutoffs for assignment to the treatment group are reported in Table 4.3. Observe that no observations are omitted as a result of the new cutoffs. Pre-testing showed no significant pre-trends, except for the specification where the 70th percentile was used as the cutoff. In this case, the treatment effect was observed in 2013, which could be due to the decrease in hate crime rates observed in municipalities with low income inequality in Table 4.2. For this specification the cutoff has been set such that the treatment group contains more low income inequality municipalities.[12] Except for the median the 70th percentile cutoff is also the only other specification that displays the dissipative effect in 2016. This is unexpected as we know the treatment group consists of more low income inequality municipalities, which did not display such a treatment effect in Table 4.2. However, the significance of β_{2016} falls away when controls are added. Treatment effects for all specifications with controls are plotted in Figure A.9 in the Appendix. When it comes to λ_t all specifications have a significant increase in hate crime rates in 2015 without the controls, and in 2015 and 2018 with the covariates (*Gini*, *Vote Share*, and *Immigrant Share*). λ_{2015} and λ_{2018} have similar size and the same or higher significance than in the original specification. This is consistent with previous results. This robustness check highlights that the selection of the cutoff both impact the observed treatment effects and the plausibility of the parallel trends assumption. The only results robust to changing the cutoff in this way are those with respect to λ_t.

[12] For the 70th percentile the average Gini coefficient was 0.2416 and 0.2513 for the treatment and control group respectively. When the cutoff is based on the median, the average is 0.2462 for the treatment group and 0.2505 for the control group

Table 4.3

Hate Crime and Changes in Ethnic Composition with Staggered Cutoffs

	Dependent Variable: *Hate Crime*							
	50th		60th		70th		80th	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2011	0.05 (0.06)	−0.08 (0.10)	0.04 (0.05)	−0.11 (0.10)	0.04 (0.04)	−0.10 (0.10)	0.02 (0.04)	−0.10 (0.10)
2012	−0.03 (0.06)	−0.13 (0.08)	−0.04 (0.05)	−0.15* (0.08)	−0.05 (0.05)	−0.15* (0.08)	−0.04 (0.05)	−0.13* (0.07)
2013	−0.05 (0.05)	−0.10 (0.07)	−0.06 (0.05)	−0.12* (0.07)	−0.05 (0.05)	−0.10 (0.06)	−0.09* (0.05)	−0.13** (0.06)
2015	0.14* (0.07)	0.18** (0.08)	0.13** (0.06)	0.18** (0.07)	0.14*** (0.05)	0.19*** (0.06)	0.12** (0.05)	0.16** (0.06)
2016	0.05 (0.07)	0.16* (0.09)	0.02 (0.06)	0.13 (0.09)	0.01 (0.06)	0.12 (0.09)	−0.02 (0.05)	0.07 (0.08)
2018	0.05 (0.07)	0.27** (0.13)	0.05 (0.06)	0.29** (0.14)	0.04 (0.06)	0.26** (0.13)	0.04 (0.05)	0.21* (0.12)
Gini		−0.62 (2.85)		−0.74 (2.77)		−0.47 (2.62)		−0.62 (2.59)
Vote Share		−0.36 (1.75)		−0.87 (1.92)		−0.56 (2.01)		−0.19 (1.96)
Immigrant Share		−9.08** (4.03)		−8.69** (4.08)		−8.00** (3.80)		−6.58* (3.60)
2011 × *Immigrant Shock*	−0.05 (0.07)	−0.12 (0.08)	−0.03 (0.07)	−0.11 (0.08)	−0.05 (0.08)	−0.12 (0.08)	0.06 (0.09)	−0.0004 (0.09)
2012 × *Immigrant Shock*	−0.01 (0.09)	−0.06 (0.10)	0.01 (0.09)	−0.06 (0.10)	0.05 (0.09)	0.0003 (0.10)	0.06 (0.11)	0.01 (0.12)
2013 × *Immigrant Shock*	−0.09 (0.08)	−0.11 (0.08)	−0.09 (0.09)	−0.12 (0.09)	−0.16* (0.09)	−0.19** (0.09)	−0.05 (0.09)	−0.07 (0.09)
2015 × *Immigrant Shock*	−0.05 (0.10)	−0.01 (0.10)	−0.05 (0.10)	−0.01 (0.10)	−0.10 (0.12)	−0.07 (0.12)	−0.03 (0.14)	0.003 (0.14)
2016 × *Immigrant Shock*	−0.17* (0.09)	−0.10 (0.09)	−0.12 (0.09)	−0.05 (0.09)	−0.16* (0.09)	−0.09 (0.09)	−0.08 (0.10)	−0.02 (0.11)
2018 × *Immigrant Shock*	−0.06 (0.09)	0.04 (0.10)	−0.08 (0.09)	0.01 (0.10)	−0.08 (0.10)	0.02 (0.11)	−0.09 (0.11)	−0.01 (0.12)
Time Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Municipality Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Clustered Standard Errors	✓	✓	✓	✓	✓	✓	✓	✓
Muni. in Treatment Group	29	29	24	24	18	18	12	12
N	353	353	353	353	353	353	353	353
R^2	0.66	0.67	0.66	0.66	0.66	0.67	0.66	0.66

Note: This table presents the results from: $\textit{Hate Crime}_{it} = \alpha_i + \sum \beta_t \cdot \textit{Immigrant Shock}_i + \delta \cdot X_{it} + \lambda_t + \epsilon_{it}$. $\textit{Hate Crime}_{it}$ is the log of hate crime per 10 000 citizens. *Immigrant Shock* is an indicator variable equal to 1 for each year t, if the municipality i experienced an above a certain percentile (displayed over the specification) change in immigrant share from 2014 to 2015. X_{it} is a vector of covariates. Covariates are all transformed as follows log(1+x). α_i and λ_t are municipality and time fixed effects respectively. The sample consists of yearly observations for the period 2011-2016 and 2018. 2014 is omitted. The panel contains 59 municipalities with data before and after 2015. T-statistics are computed using clustered standard errors at the municipality level which are reported in parentheses. Statistical significance is attributed based on p-values as follows: *p<0.1; **p<0.05;***p<0.01.

We now look at other measures of income inequality. The Gini coefficient is known to have some difficulties estimating fat-tailed distributions. Substituting the Gini coefficient with an income ratio, such as the ratio between the 90th an the 10th income percentile, could potentially better capture changes in the tails of the distribution. The Gini coefficient and the selected income ratio are highly related, with a correlation of 0.83. We run equation (2) again, but let the median income ratio, instead of the median Gini coefficient, determine the assignment to the high or the low income inequality group. Results are reported in Table 4.4. Based on pre-testing the parallel trends assumption is deemed plausible for specification (1), (3), and (4), but (2) shows differing trends in 2011. We therefore interpret the results with caution. A coefficient plot of β_t from column (1) and (3) is found in Figure A.10 in the Appendix and a plot of average hate crime rate by treatment status and income inequality in Figure A.11. With the switch from Gini coefficient to income ratio as our measure of income inequality we still find that the spike in hate crime is only present in the high income inequality group. In Table 4.2 we also observed heightened hate crime rates in column (4) for 2016 and 2018 which is not the case in Table 4.4. There is also a sizable difference between λ_{2015} from Table 4.4 and Table 4.2, around 6 percentage points. Furthermore, we cannot find any dissipative effect among the high income inequality group from the "immigrant shock". However, the coefficient is of the right sign but the error is to large for statistical significance. As seen in Figure A.10 the treatment effects between the two groups are overall very similar. In the low income inequality regression we do not observe any statistically significant decrease in hate crime rates in 2013, as observed in Table 4.2. In Figure A.11 however, one observes that average hate crime rates for the low income inequality group decreases substantially for this year. Our findings regarding the positive association between income inequality and hate crime rates are robust to using the income ratio instead of the Gini coefficient. This suggests that this result is not sensitive to the specific measure of income inequality used in the analysis. However, it is clear that the dissipative effect in 2016 is.

In conclusion, our series of robustness checks reveals that some of our results and assumptions are more robust than others. The robustness checks suggest that the parallel trends assumption is, although impossible to test, a strong assumption to make. In Table 4.3 the dissipative treatment effect is seemingly not that robust, as it appears in only one of the other cutoffs. However, all results from the original specification regarding the time fixed effects can also be observed with the other cutoffs. Furthermore, the results from Table 4.4 support our previous finding that income inequality may underlie heterogeneous hate crime rates. Though, Table 4.4 makes us again question the robustness of the dissipative effect in 2016.

