CEPA DP No. 31

MAY 2021

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CEPA Discussion Papers

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ISSN (online) 2628-653X

CEPA Discussion Papers can be downloaded from RePEc
https://ideas.repec.org/s/pot/cepadp.html

Published online at the Institutional Repository of the University of Potsdam
https://doi.org/10.25932/publishup-50797

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Hate is too great a burden to bear: Hate crimes and the mental health of refugees*

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ABSTRACT

Against a background of increasing violence against non-natives, we estimate the effect of hate crime on refugees' mental health in Germany. For this purpose, we combine two datasets: administrative records on xenophobic crime against refugee shelters by the Federal Criminal Office and the IAB-BAMF-SOEP Survey of Refugees. We apply a regression discontinuity in time design to estimate the effect of interest. Our results indicate that hate crime has a substantial negative effect on several mental health indicators, including the Mental Component Summary score and the Patient Health Questionnaire-4 score. The effects are stronger for refugees with closer geographic proximity to the focal hate crime and refugees with low country-specific human capital. While the estimated effect is only transitory, we argue that negative mental health shocks during the critical period after arrival have important long-term consequences.

Keywords: mental health, hate crime, migration, refugees, human capital
JEL Codes: I10, J15, J24, F22, O15

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* The authors are grateful for valuable feedback from Eli Berman, Marco Caliendo, Gordon Dahl, Yvonne Giesing, Katrine Løken, Daniel Schnitzlein, Carsten Schroeder as well as the audience of presentations at the BENA Workshop 2019, the DIW Berlin, the DIW Graduate Center Workshop 2019, Essen Health Conference 2020, UC San Diego, and the ZEW Workshop on Immigration, Integration and Attitudes. Daniel is especially grateful for Prashant Bharadwaj's hospitality and valuable feedback during his research stay at UC, San Diego. We are also very thankful for Tobias Scheckel's valuable research assistance. The Socio-Economic Panel data can be made available on-site via the research data center of the Socio-Economic Panel group at the DIW Berlin. The authors declare they have no conflict of interest. Any remaining errors are our own.

1 Introduction

In the 2010s, the world witnessed two global phenomena: First, forced migration increased dramatically. The number of displaced persons almost doubled from about 42 million in 2008 to 75 million in 2018 (UNHCR, 2019). Second, the prevalence of hate crimes increased markedly. For example, CSTE (2017) reports that hate crime rose by 22 percent in the United States' six largest cities between 2016 and 2017.[1] This marks the third consecutive annual increase for the U.S., a pattern that has not been observed since 2004. We further observe an immense surge of violence against immigrants in Europe (Council of Europe, 2016). The arrival of about 800,000 refugees in Germany in 2015 was accompanied by a sudden increase in hate crimes against refugees (+ 400 percent, BKA).[2]

Hate crimes affect economic behavior through increased feelings of uncertainty, fear, and risk (Becker and Rubinstein, 2011). As a consequence, being victimized is associated with considerable costs in the economic, behavioral, and health domain (Bindler et al., 2020). For example, the costs of victimization amount to two to six percent of the gross domestic product in the U.S. (Chalfin, 2015). One important group which is regularly targeted by hate crimes are migrants, including refugees.

In the economic literature on migration, refugees are considered "permanent" migrants (Dustmann and Glitz, 2011). They remain in their destination country for a long period of time, unable or unwilling to return to their home country, where they are at risk of persecution or conflict. Given that permanent migrants can expect to accrue the returns to integration over longer time horizons than temporary migrants, their lifetime utility strongly therefore depends on their initial integration success (Dustmann and Glitz, 2011). For this reason, the potential consequences of adverse experiences due to hate crime are particularly consequential for refugees. Therefore, we answer the important question, what are the mental health costs of hate crimes for refugees?

[1] The increase for all thirteen surveyed cities was 19.9 percent for the period under consideration.

[2] A hate crime is defined as a crime against a specific group of individuals. Typically, hate crimes are committed because of the victim's race, gender, sexuality, color or ancestry (Gale et al., 2002).

Clearly, mental health shocks can have very detrimental long-term consequences for the victims of hate crimes. For instance, research on the psychological foundations of poverty stresses that reduced mental bandwidth increases the likelihood of worse economic choices. Worse economic choices in turn reduce mental bandwidth, resulting in a downward spiral (Schilbach et al., 2016). Similarly, we propose that hate crimes cause mental stress, which in turn may reduce refugees' mental bandwidth. This reduction in their mental bandwidth could impair refugees' economic decision-making ability. This could be particularly detrimental for refugees who fled severe conditions and are at the start of a life in a new country. In addition, a broad literature shows the adverse consequences of childhood exposure to stress and adverse conditions, including in-utero exposure to severe stress, on an individual's long-term life outcomes (Almond and Currie, 2011; Almond et al., 2018). This possibly impairs the life-trajectories of the next generation.[3]

To the best of our knowledge, the effect of hate crime on refugees' mental health has not been assessed in the existing literature. There are two reasons for this short-coming: First, we need data that combines both, representative information on refugees' mental health and their place of residence as well as information on a wide range of individual characteristics. Second, unobservable variables potentially bias the relationship between the occurrence of a hate crime and refugees' mental health. For instance, refugees may choose their place of residence endogenously based on regional characteristics, such as favorable economic conditions or existing ethnic networks, which may jointly determine both refugees' mental health and the occurrence of hate crime. Thus, it is essential to rely on an identification strategy that allows for the consistent estimation of the effect of hate crime on refugees' mental health. We advance the literature by solving these two problems.

To estimate the effect of hate crime on refugees' mental health, we rely on a regression

[3]For instance, Persson and Rossin-Slater (2018) show that prenatal exposure to stress increases take-up of ADHD medications during childhood and take up of depression medication later in life. Further, the infants' indirect in-utero exposure to the 9/11 attacks in the U.S. caused their birth weight to decrease by 15 grams, the likelihood of being born weighting less than 1,500 grams by 14%, and the likelihood of being born at less than 37 gestational weeks by 9% (Brown, 2020).

discontinuity in time (RD) design (Hausman and Rapson, 2018).[4] Using German counties that experience at least one hate crime against a refugee shelter, we assign each refugee the closest hate crime in the respective county measured in days elapsed since this focal hate crime. We then compare refugees' mental health immediately before and after an attack on the county level. Thus, the identification of our effect relies on the assumption that refugees' mental health is a continuous function of the number of elapsed days since the focal hate crime. We find strong support for this assumption, emphasizing the credibility of our research design.