Table 4.4

Hate Crime, Ethnic Composition and Income Inequality (Income Ratio)

	Dependent Variable: *Hate Crime*			
	Low Income Inequality		High Income Inequality	
	(1)	(2)	(3)	(4)
2011	0.06 (0.08)	0.16 (0.26)	0.05 (0.09)	−0.07 (0.13)
2012	0.003 (0.08)	0.09 (0.19)	−0.05 (0.10)	−0.13 (0.11)
2013	−0.09 (0.08)	−0.04 (0.12)	−0.01 (0.07)	−0.05 (0.08)
2015	0.09 (0.10)	0.06 (0.14)	0.18* (0.10)	0.21* (0.10)
2016	0.11 (0.12)	0.08 (0.22)	0.0003 (0.07)	0.07 (0.09)
2018	−0.02 (0.09)	−0.04 (0.34)	0.13 (0.11)	0.28 (0.19)
Unemployment Rate		−9.26 (7.48)		0.05 (3.97)
Vote Share		3.17 (4.20)		−1.66 (2.49)
Immigrant Share		−10.07 (7.20)		−4.10 (5.46)
2011 × *Immigrant Shock*	−0.10 (0.12)	−0.24* (0.13)	0.001 (0.10)	−0.04 (0.11)
2012 × *Immigrant Shock*	−0.04 (0.11)	−0.13 (0.12)	0.03 (0.13)	0.003 (0.14)
2013 × *Immigrant Shock*	−0.06 (0.13)	−0.10 (0.13)	−0.11 (0.11)	−0.12 (0.11)
2015 × *Immigrant Shock*	0.001 (0.16)	0.08 (0.16)	−0.08 (0.14)	−0.06 (0.14)
2016 × *Immigrant Shock*	−0.21 (0.15)	−0.05 (0.17)	−0.12 (0.10)	−0.09 (0.10)
2018 × *Immigrant Shock*	0.06 (0.14)	0.24 (0.17)	−0.14 (0.13)	−0.10 (0.12)
Time Fixed Effects	✓	✓	✓	✓
Municipality Fixed Effects	✓	✓	✓	✓
Clustered Standard Errors	✓	✓	✓	✓
N	159	159	190	190
R^2	0.69	0.71	0.63	0.64

Note: This table presents the results from: $Hate\ Crime_{g,it} = \alpha_{g,i} + \sum \beta_{g,t} \cdot Immigrant\ Shock_{g,i} + \delta \cdot X_{g,it} + \lambda_{g,t} + \epsilon_{g,it}$ estimated separately for the municipalities with above (high) and below (low) median income ratio (ratio between the 90th and 10th percentile in the income distribution). $Hate\ Crime_{it}$ is the log of hate crime per 10 000 citizens. *Immigrant Shock* is an indicator variable equal to 1 for each year t, if the municipality i experienced an above median change in immigrant share from 2014 to 2015. X_{it} is a vector of covariates. Covariates are all transformed as follows log(1+x). α_i and λ_t are municipality and time fixed effects respectively. The sample consists of yearly observations for the period 2011-2016 and 2018. 2014 is omitted. The panel contains 59 municipalities with data before and after 2015. T-statistics are computed using clustered standard errors at the municipality level which are reported in parentheses. Statistical significance is attributed based on p-values as follows: *p<0.1; **p<0.05;***p<0.01.

5 Discussion

The results of the empirical analysis indicate some evidence of the explanatory value of changes in ethnic composition and the defended neighborhoods theory for hate crime. From Table 4.1 it is evident that hate crime per 10 000 citizens has increased significantly during the Migrant Crisis as implied by the positive and significant λ_{2015} from equation (1). This is in line with the defended neighborhoods theory and our hypothesis about the inflow of immigrants resulting in heightened hate crime rates. This outcome is also concurrent with the results of Green et al. (1998b), Grattet (2009) and Entorf & Lange (2019). However, the main result of Entorf & Lange (2019) is that it is not simply the inflow of immigrants but the size of the resulting compositional change of the residential population that matters for hate crime responses. We cannot find any positive effect from the size of the change in ethnic composition on hate crime rates, and thus we cannot support this finding made by Entorf & Lange (2019). β_{2015} is both negative and insignificant. However, we note that our analysis differs in many ways from that of Entorf & Lange (2019) such that their sample contains all German counties, and they have knowledge of the distribution of refugees across counties. Furthermore, they utilize a first-difference approach and not an event study. All may be reasons why the results found by Entorf & Lange (2019) are not reproducible using our sample.

Instead of the expected positive effect from the "immigrant shock" we find that the municipalities in the treatment group experienced lower hate crime rates in 2016, the year immediately following the peak of the Migrant Crisis. Green et al. (1998b) expects heightened hate crime rates to dissipate following the inflow of newcomers, as the residents of the defended neighborhoods gradually learns to accept them as a part of the community and the shared social identity. Because there is no positive treatment effect in 2015, it is not clear why the dissipative effect in 2016 would only occur in treated municipalities, as the initial response is the same regardless of how drastic the change in ethnic composition is. The dissipative effect among the treated could imply that greater exposure to newcomers increases tolerance and induces acceptance at a faster pace than a less drastic change in ethnic composition would. Though, it could also be the case that the drop in hate crime rates is a result of those opposing the inflow moving away. One cannot rule out the possibility of either theory or the existence of a completely different reason for this observation. However, when we add the covariates, β_{2016} loses it significance. Moreover, the drop is only present in one of the other specifications with a different cutoff for treatment. Both accounts suggest that this observation may not be very robust.

In column (2) and (3) of Table 4.1, where controls have been added we find that λ_{2015} is smaller than λ_{2018}. This result contradicts the defended neighborhoods theory, which predicts a decline in hate crime rates after the newcomers have resided in the neighborhood for some time (Green et al., 1998b). We try to understand what may have caused increased hate crime in 2018. BRÅ highlights in their 2018 hate crime report the role of the Swedish general election for the increased hate crime rate in 2018. The increase stems from an almost doubling of the reported incitements to hatred produced by events connected to the election (Forselius & Westerberg, 2019). This is a likely explanation why λ_{2018} (0.27) is substantially greater than λ_{2015} (0.18). Green et al. (1998b), as mentioned, expect that the increase in hate crime following an inflow of newcomers would dissipate when the newcomers have resided in the same place for some time. The fact that hate crime rates are systematically higher in the period after the Migrant Crisis, when covariates are added, tells us that the time frame for when this may happen is longer than our sample. It also may be the case that there are counteracting effects such as the election entirely covering it. The analysis would here have benefited from data with a longer time frame.

In our analysis we find partial support for our hypothesis with respect to strain theory. In Table 4.2 we observe an increase in the average hate crime rates in 2015 among the high income inequality municipalities, however no corresponding increase is seen among the low income inequality municipalities. $\lambda_{l,2015}$ is smaller than $\lambda_{h,2015}$ and insignificant. This is in line with what would be expected from strain theory, which suggests that those subject to economic strain are more likely to express dislike and violence towards newcomers. Hence, hate crime rates are expected to be higher for the high income inequality group. This finding is robust to adding covariates and changing the measure of income inequality from the Gini coefficient to the income ratio (see Table 4.4). The low income inequality group, as mentioned, displays no surge in hate crime rates in 2015. However, the low income inequality group shows significantly lower rates in 2013 than in 2014. Though, this result is not robust to adding covariates. Nonetheless, we try to understand the underlying cause of this observation, which has no given reason in the report accompanying the hate crime data. We investigate whether this may be a result of missingness in our data, but the number of municipalities that have missing values are very similar the first three years of our sample. Thus, we cannot see missingness as a plausible explanation.[13] The reason why we would observe such a decrease in hate crime rates in 2013 for the low income inequality group remains unclear.

[13] The number of missing values are as follows; 13 (2011), 14 (2012), 13 (2013), 8 (20114), 4 (2015), 3 (2016), and 5 (2018)

Entorf & Lange (2019) found the combination of poor economic conditions, measured by high unemployment, and rapid compositional changes to be important when trying to explain the occurrence of hate crime. We have proposed an extrapolation of this finding to other economic conditions, such as income inequality. We would thus expect larger inflows of immigrants to lead to increased hate crime in areas subject to high income inequality. Contrary to our hypothesis, we cannot find anything that would suggest that the combination of "immigrant shocks" and high income inequality would produce higher hate crime rates. In Table 4.1 the only significant treatment effect was a dissipative one in 2016. In Table 4.2 we find the same dissipative treatment effect on hate crime present only in the high income inequality event studies. This would indicate, that it is the municipalities subject to "immigrant shocks" and high income inequality that are driving the results. We believe the reasoning laid forth before for the dissipative effect is improbable. We see no reason why greater exposure to newcomers would increases tolerance or cause those opposed to the inflow to move only in high income inequality municipalities. When changing the measure of income inequality from the Gini coefficient to the income ratio the result does not subsist indicating a lack of robustness (see Table 4.4).

Our results can partially corroborate those of Entorf & Lange (2019) and Dustmann et al. (2011) who also find indications that economic conditions may matter for the frequency of hate crimes committed in an area, when faced with an inflow of immigrants. Though, in their case, they connect the degree of economic hardship in an area to racially motivated crime and not income inequality. Our results suggests that income inequality may also be one of the economic conditions conducive to hate crime. Gale et al. (2002) also evaluates the role of relative income, but between between Blacks and Whites rather than the whole income distribution. The results presented by Gale et al. (2002) also point towards the explanatory value of income inequality in a hate crime setting.

6 Conclusion

This paper explores if the increase in hate crime in Sweden following the Migrant Crisis can be explained by changes in ethnic composition or the degree of income inequality. Using municipality-level data and an event study methodology we test the plausibility of the defended neighborhoods theory and the strain theory explaining hate crime.

Our analysis does indicate some evidence of the explanatory value of changes in ethnic composition and the defended neighborhoods theory. As expected, hate crime rate rose in all Swedish municipalities in our sample during the peak of the Migrant Crisis. However, there is no indication that higher hate crime rates would be a product of a proportionally larger inflow of immigrants or "immigrant shocks". Therefore, we cannot support this finding made by Entorf & Lange (2019) using data from Germany. Instead, our analysis points to the peak being followed by a decrease in hate crime among the municipalities subject to "immigrant shocks". We propose that this may be due to faster assimilation or hate crime perpetrators moving away. However, the robustness of this result can be questioned as evidenced by our robustness checks. Furthermore, our analysis is subject to several data limitations, both when it comes to hate crime and the inflow of immigrants, which makes causal inference difficult.

Arguably our most interesting findings are those with respect to income inequality. We observe that the 2015 surge in hate crime rates is only present among the municipalities with a high degree of income inequality. Hence, we can add high income inequality to one of the economic conditions conducive to hate crime. This is in line with our hypothesis regarding the strain theory's explanatory value for hate crime.

Our findings suggests that prioritizing integration efforts is crucial for limiting hate crime in Swedish municipalities, when faced with a inflow of immigrants. Furthermore, our results suggest that special attention should be directed towards municipalities with high levels of income inequality.

While this paper contributes to interesting insights into which the structural factors underlying hate crime rates are, there exists a lot of possible extensions. Other factors that could be of interest when attempting to understand what environments are conducive to hate crime are political and religious beliefs. It is important that more research in this field is conducted to understand how we can efficiently tackle hate crime and induce acceptance among neighbors.

References

Agnew, R. (2001). Building on the Foundation of General Strain Theory: Specifying the Types of Strain Most Likely to Lead to Crime and Delinquency. *Journal of Research in Crime and Delinquency*, *38*(4), 310–361.

Arbetsmarknadsdepartementet (2013). Vanliga Påståenden om Invandring. Retrived from https://web.archive.org/web/20130705163043/http://www.regeringen.se/sb/d/17468.

Becker, G. S. (1968). Crime and Punishment: An Economic Approach. *Journal of Political Economy*, *76*(2), 169–217.

Dustmann, C., Fabbri, F., & Preston, I. (2011). Racial Harassment, Ethnic Concentration and Economic Conditions. *The Scandinavian Journal of Economics*, *113*(3), 689–711.

Entorf, H., & Lange, M. (2019). Refugees Welcome? Understanding the Regional Heterogeneity of Anti-Foreigner Hate Crimes in Germany. ZEW - Centre for European Economic Research Discussion Paper No. 19-005.

Esaiasson, P., Martinsson, J., & Sohlberg, J. (2016). Flyktingkrisen och Medborgarnas Förtroende För Samhällets Institutioner: En Forskarrapport. https://www.msb.se/sv/publikationer/flyktingkrisen-och-medborgarnas-fortroende-for-samhallets-institutioner--en-forskarrapport/.

European Union Agency of Fundamenal Rights (2016). Current Migration Status in the EU: Hate Crime. https://fra.europa.eu/sites/default/files/fra_uploads/fra-2016-november-monthly-focus-hate-crime_en.pdf. Publication office in Luxembourg.

Forselius, N., & Westerberg, S. (2019). Hatbrott 2018, Statistik över polisanmälda brott med identiferade hatbrottsmotiv (Report 2019:13). *Brottsförebyggande Rådet*. https://bra.se/download/18.bbb8316de12eace227048/1614334407813/2019_13_Hatbrott%20_2018.pdf.

Friedberg, R. M. (2000). You Can't Take It With You? Immigrant Assimilation and the Portability of Human Capital. *Journal of Labor Economics*, *18*(2), 221–251.

Gale, L., Heath, W. C., & Ressler, R. (2002). An Economic Analysis of Hate Crime. *Eastern Economic Journal*, *28*(2), 203–216.

Grattet, R. (2009). The Urban Ecology of Bias Crime: A Study of Disorganized and Defended Neighborhoods. *Social Problems*, *56*(1), 132–150.

Green, D. P., Glaser, J., & Rich, A. (1998a). From Lynching to Gay Bashing: The Elusive Connection Between Economic Conditions and Hate Crime . *Journal of Personality and Social Psychology*, *75*(1), 82–92.

Green, D. P., McFalls, L. H., & Smith, J. K. (2001). Hate Crime: An Emergent Research Agenda. *Annual Review of Sociology*, *27*, 479–504.

Green, D. P., Strolovitch, D. Z., & Wong, J. S. (1998b). Defended Neighborhoods, Integration, and Racially Motivated Crime. *American Journal of Sociology*, *104*(2), 372–403.

Hipp, J. R. (2007). Income Inequality, Race, and Place: Does the Distribution of Race and Class Within Neighborhoods Affect Crime Rates? *Criminology*, *45*(3), 665–697.

Justitiedepartementet (SFS 1962:700). Swedish Penal Code. https://www.riksdagen.se/sv/dokument-lagar/dokument/svensk-forfattningssamling/brottsbalk-1962700_sfs-1962-700.

Kahn-Lang, A., & Lang, K. (2020). The Promise and Pitfalls of Differences-in-Differences: Reflections on 16 and Pregnant and Other Applications. *Journal of Business Economic Statistics*, *38*(3), 613–620.

Kelly, M. (2000). Income Inequality and Crime. *The Reveiw of Economics and Statistics*, *82*(4), 530–539.

Lindgren, S. (2019). Income Inequality and Crime: Evidence from Sweden. [Master Thesis, University of Gothenburg, School of Business, Economics and Law].

Merton, R. K. (1938). Social Structure and Anomie. *American Sociological Review*, *3*(5), 572–682.

Müller, T., & Valdez, S. (2014). Right-wing populism and social distance towards muslims in sweden: Results from a nation-wide vignette study. *Institute for Analytical Sociology Working Paper*.

Nilsson, A. (2004). Income Inequality and Crime: The Case of Sweden. *45*(3), 665–697. Institute for Labour Market Policy Evaluation, Swedish Ministry of Industry, Employment and Communications, Working Paper no 2004:6.

Noel, N., Stewart III, D., & Wright, J. (2019). The economic impact of closing the racial wealth gap, *McKinsey Institute for Black Economic Mobility*. https://www.mckinsey

.com/industries/public-and-social-sector/our-insights/the-economic-impact-of-closing-the-racial-wealth-gap#.

Roth, J. (2022). Pretest with caution: Event-study estimates after testing for parallel trends. *American Economic Review: Insights*, *4* (3), 305–22.
URL https://www.aeaweb.org/articles?id=10.1257/aeri.20210236

Roth, J., & Sant'Anna, P. H. (2023). When Is Parallel Trends Sensitive to Functional Form? *Econometrica*, *91* (2), 737–747.

Singh, A. S., & Masuku, M. B. (2014). Sampling techniques determinantion of sample size in applied statistics research: An overview. *Eastern Economic Journal*, *11* (2), 1–22.

Suttles, G. D. (1972). *The Social Construction of Communities*. Chicago: University of Chicago Press.

Walters, M. A. (2011). A General Theories of Hate Crime? Strain, Doing Difference and Self Control. *Critical Crimonology - Springer Journal*, *19*, 313–330.

Westerberg, S., & Faramarzi, A. I. (2018). *Självrapporterad Utsatthet för Hatbrott : Analys Utifrån Nationella Trygghetsundersökningen 2006-2017*. Stockholm: Brottsförebyggande rådet.

Whitney K. Newey, K. D. W. (1987). A Simple, Positive Semi-Definite, Heteroskedasticity and Autocorrelation Consistent Covariance Matrix. *Econometrica*, *55* (3), 703–708.