Our empirical analysis relies on the unique IAB-BAMF-SOEP Survey of Refugees in Germany as well as geo-referenced administrative data on hate crimes from the Federal Criminal Office (BKA). The IAB-BAMF-SOEP Survey of Refugees is a representative survey of refugees who arrived in Germany between 2013 and 2016.[5] The data provides information on refugees' migration histories, background characteristics as well as overall living conditions and integration outcomes. Most importantly, it includes information on the exact interview date, the place of residence, and high-quality information on refugees' mental health. Our two mental health measures included in the IAB-BAMF-SOEP Survey of Refugees are the Mental Component Summary (MCS) score and the Patient Health Questionnaire-4 (PHQ-4) score. These are two well-established summary measures of general mental health as well as anxiety and depression, respectively. In order to link our analysis to related studies, including Deole (2019) and Steinhardt (2018), we also investigate the effect of hate crime on refugees' life satisfaction or intention to stay at the extensive margin.

Our second source of information is the BKA data, which reports hate crimes against refugee shelters. The BKA data contains time, place, the type of crime, and the crime's political motivation. This allows us to geo-reference the information and combine the administrative data on hate crimes with the IAB-BAMF-SOEP Survey of Refugees. The advantage

[4]In what follows, we refer to our research design as RD design.

[5]The IAB-BAMF-SOEP Sample of Refugees in Germany is part of the German Socio-Economic Panel (SOEP). We use version 34 of the SOEP. DOI: 10.5684/soep.v34.

of the administrative BKA data is that it contains information on hate crime directed toward refugees' shelters, which unambiguously represent hate crimes. This is an advantage over other data sources that do not differentiate between hate crime directed toward refugees or other residents with a migration background. Thus, we focus on refugee shelters since these are very salient forms of hate crime. In addition, data from non-administrative sources, such as newspapers, could suffer from endogenous coverage (Entorf and Lange, 2019).

Our results indicate that the experience of a hate crime reduces refugees' MCS score by 37% of a standard deviation. Similarly, hate crimes reduce refugees' PHQ-4 score by 28% of a standard deviation.[6] In contrast to existing studies that focus on economic migrants in Germany, such as Deole (2019) and Steinhardt (2018), we find no effect on refugees' life satisfaction. A potential reason for this may be the fact that refugees draw from different segments of the population in their home country than do economic migrants.[7] For instance, Deole (2019) and Steinhardt (2018) focus on the population of migrants who moved to Germany in the late 1960s to meet the shortage of labor that was prevalent in Germany at that time. These migrants were actively recruited, either by the German government or the sending countries' government. Furthermore, we find no effect on refugees' intention to stay (ITS) in Germany at the extensive margin. This is an important result. Existing research shows that the time horizon over which migrants can accrue returns to investments in country-specific human capital is positively associated with the gradient in their age-earnings profile (e.g., Dustmann, 1993, 1997, 2000). With hate crimes having little effect on the refugees' ITS, we conclude that a change in the ITS can be ruled out as a mediator between hate crime and the accumulation of country-specific human capital.

We also find strong suggestive evidence that our effects are mostly driven by refugees living in close proximity to the focal hate crime. Our data allows us to calculate geographical distances between the location of the focal hate crime, e.g., city, town or municipality, and

[6]Typically, the PHQ-4 score indicates the intensity of symptoms of depression and anxiety. To allow for a consistent comparison with the MCS score, we inverted the scale. Thus, a higher score indicates better mental health.

[7]Economic migrants normally leave their country of origin because of pull rather than push factors.

the refugees' place of residence. We then perform a median split distinguishing between refugees who live close to the hate crime and refugees who live further away. We find that refugees living closer to the respective hate crime have also stronger adverse mental health effects, while the effects are considerably smaller and insignificant for refugees living further away. Thus, this finding shows that the mental health effects reflect a response to a more direct exposure to hate crime.

In a second part, we test Becker and Rubinstein's (2011) conjecture that individuals with higher cognitive abilities are more likely to overcome the shock caused by hate crimes. To be more precise, Becker and Rubinstein's (2011) mechanism suggests that individuals with higher ability are more likely to align the objective and subjective likelihood of becoming a victim of a hate crime. For migrants, such as refugees, we contend that it is the country-specific human capital, i.e. language proficiency, that helps refugees to assess the true risk of being harmed by hate crimes. Therefore, we show that refugees, who are better integrated within their host country's society–e.g., those who have frequent contact with German natives or possess higher language proficiency levels–are less severely affected if they experienced a hate crime. This effect is most prevalent for the PHQ-4 score: While the estimated effect amounts to roughly 45% of a standard deviation for respondents who report low levels of German language proficiency, the effect size is halved for refugees with high country-specific human capital and statistically insignificant. Hence, in line with Becker and Rubinstein (2011), we find that individuals, who are more likely to align the subjective with the objective probability of being harmed by a hate crime, are less severely affected by hate crimes. In addition, our analysis suggests an important role for the refugees' ability to acquire information about these hate crimes. Lastly, our test, in which we interact country-specific human capital with the opportunity costs of acquiring this human capital, shows that "general ability" and the stock of country-specific human capital are complementary.

Moreover, while our empirical results indicate that the effect dissipates after approximately three months, we argue, similar to Clark et al. (2020), that those shocks have considerable long-term consequences via the reduced mental bandwidth, which can lead to worse economic decisions and thus, detrimental long-term consequences (Becker and Rubinstein, 2011; Schilbach et al., 2016), potentially also affecting the next generation (Almond and Currie, 2011; Almond et al., 2018).

Our paper relates to five branches of the literature. Abstaining from immigration, previous papers unanimously conclude that terrorist attacks have substantial negative effects on individuals' life satisfaction that persist, albeit, only temporarily (Akay et al., 2020; Clark et al., 2020). Using the 9/11 terrorist attacks as a quasi-experiment, Metcalfe et al. (2011) further shows that there are spillover effects to other countries such as the U.K..