Appendix

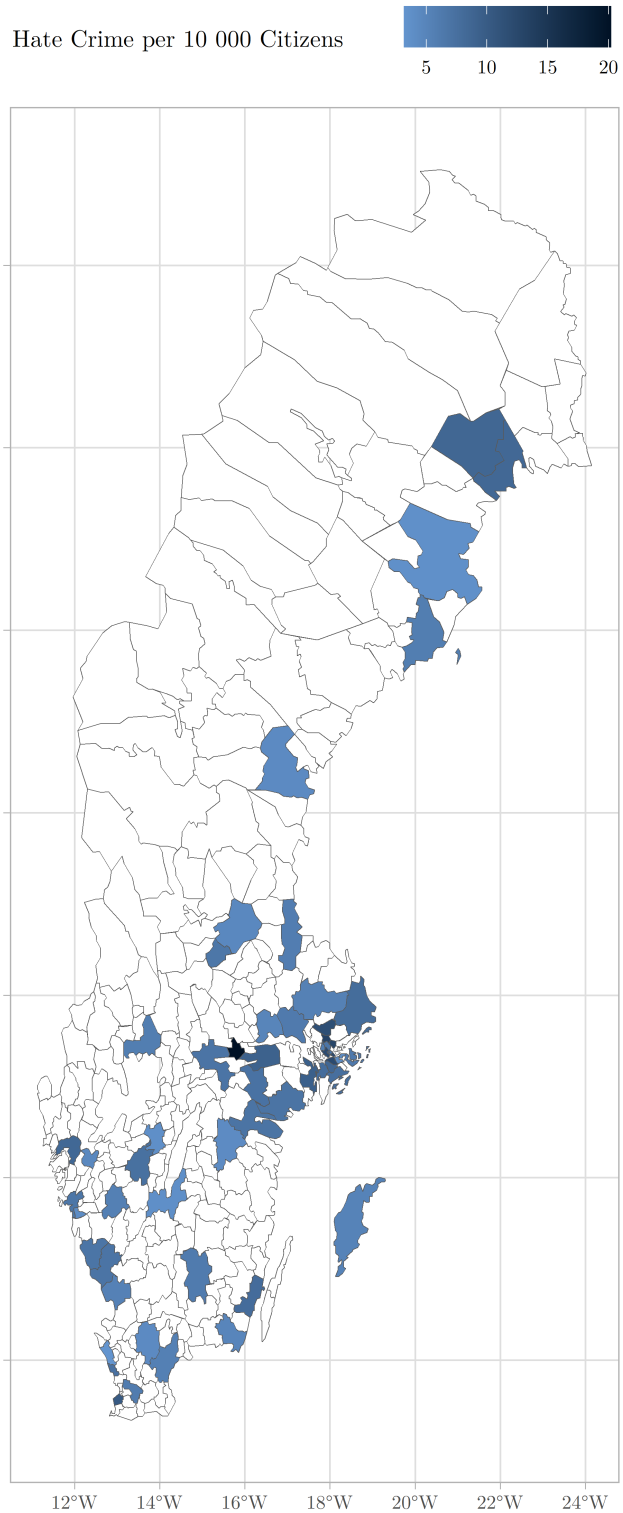
Table A.1

Municipalities in the Dataset

1	Arboga	31	Malmö
2	Boden	32	Mölndal
3	Borlänge	33	Nacka
4	Borås	34	Norrköping
5	Botkyrka	35	Norrtälje
6	Enköping	36	Nyköping
7	Eskilstuna	37	Nässjö
8	Falkenberg	38	Sigtuna
9	Falköping	39	Skellefteå
10	Falun	40	Skövde
11	Gotland	41	Sollentuna
12	Gävle	42	Solna
13	Göteborg	43	Stockholm
14	Halmstad	44	Sundbyberg
15	Haninge	45	Sundsvall
16	Helsingborg	46	Södertälje
17	Huddinge	47	Trelleborg
18	Hässleholm	48	Trollhättan
19	Järfälla	49	Täby
20	Jönköping	50	Uddevalla
21	Kalmar	51	Umeå
22	Karlskrona	52	Upplands Väsby
23	Karlstad	53	Uppsala
24	Katrineholm	54	Varberg
25	Kristianstad	55	Värmdö
26	Landskrona	56	Västerås
27	Linköping	57	Växjö
28	Ludvika	58	Örebro
29	Luleå	59	Östersund
30	Lund		

Note: This is a list of the 59 municipalities in the dataset. The municipalities have at least one data point on hate crime per 10 000 citizens before and after 2015.

Figure A.1. Map of Municipalities in the Dataset



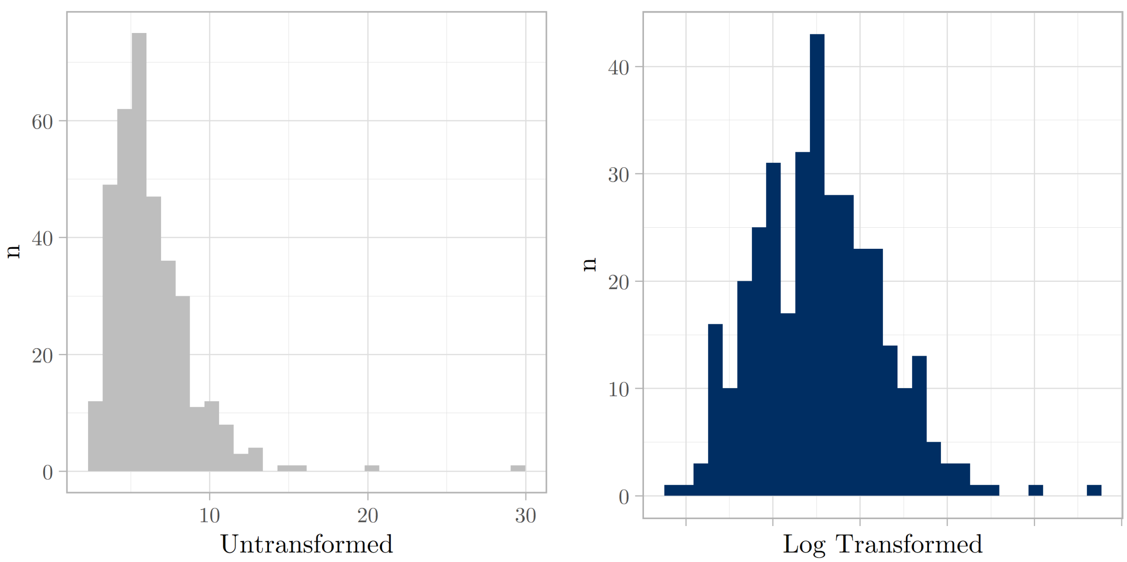
Note: The map shows the 59 municipalities in the dataset and their number of hate crime per 10 000 citizens in 2015. The municipalities have at least one data point on hate crime per 10 000 citizens before and after 2015. The data is taken from BRÅ.

Table A.2
Municipalities with the Highest and Lowest Hate Crime Rates

High Hate Crime			Low Hate Crime		
1	Arboga	20.20	1	Helsingborg	3.19
2	Upplands Väsby	13.13	2	Skellefteå	3.61
3	Täby	12.89	3	Skövde	3.73
4	Stockholm	12.19	4	Jönköping	3.75
5	Sigtuna	11.61	5	Mölndal	4.10

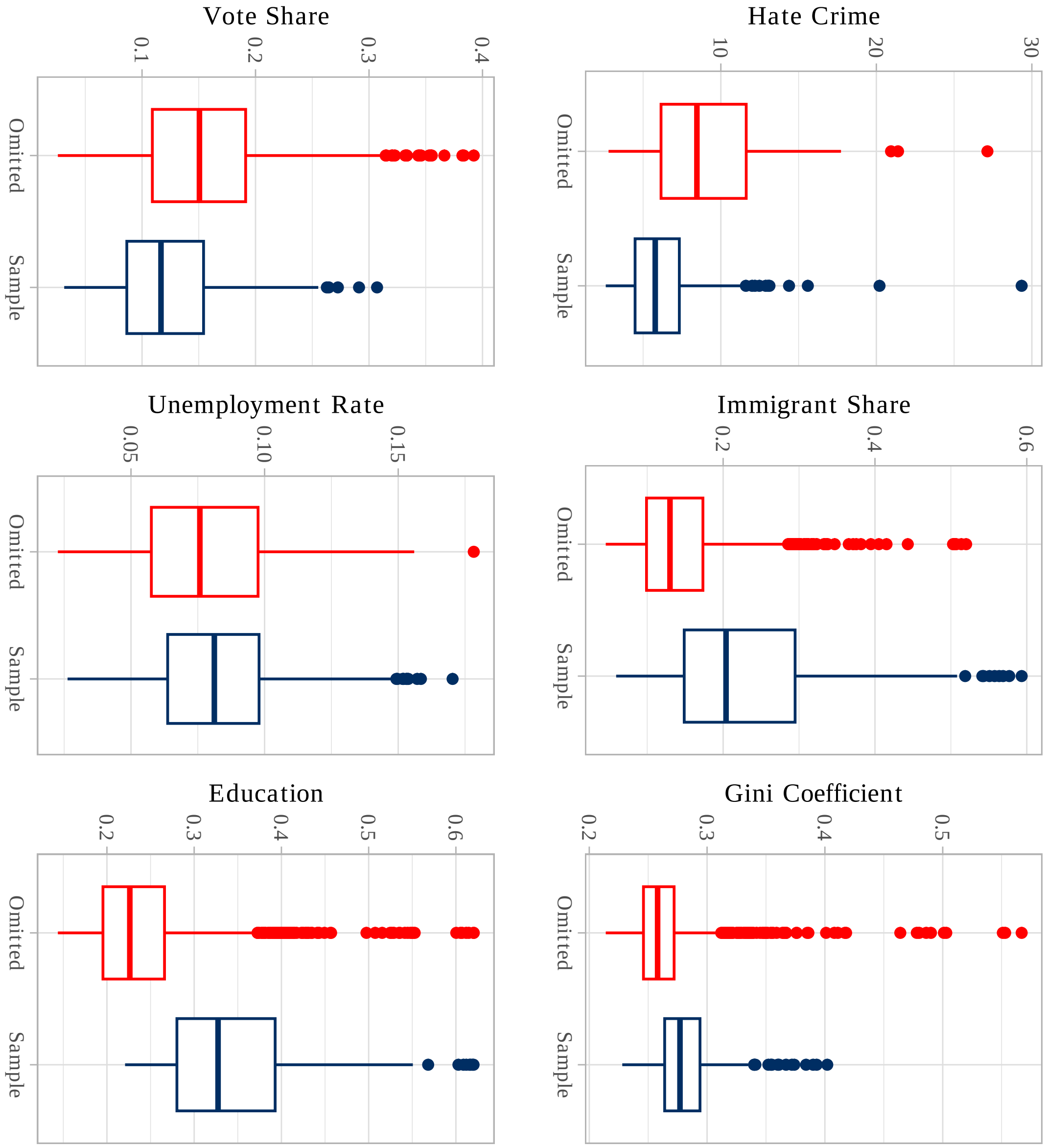
Note: This table presents the 10 municipalities with the highest and lowest hate crime per 10 000 citizens in 2015, among the 59 municipalities in the dataset. The data is taken from BRÅ and is presented without log transformations.