Second, we also contribute to the literature on the effect of hate crimes on immigrants' health and integration within the host society. For the U.S., Gould and Klor (2014) show that the 9/11 attacks induced a backlash against Muslim immigrants, which in turn increased the opportunity costs of assimilation. For instance, in response to the 9/11 attack, Muslim immigrants in the U.S. were more likely to marry someone with the same ethnic background than before. Further, they also experienced lower rates of labor force participation (Gould and Klor, 2014). For Germany, there is evidence that hate crimes reduce integration outcomes as well as life and health satisfaction for immigrants with a Turkish background. Steinhardt (2018) shows that macro exposure to anti-immigrant attacks in the early 1990s in West Germany reduces the Turkish migrants' life satisfaction, increases their return intentions, and slows language acquisition. Further, Deole (2019) studies the revelation crimes directed toward Turkish residents in Germany in 2011. Deole (2019) finds that these revelations reduced the Turkish immigrants' life satisfaction.

We also relate to the literature focusing on the causes of hate crime. For Germany, Krueger and Pischke (1997), Falk et al. (2011) and Entorf and Lange (2019) analyze which socio-demographic characteristics predict hate crimes on the county level. Moreover, the literature is investigating how social media can predict hate crime (Bursztyn et al., 2019; Müller and Schwarz, 2020; Müller and Schwarz, 2020). We add to this literature by turning to the effect of hate crime on the most vulnerable group among those targeted: refugees. Lastly, we also contribute to a larger more general literature about the socioeconomic determinants of mental health (e.g. Adhvaryu et al., 2019; Fruewirth et al., 2019).

Our contribution to the literature is twofold: First, to the best of our knowledge, we are the first to analyze the effect of hate crime on refugees' mental health. This is surprising, given the stark increase in forced migration, which is expected to increase further given the economic and environmental changes worldwide (UNHCR, 2019), and the fact that mental illnesses has the highest prevalence of all non-communicable diseases (Bloom et al., 2011). Our results further suggest the importance of mental health for (labor market) integration and the subsequent long-term consequences for refugees in Germany.

Second, we test the importance of the refugees' opportunity to acquire information as a mediator between hate crime and the refugees' mental health response. This allows to further characterize Becker and Rubinstein's (2011) prediction that individuals with higher ability are more likely to overcome the shock due to hate crimes.

2 Forced migration and hate crime

In the 2010s, environmental deterioration and political upheavals in many African and Asian countries caused a stark increase in the number of refugees worldwide. Figure 4 in the appendix shows that the trend accelerated starting in 2013, following the outbreak of the Arab spring. Among refugees, the vast majority typically migrates either within their country of origin or settles down in a neighboring country (UNHCR, 2019). However, as the supply conditions deteriorated rapidly in the neighboring countries' refugee camps and intermediary states like Libya collapsed, large numbers of refugees began to migrate to Central Europe in 2014 and 2015 (Luft, 2016).

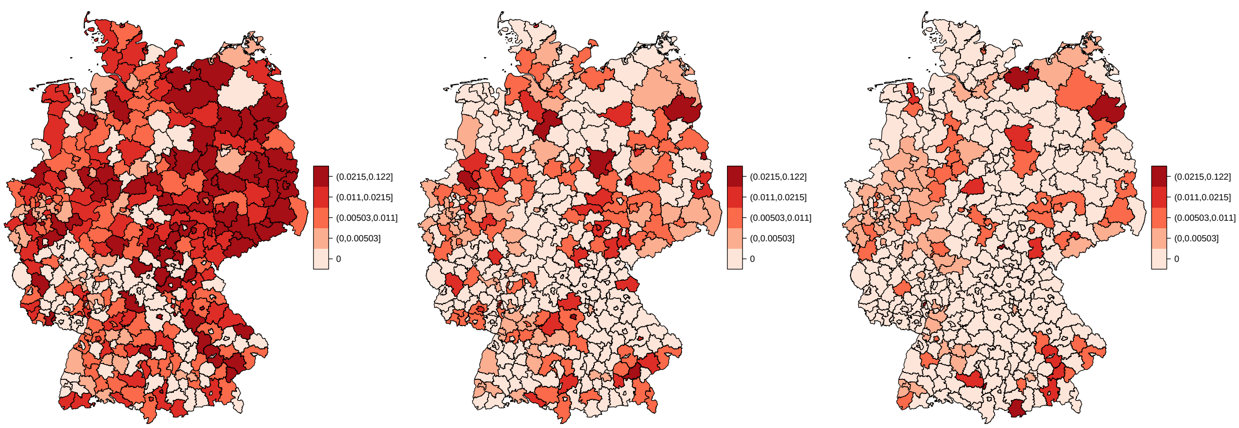
In Europe, the Dublin regulation stipulates that an application for asylum must be processed by the first Dublin country the asylum seeker enters. Therefore, European Union (EU) members closest to the refugees' countries of origin–normally at the edge of the EU–were disproportionately affected by the number of refugees migrating to Europe. As the number of refugees increased in these countries, the local conditions deteriorated quickly. Initially, the European countries tried to negotiate a new scheme to distribute refugees across the European Union's member countries. However, these negotiations were unsuccessful and, finally, in light of the inhumane situation of the refugees in some of the EU's host countries, the German government suspended the Dublin regulation in fall 2015 (BAMF, 2015). This triggered a large influx of refugees to Germany. Consequently, in 2015 Germany received the largest number of refugees in absolute terms, ranking third after Austria and Sweden in relative terms (Organization for Economic Co-operation and Development, 2017). Subsequently, however, the number of refugees in Germany decreased to pre-2015 levels (Figure 5 in the appendix).

Turning to the refugees' demographics, the majority of refugees in Germany originate from Syria, Afghanistan, and Iraq. In 2016, the Federal Office for Migration and Refugees (BAMF) reported the share of first-time asylum applications was 36.9% Syrian, 17.6% Afghan, and 13.3% Iraqi (BAMF, 2016). In addition, these refugees tend to be very young with 73.8% of these refugees younger than 30 years of age (BAMF, 2016).