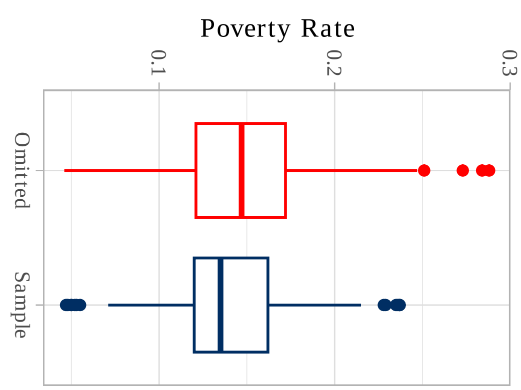
Figure A.2. Histogram of Hate Crime per 10 000 Citizens



Note: The histograms plots the density of hate crime using untransformed and log transformed data from the 59 municipalities in the dataset. The number of reported hate crime is taken from BRÅ and the census data used when creating the per 10 000 citizens is taken from Statistics Sweden database.

Figure A.3. Our Sample vs. Omitted Municipalities





Note: Box plot of untransformed variables for the 59 municipalities in our dataset and the 221 omitted municipalities. Hate Crime data that exists for the municipalities that are omitted on the basis of insufficient data before and after the Migrant Crisis. However most municipalities that are omitted have no existing data points on hate crime.

Table A.3
Correlation Table

	Hate Crime	*Im. Share*	*Gini*	*Pov. Rate*	*Vote Share*	*Unemp. Rate*	*Education*
Hate Crime	1.00						
Immigrant Share	0.36***	1.00					
Gini	0.11**	0.42***	1.00				
Poverty Rate	0.23***	0.28***	-0.08	1.00			
Vote Share	0.10	0.08	-0.08	0.43***	1.00		
Unemployment Rate	0.16***	0.25***	-0.25***	0.80***	0.29***	1.00	
Education	-0.18***	0.16***	0.59***	-0.33***	-0.41***	-0.42***	1.00

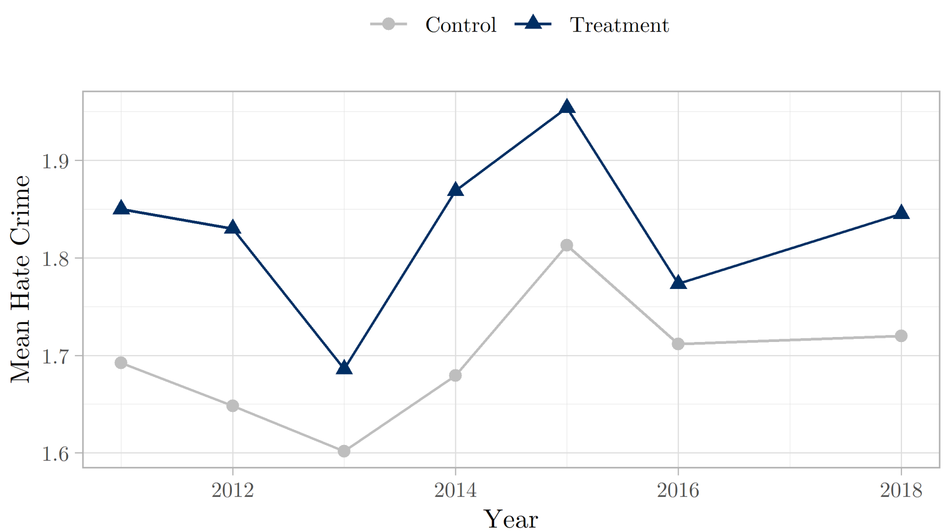
Note: This table presents the Pearson's correlation coefficient between the variables in our dataset. Statistical significance is attributed based on p-values as follows: *p<0.1; **p<0.05;***p<0.01.

Table A.4
Averages by Treatment Status

	Control	Treatment	Difference
Hate Crime	1.699	1.833	0.134^{***}
Gini	0.251	0.246	-0.004
Vote Share	0.105	0.127	0.023^{***}
Immigrant Share	0.184	0.220	0.036^{***}
Unemployment Rate	0.075	0.085	0.01^{***}
Education	0.314	0.277	-0.036^{***}
Poverty Rate	0.124	0.135	0.011^{***}

Note: The table consist of averages by treatment status. It is based municipalities by treatment and control status assigned by *Immigrant Shock* which is an indicator variable equal to 1 for each year t, if the municipality i experienced an above median change in immigrant share from 2014 to 2015 the municipality is assigned to the treatment group. All variables are log transformed.

Figure A.4. Average Hate Crime Rate by Treatment Status



Note: The graph plots the log of mean hate crime rates for the treatment and control group (N=353). Data on hate crime comes from BRÅ's hate crime reports and the census data from Statistics Sweden.

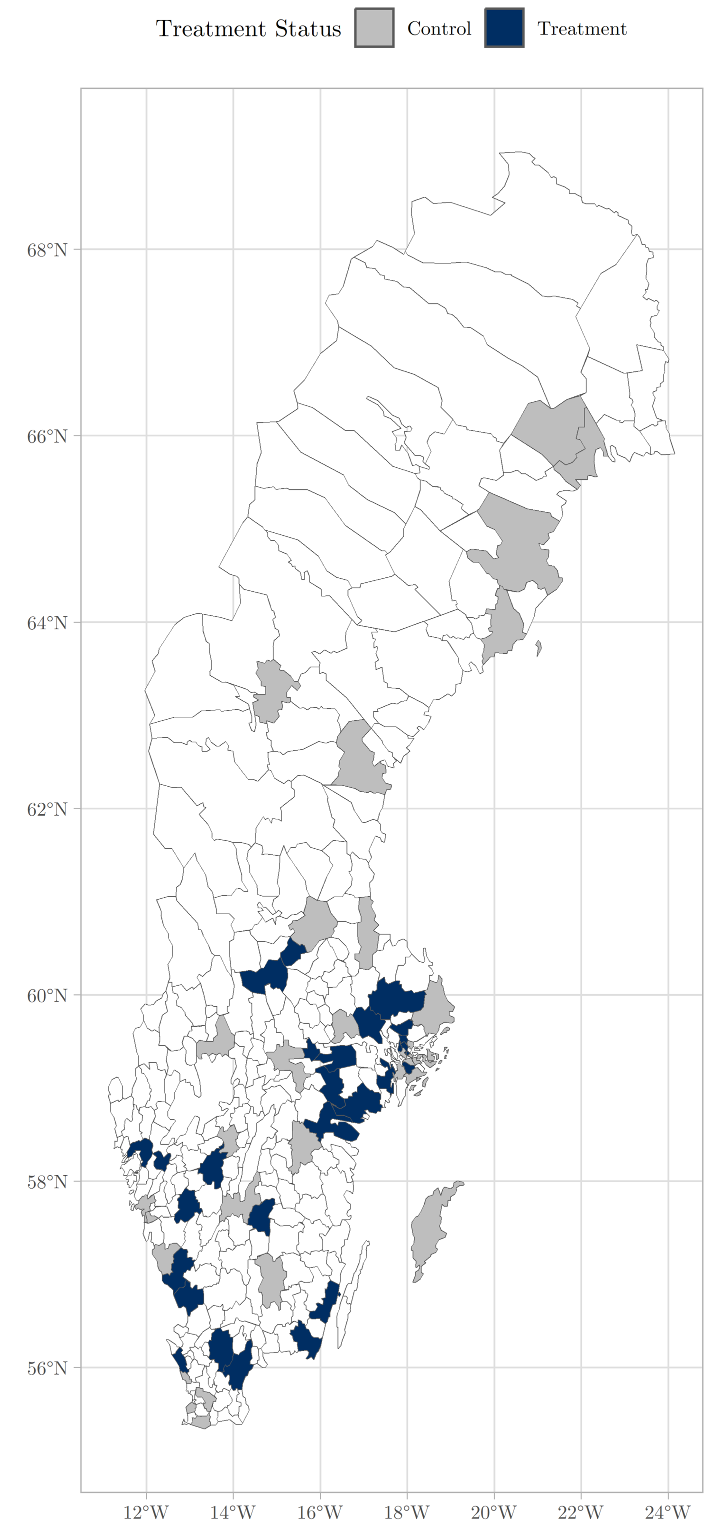
Table A.5

Municipalities by Treatment Status

	Treatment		Control
1	Arboga	1	Boden
2	Borlänge	2	Botkyrka
3	Borås	3	Falun
4	Enköping	4	Gotland
5	Eskilstuna	5	Gävle
6	Falkenberg	6	Göteborg
7	Falköping	7	Haninge
8	Halmstad	8	Jönköping
9	Helsingborg	9	Karlstad
10	Huddinge	10	Landskrona
11	Hässleholm	11	Linköping
12	Järfälla	12	Luleå
13	Kalmar	13	Lund
14	Karlskrona	14	Malmö
15	Katrineholm	15	Mölndal
16	Kristianstad	16	Nacka
17	Ludvika	17	Norrtälje
18	Norrköping	18	Skellefteå
19	Nyköping	19	Skövde
20	Nässjö	20	Stockholm
21	Sigtuna	21	Sundsvall
22	Sollentuna	22	Trelleborg
23	Solna	23	Täby
24	Sundbyberg	24	Umeå
25	Södertälje	25	Varberg
26	Trollhättan	26	Värmdö
27	Uddevalla	27	Västerås
28	Upplands Väsby	28	Växjö
29	Uppsala	29	Örebro
		30	Östersund

Note: The table lists municipalities by their treatment status. Treatment is assigned based on $Immigrant\ Shock_i$, which is equal to 1 if municipality i experienced an above median change in immigrant share from 2014 to 2015.