Associated with the stark increase in the number of refugees, Germany experienced a strong increase in xenophobic sentiments directed against immigrants and refugees. For instance, using data from the Federal Criminal Office, we observe a strong increase in hate crimes against refugee shelters around the time when large number of refugees entered Germany (Figure 6 in the appendix). The number of these hate crimes increased strongly from 2014 to 2015, remained on an elevated level in 2016, and then returned to initial levels as the number of foreigners arriving in Germany fell. For instance, while our data shows 971 hate crimes in 2016, it declines to 303 hate crimes in 2017 and 170 in 2018.

Figure 1 provides a more detailed picture, displaying the number of attacks on refugee shelters per 100,000 residents at county-level per year.[8] We make two observations from this figure. First, as described before, the intensity of hate crimes declines over time. Second, although hate crime is always more prevalent in Eastern German states, it is also widely dispersed across Germany.

Figure 1: Number of attacks on refugee shelters per 100,000 inhabitants and counties



(a) 2016 (b) 2017 (c) 2018

Note: Figures 1a to 1c display the number of attacks on refugee shelters per 100,000 inhabitants and county from 2016 until 2018, respectively. Source: BKA data.

3 Data

We use two innovative datasets to estimate the effect of hate crime on refugees' mental health: The first dataset incorporates administrative information on hate crime against refugee shelters. The second dataset is the IAB-BAMF-SOEP Survey of Refugees in Germany, which provides us with detailed information on refugees' mental health as well as a wide range of socio-economic characteristics. In what follows, we describe these two datasets in detail.

[8]We resort to hate crimes per capita in these figures since the initial distribution of refugees within states relies on the counties' share of the population within each state. Consequently, a cross-sectional regression of the counties' share of the states' intake of refugees on the counties' population share within the respective state and state fixed effects results in an estimated OLS coefficient of one. Results are available on request.

3.1 Administrative data on hate crime against refugee shelters

Our comprehensive data on hate crime against refugee shelters stems from the German Federal Criminal Police Office ("*Bundeskriminalamt*"). The information was compiled by the German Federal Government in response to small inquiries ("*kleine Anfrage*") of the parliamentary group "DIE LINKE" and is published on a quarterly basis (e.g. Bundestag, 2016). Each entry in these files comprises information on the date of the attack, the state, the locality, the type of crime, and the crime's political motivation. For illustrative purposes, Table 7 in the appendix illustrates an excerpt of the data for January 1, 2016. The major advantage of this dataset is that it reports hate crimes that target refugees specifically rather than an aggregate measure on hate crimes that would have precluded the ability to distinguish between economic migrants and refugees. Second, hate crime against refugee shelters are much more salient than individual incidents such as refugees being attacked on the street. Finally, the BKA data is less likely to suffer from endogenous coverage, which could, for instance, be the case for newspaper data (Entorf and Lange, 2019). As such, it is an ideal source of information on hate crime against refugees for this analysis.

In a first step, we collected all information on hate crime against refugee shelters from the small inquiries and digitized the BKA information accordingly. In a second step, we geo-referenced the data based on information on the state and the exact location, e.g., the name of the city or municipality.[9] Overall, our data records 1,444 events between 2016 and 2018. As displayed in Section 2, the incidence of hate crimes against refugee shelters has substantially decreased over time.

3.2 IAB-BAMF-SOEP Survey of Refugees

The IAB-BAMF-SOEP Survey of Refugees comprises information on refugees' mental health and their socio-economic characteristics. The IAB-BAMF-SOEP Survey of Refugees has

[9] In less than five cases, we were not able to determine the exact GPS location since the respective location existed several times in the respective state.

been introduced in 2016, in response to the major influx of migrants to Germany in 2015 (Brücker et al., 2016; Deutsches Institut für Wirtschaftsforschung, 2017). This novel survey is part of the Socio-Economic Panel (SOEP) (Göbel et al., 2018) and, hitherto, is the only data base that allows for quantitative and empirical social research on this timely manner. Besides information on refugees' migration histories, background characteristics, overall living conditions, and integration outcomes in Germany, the IAB-BAMF-SOEP Survey of Refugees provides detailed information on refugees' mental health and their exact place of residence.

In our analysis, we make use of a single cross-section from the year 2016 due to two reasons: First, 2016 is the year of the last decade in which hate crimes were most prevalent in Germany. Second, information on the PHQ-4 score is only available in 2016. The interviews in the IAB-BAMF-SOEP Survey of Refugees typically take place from June to December in each year. Consequently, our period of observation is the second half of 2016. We merge each observation in the IAB-BAMF-SOEP Survey of Refugees to the respective hate crimes, based on the information on the exact interview date and the location. For each survey respondent, we then calculate the number of elapsed days since the most recent hate crime–the focal hate crime–in the county of residence. This running variable then governs the treatment status. The running variable is negative for refugees who have been interviewed before the focal hate crime occurred. If, on the other hand, the focal hate crime took place before the refugee was interviewed, the running variable is positive thereby marking the respondent as a treated individual.

3.3 Measuring refugees' mental health

We measure the refugees' mental health by the two mental health measures available in the IAB-BAMF-SOEP Survey of Refugees: the MCS and the PHQ-4 score. The MCS score is based on the Short Form-12 (SF-12) questionnaire, which contains twelve health-related items inferring the respondent's physical and mental health within 30 days preceding the interview (Andersen et al., 2007). The MCS score has been shown to be highly predictive for mental illnesses in the European population (Vilagut et al., 2013) and is an established measure of mental health in the economic literature (e.g. Eibich, 2015; Hofmann and Mühlenweg, 2018; Marcus, 2013).[10]

For the principal component analysis, we combine the twelve health items in eight subscales and normalize these subscales to have mean zero and standard deviation one. Subsequently, we perform a principal component analysis of these eight subscales for all first-time respondents in 2016 and 2017. The eight subscales of the SF-12 questionnaire load exactly on two factors. Figure 7 in the appendix, which plots the factors against the respective Eigenvalues, shows that the first two factors have Eigenvalues greater or equal to one. We conclude that the first two factors are the only significant factors. In a last step, we perform a varimax rotation. The resulting factor loadings are displayed in Table 8 in the appendix.

Clearly, the factor loadings of the second factor in column (2) of Table 8 in the appendix load very high on the subscales that are associated with mental health. The respective factor loadings for the mental health subscales range from 0.577 to 0.823, whereas the remainder factor loadings range from 0.084 to 0.313. In what follows, we refer to this factor as the MCS score.