Figure A.5. Map of Municipalities by Treatment Status



Note: The map shows the municipalities in our sample and their treatment status based on the median change in the immigrant share (census data from Statistics Sweden). The municipalities have at least one data point on hate crime before and after 2015.

Table A.6

Hate Crime and the Change in Ethnic Composition (*Immigrant Share* < 25%)

	Dependent Variable: *Hate Crime*		
	(1)	(2)	(3)
2011	0.11 (0.07)	0.09 (0.12)	0.01 (0.16)
2012	0.01 (0.08)	−0.01 (0.10)	−0.05 (0.11)
2013	−0.07 (0.06)	−0.09 (0.09)	−0.10 (0.08)
2015	0.18** (0.09)	0.21** (0.10)	0.23** (0.09)
2016	0.12 (0.08)	0.19* (0.11)	0.25* (0.14)
2018	0.13 (0.09)	0.27* (0.15)	0.40 (0.24)
Gini		−1.11 (3.71)	−0.88 (3.48)
Vote Share		3.07 (2.31)	4.92 (3.33)
Immigrant Share		−11.08* (5.63)	−13.00* (7.61)
Unemployment Rate			−0.68 (5.50)
Education			−10.99 (9.62)
Poverty Rate			2.00 (5.71)
2011 × *Immigrant Share*	−0.13 (0.10)	−0.16 (0.10)	−0.12 (0.11)
2012 × *Immigrant Share*	−0.02 (0.11)	−0.06 (0.13)	−0.05 (0.13)
2013 × *Immigrant Share*	−0.08 (0.12)	−0.10 (0.12)	−0.09 (0.12)
2015 × *Immigrant Share*	−0.09 (0.13)	−0.04 (0.13)	−0.04 (0.13)
2016 × *Immigrant Share*	−0.23* (0.12)	−0.13 (0.13)	−0.12 (0.13)
2018 × *Immigrant Share*	−0.12 (0.13)	−0.01 (0.14)	−0.02 (0.14)
Time Fixed Effects	✓	✓	✓
Municipality Fixed Effects	✓	✓	✓
Clustered Standard Errors	✓	✓	✓
N	224	224	224
R^2	0.61	0.62	0.62

Note: This table presents the results from the following event study: $Hate\ Crime_{it} = \alpha_i + \sum \beta_t \cdot Immigrant\ Shock_i + \delta \cdot X_{it} + \lambda_t + \epsilon_{it}$. $Hate\ Crime_{it}$ is the log of hate crime per 10 000 citizens. $Immigrant\ Shock_i$ is an indicator variable equal to 1 for each year t, if the municipality i experienced an above median change in immigrant share from 2014 to 2015. X_{it} is a vector of covariates. Covariates are all transformed as follows log(1+x). α_i and λ_t are municipality and time fixed effects respectively. The sample consists of yearly observations for the period 2011-2016 and 2018. 2014 is omitted. The dataset contains 40 municipalities with data before and after 2015 and with an immigrant share below 25%. T-statistics are computed using clustered standard errors at the municipality level which are reported in parentheses. Statistical significance is attributed based on p-values as follows: *p<0.1; **p<0.05;***p<0.01.

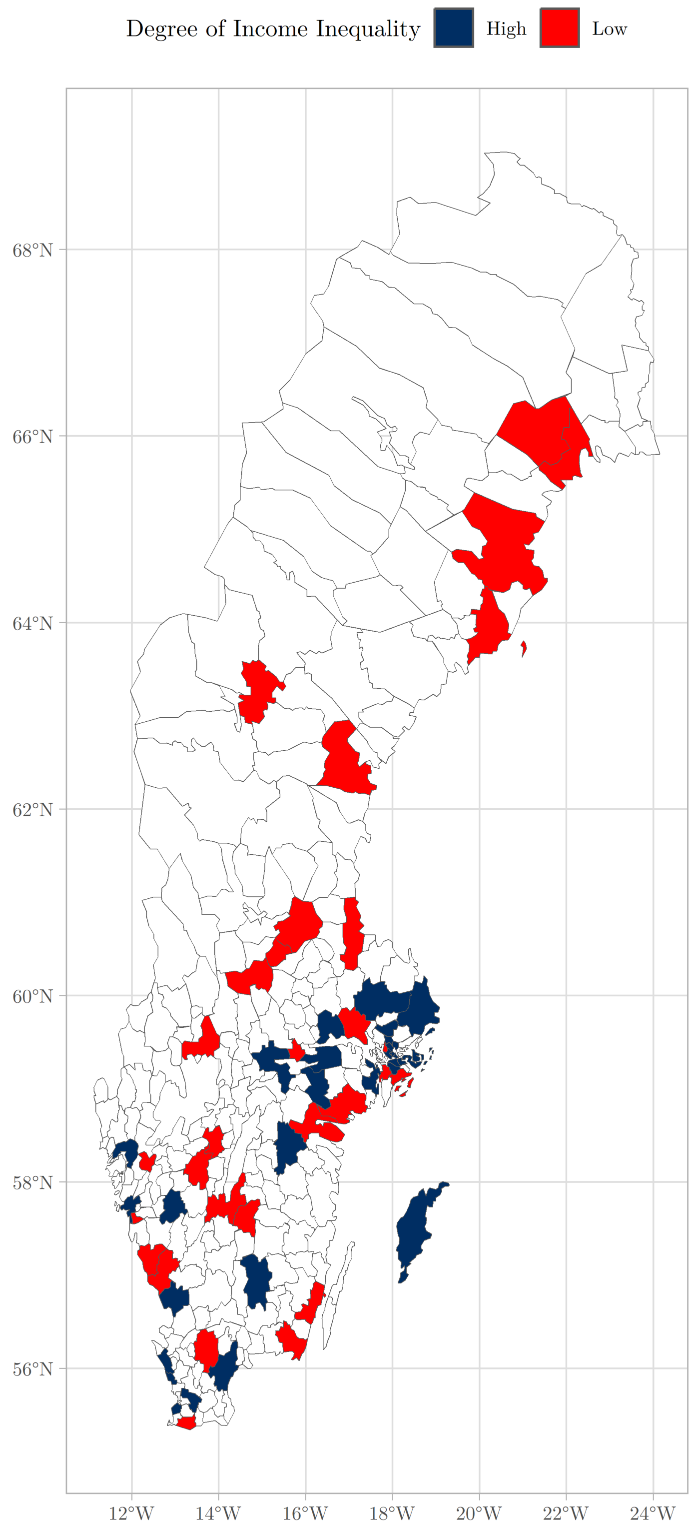
Table A.7

Municipalities by Degree of Income Inequality

	High Income Inequality		Low Income Inequality
1	Enköping	1	Arboga
2	Göteborg	2	Boden
3	Halmstad	3	Borlänge
4	Haninge	4	Borås
5	Helsingborg	5	Botkyrka
6	Huddinge	6	Eskilstuna
7	Järfälla	7	Falkenberg
8	Jönköping	8	Falköping
9	Linköping	9	Falun
10	Luleå	10	Gotland
11	Lund	11	Gävle
12	Mölndal	12	Hässleholm
13	Nacka	13	Kalmar
14	Norrtälje	14	Karlskrona
15	Nyköping	15	Karlstad
16	Sigtuna	16	Katrineholm
17	Skövde	17	Kristianstad
18	Sollentuna	18	Landskrona
19	Solna	19	Ludvika
20	Stockholm	20	Malmö
21	Sundbyberg	21	Norrköping
22	Sundsvall	22	Nässjö
23	Täby	23	Skellefteå
24	Upplands Väsby	24	Södertälje
25	Uppsala	25	Trelleborg
26	Varberg	26	Trollhättan
27	Värmdö	27	Uddevalla
28	Västerås	28	Umeå
29	Växjö	29	Örebro
		30	Östersund

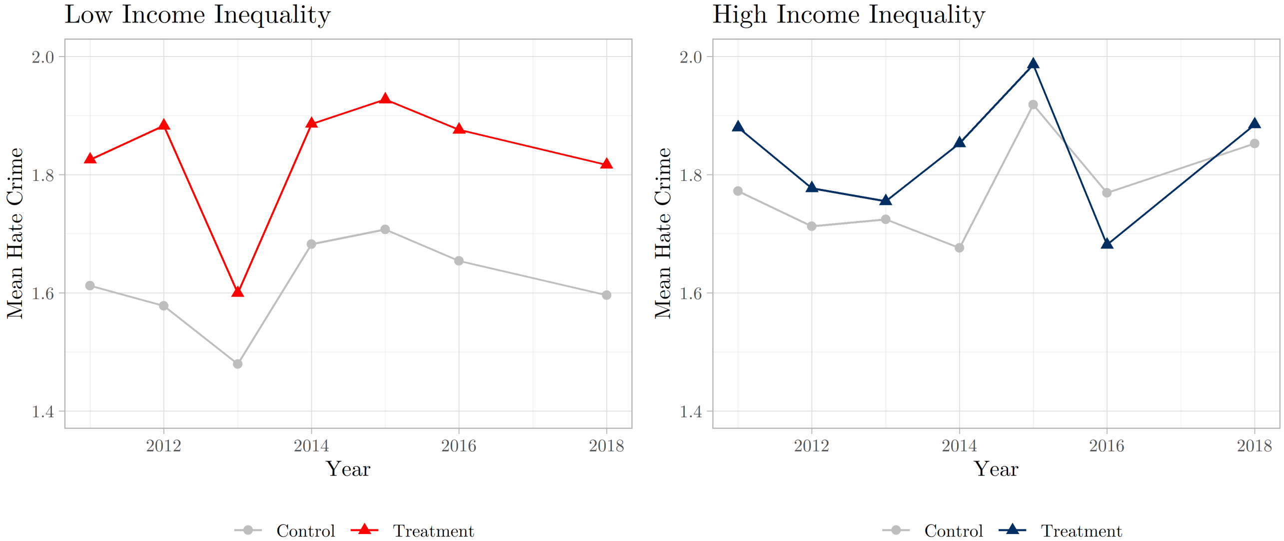
Note: The table consist of municipalities by high and low income inequality based on the Gini coefficient in 2014. An above median Gini coefficient implies that the municipality becomes part of the high income inequality group and vice versa. The Gini coefficient is taken from Statistics Sweden.