Along with the MCS score, we also employ a mental health measure based on the PHQ-4 inventory (Kroenke et al., 2009). The scores based on the PHQ-4 inventory have been shown to have high reliability and validity (e.g. Kroenke et al., 2009; Loewe et al., 2010) and, importantly, to have good psychometric properties in a representative survey of Arab refugees (Kliem et al., 2016). The PHQ-4 inventory consists of four items, including the frequency of feeling little interest or pleasure in one's activities, melancholy, anxiety, and the inability to stop worrying. Responses to the four items are given on a four-point Likert-scale, ranging from one "Not at all" to four "(Almost) every day". In what follows, we proceed similar to the construction of the MCS score and perform a principal component analysis

[10] We apply the algorithm of Andersen et al. (2007) to the IAB-BAMF-SOEP Survey of Refugees. The number of factors as well as the factor loadings are very similar to those of the SOEP norm population in Andersen et al. (2007).

of the PHQ-4 inventory.[11] Figure 8 in the appendix shows that the Eigenvalue of the first factor is 2.40. In contrast, the second eigenvalue is 0.76. Consequently, we use the first factor as the only significant factor. Additionally, the factor loadings of the first and only factor, depicted in Table 9 in the appendix, range from 0.598 to 0.845. We label this factor PHQ-4 score. Initially, higher scores indicate worse mental health. However, to ease interpretation, we invert the scale. In this study, higher values are indicative of better mental health.

3.4 Additional outcomes and covariates

Additional outcome variables are life satisfaction and the respondents' intention to stay in Germany. Life satisfaction is inferred by the answer to the question "How satisfied are you with your life, all things considered?". The answers to this question are given on an eleven-point Likert-scale, ranging from zero, "Completely dissatisfied", to ten, "Completely satisfied". The respondents' intention to stay is inferred from the answer to the question "Do you want to stay in Germany forever?" Based on responses to this item, we construct an indicator which is equal to one if a respondent wants to stay in Germany forever and zero otherwise.

Additionally, we use the command over the German language as well as the number of contacts with Germans as proxies for country-specific human capital. The respondents are asked how well they can speak, read, or write in German. Answers are given on a five-point Likert-scale ranging from one "Very well" to five "Not at all". We construct an indicator which is equal to one if respondents state that they can speak, read, or write German at least averagely. The time spent with Germans is inferred by a six-point Likert-scale that ranges from one "Daily" to six "Never", with three "Weekly" being the median category. We construct an indicator which is equal to one if a refugee states that he or she has at least weekly contact with Germans. The final summary characteristics, together with further

[11] Often, researchers just use the sum of the four items, implying equal weighting of each factor. However, we decided to use an equal procedure as with the SF-12 questionnaire to remain consistent across mental health measurements.

predetermined characteristics, of our working sample are displayed in Table 1.[12]

Table 1: Summary statistics

	Mean	S.D.	Min.	Max.	N
	(1)	(2)	(3)	(4)	(5)
Outcomes:					
MCS score	49.357	10.471	15.075	74.147	1215
PHQ-4 score	0.010	1.031	-1.098	3.150	1215
Life satisfaction	7.280	2.326	0.000	10.000	1215
Intention to stay	0.947	0.225	0.000	1.000	1215
Refugee's characteristics:					
Female	0.388	0.488	0.000	1.000	1215
Year of birth	1981.821	10.376	1940.000	1998.000	1215
Country origin SIA	0.769	0.422	0.000	1.000	1215
Child present	0.674	0.469	0.000	1.000	1215
Married	0.686	0.464	0.000	1.000	1215
Characteristics of counties (2014):					
GDP per capita (in 1000 Euro)	35.939	12.876	20.373	93.773	1215
Average age	44.644	1.801	41.100	49.700	1215
Share of foreigners	0.085	0.050	0.013	0.240	1215

Note: Table 1 displays summary statistics for our outcomes, refugees characteristics and the characteristics of the counties in 2014. Column (1) displays means. Column (2) displays the corresponding standard deviations. Column (3) and (4) display the minimum and the maximum. The sample is restricted to a bandwidth of 90 days around the cutoff. Source: SOEP, v34.

4 Empirical method

We estimate the effect of hate crime on refugees' mental health using a RD design that compares refugees who have been interviewed shortly before and after a hate crime in the respective county of residence occurred. Thus, we estimate the following weighted local linear

[12]In Table 1, SIA is the acronym for "Syria, Iraq of Afghanistan". These are the countries from which most of the refugees in the data come from.

regression:

$$Y_{icmd} = \alpha + \beta D_{icmd} + \gamma Dist_{icmd} + \delta D_{icmd} \times Days_{icmd} + \zeta month_i + \theta dow_i + \epsilon_{icmd}. \quad (1)$$

In Equation 1, Y_{icmd} is the mental health outcome of interest, i.e., the MCS or the PHQ-4 score for respondent i in county c, in month m and day of week d. The indicator D_{icmd} is equal to one if the refugee was interviewed after a hate crime happened in the county of residence and zero otherwise. The running variable $Days_{icmd}$ captures the number of days elapsed since the focal hate crime occurred. We allow for differential linear trends before and after the focal hate crime. Consequently, we include the interaction term $D_{icmd} \times Days_{icmd}$ in Equation 1.[13] In addition, we account for potential seasonality in the mental health outcomes by including indicators for the month when respondents were interviewed, $month_i$. Further, we account for potential discontinuities in mental health and the likelihood that a hate crime takes place, which are associated with the day of week, dow_i. For instance, perpetrators could be more active on weekends than on weekdays. At the same time, refugees' mental health could be better on weekends compared to weekdays. In this case, we potentially underestimate the true effect of hate crimes. The inclusion of day of week indicators helps to account for this. We use a triangular kernel and cluster the standard errors on the running variable level, because our running variable is discrete (Lee and Card, 2010).[14]

It is notable that in some counties, hate crimes are clustered in time. Thus, it could be the case that refugees in the control group are treated if they were subject to a hate crime which took place before the focal hate crime, e.g., if a hate crime happened before an individual was observed and the number of days between this other hate crime and the day of observation is at least the number of days until the focal hate crime plus one. Similarly, treated refugees could have been subject to an additional hate crime before the focal hate

[13] In the robustness section, we also allow for quadratic trends in the running variable. Our conclusions remain unaltered.

[14] We also base our inference on standard errors clustered on the county level in the robustness section. Our conclusions remain unchanged.

crime. If this happens randomly, e.g., if these confounding attacks are independent and identically distributed, this would result in an attenuation bias. This attenuation bias potentially causes our estimates to be attenuated towards zero. Therefore, in the robustness section, we carry out a careful test gauging the relevance of this bias. We carefully drop observations that are multiply treated within various bandwidths and observe that the estimates tend to increase as we drop observations that are treated multiple times. Indeed, we find evidence for our conjecture. Thus, as precautionary measure and to optimally utilize the number of observations, we drop refugees who experienced a hate crime within 30 days before the focal hate crime. Based on this empirical specification, we choose the bandwidth to be ± 90 days.[15][16]

Continuity assumption Our identification assumption is based on the premise that, in absence of the treatment, the population mean in mental health is a continuous function of the running variable (Hahn et al., 2001). Another way to think about this is by means of selection on observables (Lee and Lemieux, 2010). In our case, the number of days elapsed since the focal hate crime governs the treatment assignment. Refugees who were interviewed before the focal hate crime are part of the control and refugees interviewed after the focal hate crime are considered part of the treatment group. Strictly speaking, the common support in the running variable is not guaranteed in this setting. Therefore, we require the continuity assumption to finally ensure the overlap condition.

Under this assumption, the estimate of γ can be interpreted as the causal effect of hate crime on refugees' mental health. However, we can not directly test the continuity assumption because it involves a counterfactual situation, i.e., we need to observe the population mean through the cutoff in absence of the treatment. Yet, we provide evidence that the continuity assumption holds. If predetermined individual and county level characteristics

[15]Following Calonico et al. (2014), we find that the asymptotically MSE-optimal bandwidths for the PHQ-4 and MCS score, life satisfaction are 88.3, 78.1, 71.8 and 112.7, respectively. For expositional clarity, we choose a bandwidth of 90 days or 3 months, which is close to the average of the three respective bandwidths. However, we show that our results are robust to a wide range of bandwidth choices in Section 5.3.

[16]We use the Stata package **rdrobust**. For the documentation, please refer to Calonico et al. (2017).

evolve continuously around the focal hate crime, we may interpret this as empirical evidence that the continuity assumption is valid. Any significant discontinuity in the mental health outcomes around the focal hate crime can be fully attributed to the focal hate crime. To test this, we apply our empirical specification to various predetermined characteristics on the individual and county level.

Our estimates reveal no discontinuity in the predetermined individual and county level characteristics around the focal hate crime. Figure 9 in the appendix displays RD plots for various predetermined individual and county characteristics. Overall, we find little visual evidence for discontinuities around the focal hate crime. Table 2 summarizes the results formally. Column (1) displays the point estimates, column (2) displays the corresponding standard errors, and column (3) displays the p-values associated with the coefficient estimates. Throughout, most of the estimates are small in relative terms. Further, all estimates are statistically insignificant. Thus, we are confident that the continuity assumption is warranted.

Precise manipulation around the cutoff A potential threat to our RD design could be the precise manipulation around the cutoff (Lee and Card, 2010; McCrary, 2008). If selection into or out of the treatment would be possible based on expected gains, our estimate of γ would suffer from selection bias and may be inconsistent. In our context, individuals would desire to select out of treatment. The more vulnerable the refugees are, the more likely they desire to select out of the treatment group. This would bias our estimate of the effect downwards.

Since our data on hate crimes is based on official crime statistics, we assert that strategic manipulation around the cutoff is difficult, if not impossible. This conjecture assumes that these hate crimes are typically not known to the public beforehand. In addition, the SOEP interviews are usually scheduled well in advance. The reason is that the interviews usually take some time, especially if a household consists of multiple individuals. In consequence, it is very unlikely that selection based on expected gains is prevalent.

Table 2: Continuity of predetermined characteristics around the focal hate crime

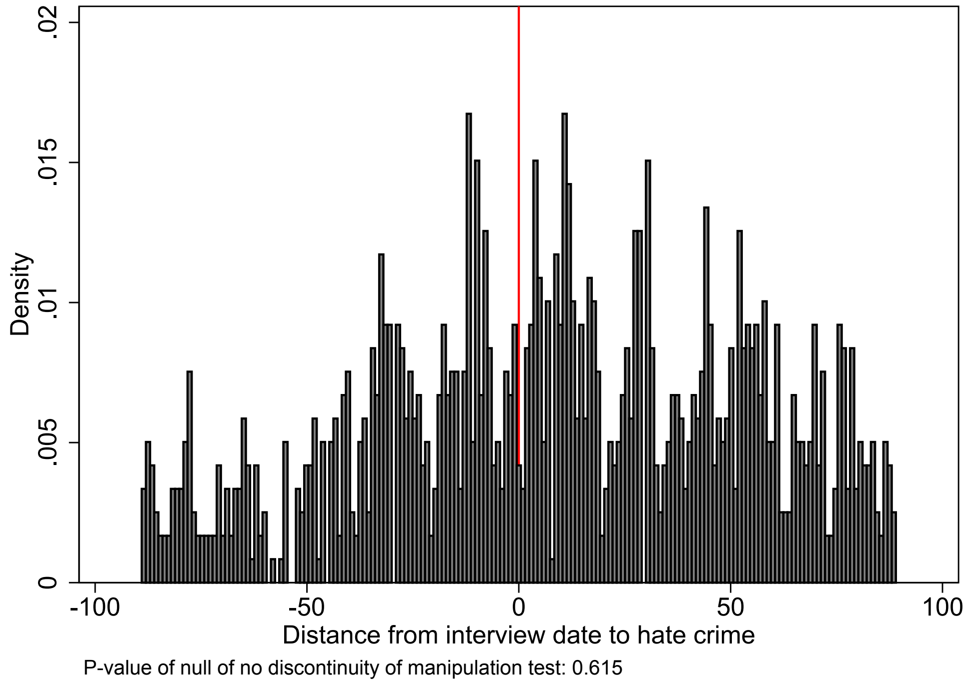
	Point estimate	Standard error	P-value
	(1)	(2)	(3)
Child present	-0.081	0.059	0.168
Country of origin SIA	0.050	0.052	0.328
Female	0.007	0.042	0.874
Married	0.033	0.065	0.606
Year of birth	-0.560	1.021	0.583
Average age in county	0.105	0.255	0.679
GDP per capita in county	0.243	1.886	0.897
Share of foreigner in county	-0.004	0.008	0.612