Figure A.6. Map of Municipalities by Income Inequality



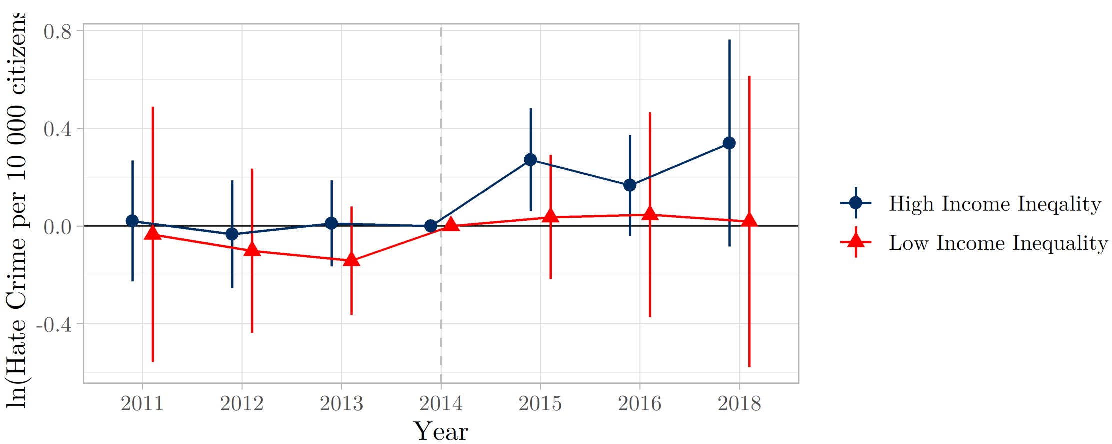
Note: The map shows the degree of income inequality in our sample. An above median Gini coefficient in 2014 implies high income inequality (taken from Statistics Sweden). The municipalities have at least one data point on hate crime before and after 2015.

Figure A.7. Average Hate Crime Rate by Treatment Status and Income Inequality



Note: The graph plots the log of mean hate crime rates for the treatment and control group based on their level of income inequality. The split is based on municipalities with above (high) and below (low) median Gini coefficient in 2014. Data on hate crime comes from BRÅ's hate crime reports and census data, and Gini coefficients comes from Statistics Sweden. *N=159* in the low income inequality group and *N=187* in the high income inequality group.

Figure A.8. Coefficient Plot of Year on Hate Crime by Income Inequality



Note: Event study plot plot of λ_t from Table 4.2 depicting the following regression: $Hate\ Crime_{g,it} = \alpha_{g,i} + \sum \beta_{g,t} \cdot Immigrant\ Shock_{g,i} + \delta \cdot X_{g,it} + \lambda_{g,t} + \epsilon_{g,it}$ estimated separately for the low and high income inequality groups. *N=159* in the low income inequality group and *N=187* in the high income inequality group.

Table A.8
Hate Crime and Change in Ethnic Composition (Untransformed)

	Dependent Variable: *Hate Crime*		
	(1)	(2)	(3)
2011	0.10 (0.33)	−0.90 (0.61)	−0.87 (0.80)
2012	−0.25 (0.34)	−0.98** (0.49)	−0.98 (0.60)
2013	−0.31 (0.28)	−0.67* (0.37)	−0.70* (0.38)
2015	0.79* (0.46)	1.07** (0.51)	1.08** (0.50)
2016	0.28 (0.41)	0.96* (0.57)	0.88 (0.63)
2018	0.27 (0.44)	1.78** (0.82)	1.66 (1.02)
Gini		−0.35 (12.49)	1.13 (12.36)
Vote Share		−8.36 (9.10)	−5.43 (9.77)
Immigrant Share		−41.56** (16.21)	−39.27** (18.01)
Unemployment Rate			−1.11 (17.39)
Education			−6.36 (29.91)
Poverty Rate			−15.82 (24.43)
2011 × *Immigrant Shock*	−0.28 (0.56)	−0.81 (0.59)	−0.85 (0.61)
2012 × *Immigrant Shock*	−0.13 (0.60)	−0.50 (0.66)	−0.51 (0.67)
2013 × *Immigrant Shock*	−0.79 (0.55)	−0.99* (0.57)	−0.99* (0.58)
2015 × *Immigrant Shock*	−0.40 (0.73)	−0.17 (0.73)	−0.14 (0.72)
2016 × *Immigrant Shock*	−1.29* (0.75)	−0.85 (0.73)	−0.81 (0.70)
2018 × *Immigrant Shock*	−0.42 (0.66)	0.22 (0.70)	0.26 (0.68)
Time Fixed Effects	✓	✓	✓
Municipality Fixed Effects	✓	✓	✓
Clustered Standard Errors	✓	✓	✓
N	353	353	353
R^2	0.71	0.72	0.72

Note: This table presents the results from the following event study: $Hate\ Crime_{it} = \alpha_i + \sum \beta_t \cdot Immigrant\ Shock_{it} + \delta \cdot X_{it} + \lambda_t + \epsilon_{it}$. $Hate\ Crime_{it}$ is hate crime per 10 000 citizens. *Immigrant Shock* is an indicator variable equal to 1 for each year t, if the municipality i experienced an above median change in immigrant share from 2014 to 2015. X_{it} is a vector of covariates. α_i and λ_t are municipality and time fixed effects respectively. The sample consists of yearly observations for the period 2011-2016 and 2018. 2014 is omitted. The dataset contains 59 municipalities with data before and after 2015. T-statistics are computed using clustered standard errors at the municipality level which are reported in parentheses. Statistical significance is attributed based on p-values as follows: *p<0.1; **p<0.05;***p<0.01.

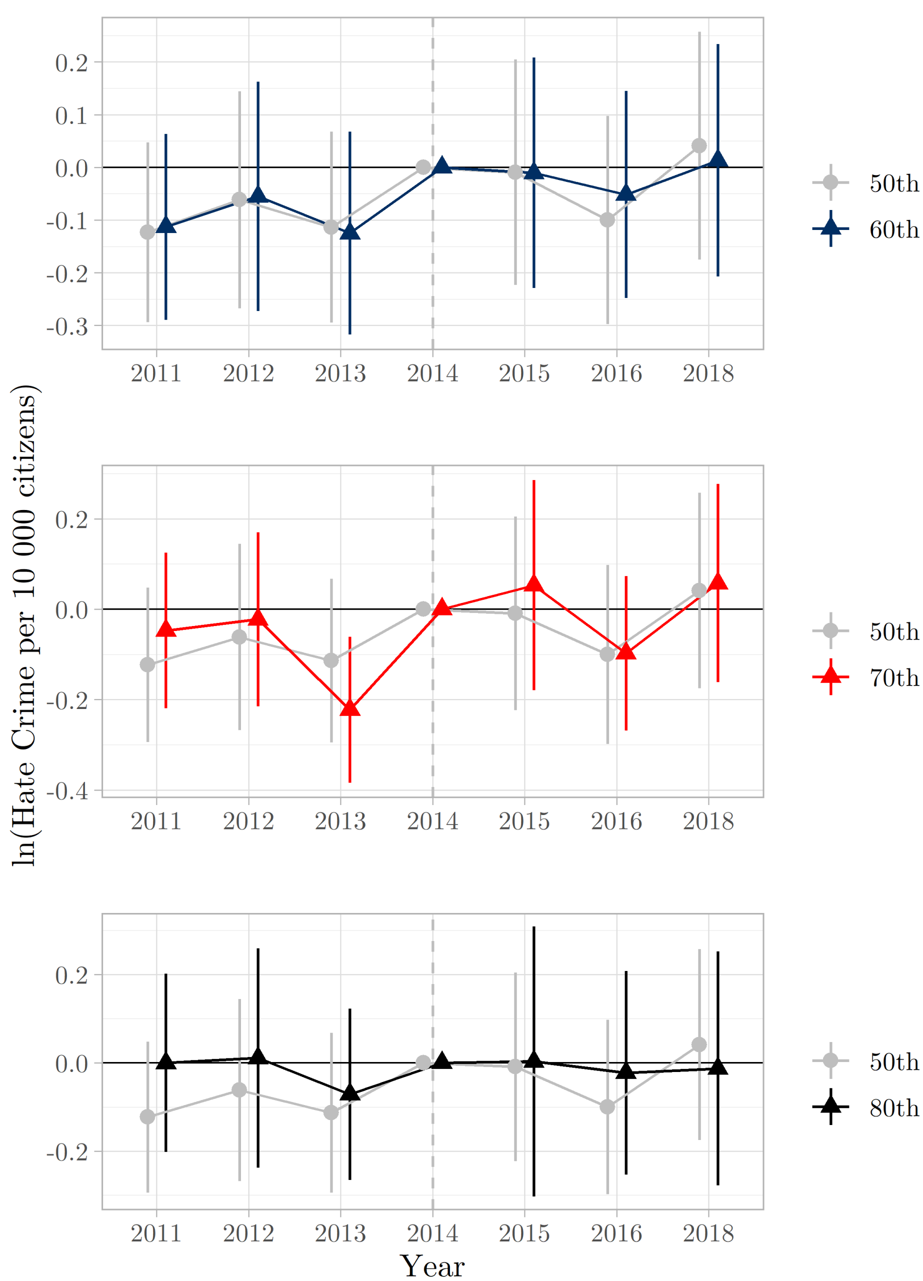
Table A.9

Hate Crime and the Change in Ethnic Composition (HAC errors)