Note: Table 2 displays results for a test of the continuity assumption for predetermined individual and county characteristics. Column (1) displays point estimates. Column (2) displays standard errors associated with the point estimates. Column (3) displays p-values. The coefficient estimates are based on a local linear regression, in which we regress the respective outcome on an indicator for a hate crime, a linear trend in the running variable, which is allowed to differ before and after the focal hate crime, and day of week as well as month of year indicators. We use triangular weights and a bandwidth of 90 days. Non-binary outcomes are standardized to have mean zero and a standard deviation of one. Standard errors are clustered on the running variable level, distance in days to the focal hate crime, and are displayed in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Source: SOEP, v34.

However, if exposure to hate crimes decreases the likelihood that respondents thoroughly reply to all questions of the interview, our estimates would be biased downward since only the most robust respondents would be able to reply. But this results in a testable assumption. If exposure to a hate crime is associated with a lower likelihood that refugees provide information in the SOEP-interviews, we would observe a discontinuity in the empirical distribution of observations around the cutoff.

A density test around the cutoff, that was proposed by McCrary (2008), suggests that neither of the two phenomena are relevant in our case. If individuals were able to select into or out of the treatment or if fewer respondents provided information about their mental health in response to the hate crime, we would detect a discontinuity in the empirical probability density function of interviews around the focal hate crime. Figure 2 displays the empirical distribution of observations against the running variable. The vertical line indicates the day

Figure 2: Checking for precise manipulation around the cutoff



Note: Figure 2 displays the empirical pdf of observations around the cutoff. A bandwidth of 90 days is choosen. Each bin corresponds to one day. Each par corresponds to the density of observations at each day. The vertical bar indicates the day of the xenophobic attack. The p-value corresponds to a p-value of a manipulation test based on local polynomial regressions of order two. Source: SOEP, v34.

of the focal hate crime. Based on the inspection of the empirical probability density function, we find no evidence of a discontinuity around the focal hate crime. A p-value of 0.615 of a formal manipulation test, based on local polynomial regressions of order two (Cattaneo et al., 2020), indicates that there exists no discontinuity around the focal hate crime. Thus, we confidently rule out manipulation or differential response behavior around the cutoff.

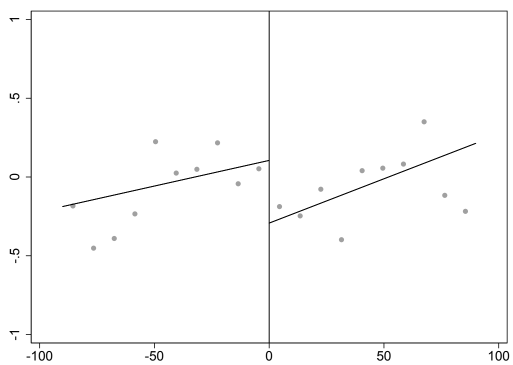
5 Results

In this section, we report our estimation results as well as additional robustness checks. Thereafter, we report heterogeneity analyses with respect to the refugees' country-specific human capital and the geographic proximity to the focal hate crime.

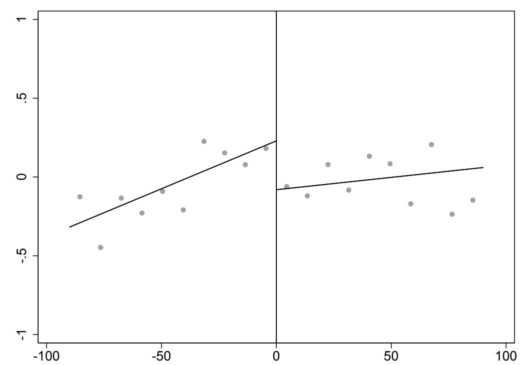
5.1 The effect of hate crime on mental health

Figures 3a and 3b illustrate the main results for the refugees' MCS and PHQ-4 score, whereby each figure displays a local linear fit on either side of the cutoff, with bandwidths of 90 and triangular kernels. We partial out day of week as well as month fixed effects. In both figures, the dots correspond to binned scatterplots, with the number of bins being equal to ten on each side of the cutoff (the focal hate crime). Both mental health outcomes are standardized to have mean zero and standard deviation one.

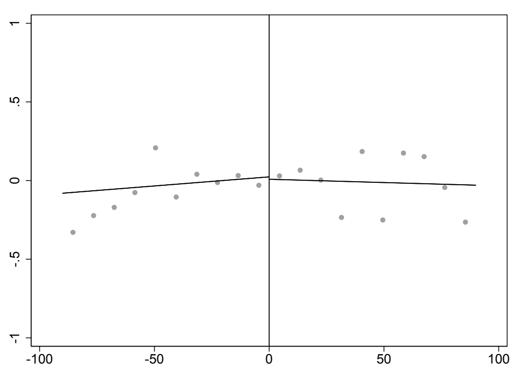
Figure 3: Visualization of results



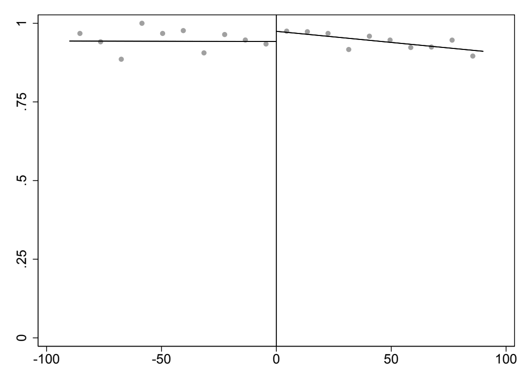
(a) MCS



(b) PHQ-4



(c) Life satisfaction



(d) ITS

Note: Figures 3a to 3d display the effect of xenophobic attacks on migrants' mental health, life satisfaction and intention to stay. Throughout, the bandwidth is chosen to be 90 days. The dots correspond to a binned scatterplots. The vertical bars are 95% confidence intervals for the means of the bins, based on standard errors that are clustered on the running variable. The linear fit corresponds to a local linear regression with a triangular kernel as in Equation 1. Source: SOEP, v34.