	Dependent Variable: *Hate Crime*		
	(1)	(2)	(3)
2011	0.05 (0.05)	−0.08 (0.09)	−0.08 (0.12)
2012	−0.03 (0.06)	−0.13 (0.08)	−0.12 (0.09)
2013	−0.05 (0.05)	-0.10^{*} (0.06)	−0.10 (0.06)
2015	0.14^{**} (0.07)	0.18^{**} (0.07)	0.18^{**} (0.07)
2016	0.05 (0.07)	0.16^{*} (0.08)	0.16^{*} (0.09)
2018	0.05 (0.07)	0.27^{**} (0.12)	0.27^{*} (0.15)
Gini		−0.62 (2.54)	−0.80 (2.63)
Vote Share		−0.36 (1.88)	−0.50 (2.17)
Immigrant Share		-9.08^{***} (3.50)	-9.11^{**} (4.07)
Unemployment Rate			−1.25 (2.76)
Education			−0.45 (5.49)
Poverty Rate			1.40 (3.21)
2011 × *Immigrant Shock*	−0.05 (0.07)	-0.12^{*} (0.07)	-0.13^{*} (0.07)
2012 × *Immigrant Shock*	−0.01 (0.08)	−0.06 (0.09)	−0.06 (0.09)
2013 × *Immigrant Shock*	−0.09 (0.08)	−0.11 (0.08)	−0.11 (0.08)
2015 × *Immigrant Shock*	−0.05 (0.09)	−0.01 (0.09)	−0.01 (0.09)
2016 × *Immigrant Shock*	-0.17^{**} (0.08)	−0.10 (0.09)	−0.10 (0.09)
2018 × *Immigrant Shock*	−0.06 (0.08)	0.04 (0.09)	0.04 (0.09)
Time Fixed Effects	✓	✓	✓
Municipality Fixed Effects	✓	✓	✓
Clustered Standard Errors	×	×	×
N	353	353	353
R^2	0.66	0.67	0.67

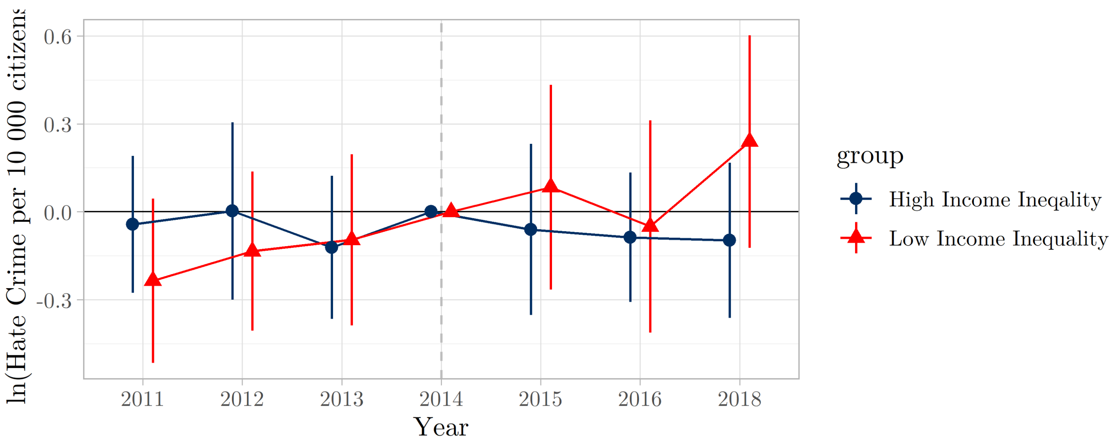
Note: This table presents the results from the following event study: $Hate\ Crime_{it} = \alpha_i + \sum \beta_t \cdot Immigrant\ Shock_{it} + \delta \cdot X_{it} + \lambda_t + \epsilon_{it}$. $Hate\ Crime_{it}$ is the log of hate crime per 10 000 citizens. *Immigrant Shock* is an indicator variable equal to 1 for each year t, if the municipality i experienced an above median change in immigrant share from 2014 to 2015. X_{it} is a vector of covariates. Covariates are all transformed as follows log(1+x). α_i and λ_t are municipality and time fixed effects respectively. The sample consists of yearly observations for the period 2011-2016 and 2018. 2014 is omitted. The dataset contains 59 municipalities with data before and after 2015. T-statistics are computed using HAC errors at the municipality level which are reported in parentheses. Statistical significance is attributed based on p-values as follows: *p<0.1; **p<0.05;***p<0.01.

Figure A.9. Event Study Plot of Immigrant Shock on Hate Crime with Staggered Cutoffs



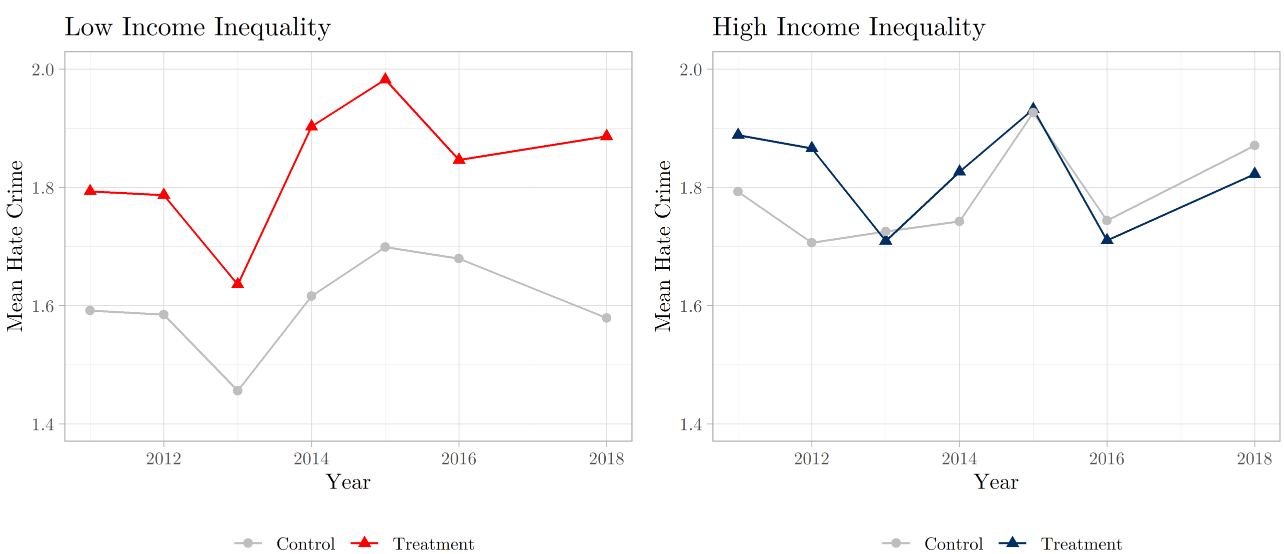
Note: Event study plot of β_t from Table 4.3 displaying results from the following regression: $Hate\ Crime_{it} = \alpha_i + \sum \beta_t \cdot Immigrant\ Shock_i + \delta \cdot X_{it} + \lambda_t + \epsilon_{it}$ with different cutoffs for treatment. A municipality is assigned to the treatment group if it experienced an above a given percentile change in immigrant share from 2014 to 2015.

Figure A.10. Event Study Plot of Immigrant Shock on Hate Crime by Income Inequality (Income Ratio)



Note: Event study plot plot of λ_t from Table 4.4 depicting the following regression: $Hate\ Crime_{g,it} = \alpha_{g,i} + \sum \beta_{g,t} \cdot Immigrant\ Shock_{g,i} + \delta \cdot X_{g,it} + \lambda_{g,t} + \epsilon_{g,it}$ estimated separately for the low (*N=159*) and high (*N=187*) income inequality groups.

Figure A.11. Average Hate Crime Rate by Treatment Status and Income Inequality (Income Ratio)



Note: The graph plots the log of mean hate crime rates for the treatment and control group based on their level of income inequality. The split is based on municipalities with above (high) and below (low) median income ratio in 2014. Data on hate crime comes from BRÅ's hate crime reports and census data, and income ratios comes from Statistics Sweden. *N=159* in the low income inequality group and *N=187* in the high income inequality group.