In these descriptive results, we find evidence for a strong discontinuity around the cutoff for the mental health outcomes, suggesting that being a victim to a hate crime worsens the refugees' mental health. Table 3 displays effect sizes corresponding to Equation 1. Columns (1) and (2) display the point estimates for the MCS and PHQ-4 score along with the standard errors, respectively. Based on these results, the effect sizes correspond to 37% of a standard deviation for the MCS score and 28% of a standard deviation for the PHQ-4 score. As a comparison, Clark et al. (2020) find that the Boston marathon bombing reduced the nearby resident's subjective well-being by a third of a standard deviation. In addition, Metcalfe et al. (2011) find that the 9/11 attack in the U.S. decreases mental distress in the U.K. population by about 7 to 14% of a standard deviation. Thus, our results are of comparable magnitude of studies such as Clark et al. (2020).

Table 3: The effect of hate crime on refugees' mental health, life satisfaction and intention to stay

	MCS	PHQ-4	LS	ITS
	(1)	(2)	(3)	(4)
Effect of hate crime	-0.368***	-0.284***	-0.040	0.023
	(0.118)	(0.104)	(0.103)	(0.022)
Number of observations	1215	1215	1215	1215

Note: Table 3 displays the effect of hate crimes on refugees' mental health, life satisfaction and intention to stay. Columns (1), (2), (3), and (4) display point estimates and corresponding standard errors for the MCS score, the PHQ-4 score as well as life satisfaction and refugees' intention to stay, respectively. The coefficient estimates are based on a local linear regression, in which we regress the respective outcome on an indicator for a hate crime, a linear trend in the running variable, which is allowed to differ before and after the focal hate crime, and day of week as well as month of year indicators. We use triangular weights and a bandwidth of 90 days. Non-binary outcomes are standardized to have mean zero and a standard deviation of one. Standard errors are clustered on the running variable level, distance in days to the focal hate crime, and are displayed in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Source: SOEP, v34.

While the estimated effects are sizable, the fact that the mental health outcomes are trending toward pre-treatment levels after the hate crime indicates that the effect is transitory. The mental health outcomes reach their pre-treatment level after approximately three months

(Figures 3a and 3b). Yet, the literature shows that such shocks can impair decision making and alter long-term outcomes, especially for the refugees' children. Thus, negative health shocks have the potential to negatively affect the trajectory of refugees, especially since they must navigate through many uncertainties shortly after arrival.

5.2 The effect of hate crime on life satisfaction and ITS

Figures 3c and 3d illustrate the main results for refugees' life satisfaction and ITS. The corresponding estimation results are displayed in columns (3) and (4) of Table 3. The outcome life satisfaction is standardized to have mean zero and standard deviation one. In contrast to mental health, hate crime has no effect on refugees' life satisfaction and their intention to stay in Germany. These are remarkable results, which stand in clear contrast to the findings of Deole (2019) and Steinhardt (2018).

It must be emphasized that life satisfaction is a multi-dimensional concept that measures the overall quality of life. As such, measures of life satisfaction are clearly distinct but associated with measures of symptoms of common mental disorders, e.g., depression (Keyes, 2006). To put it differently, having impaired mental health is not a sufficient condition for decreased life satisfaction. Furthermore, we argue that this emphasizes the difference in the perception of hate crime between refugees and economic migrants. Refugees are typically unable or unwilling to return to their home countries for fear of violent conflict or persecution. This is particularly true for the refugee population in our sample, who were mainly displaced because of civil wars. Thus, strong push factors caused these refugees to search refuge in Europe. In contrast to refugees, the composition of economic migrants is the result of an interaction of pull and push factors (Lazear, 2021). Consequently, a hypothesis consistent with our empirical observations is that the threshold that causes economic migrants to reconsider their time horizon in the host country is lower than for refugees. In addition, Steinhardt (2018) focuses on the intention to return within the next five years. Therefore, our study and that of Steinhardt (2018) compare the return intentions at different margins, e.g. extensive versus intensive margin.

The observation that hate crimes do not alter the refugees' intention to stay has an additional implication: With hate crimes not altering the time horizon over which refugees accrue returns to country-specific human capital, this cannot be considered an important mediator in the relationship between hate crime and long-term economic well-being.

5.3 Additional robustness checks

The previous section shows that hate crime has a strong negative effect on refugees' mental health, i.e., the MCS and the PHQ-4 score, and no effect on the refugees' life satisfaction and ITS. In this section, we provide additional robustness checks supporting the credibility of our estimates.

Choice of bandwidth Our results are robust to a wide range of bandwidth choices. Figure 10a to Figure 10d in the appendix display the coefficient estimates of Equation 1 and associated 95% confidence intervals as a function of the bandwidth for the MCS and PHQ-4 score as well as life satisfaction and ITS. Varying the bandwidth from 10 to 150 days in increments of 10 days, we see that the coefficient estimates for the MCS and the PHQ-4 score are similar to the main results and statistically significant for a wide range of bandwidths surrounding the respective MSE-optimal bandwidth. In contrast, for all bandwidths, point estimates for life satisfaction and the refugees' ITS are close to zero and statistically insignificant.

Inclusion of covariates The results are also robust to the inclusion of a wide set of predetermined covariates on the individual or county level. In Section 4, we argue that identification stems from the assumption that–in absence of the focal hate crime–the population mean of our outcome is a continuous function of the running variable. Alternatively, one can also think of the identification stemming from local randomization around the focal hate crime (Hausman and Rapson, 2018; Lee and Lemieux, 2010). We provide evidence for this by including predetermined individual and county level characteristics in Equation 1.

The results are displayed in Table 10 in the appendix, where each row displays coefficients and standard errors for another outcome. We subsequently add different covariates to the regression: Column (1) adds individuals characteristics, column (2) includes only county level characteristics, and column (3) includes both. Throughout, we observe that the coefficients remain remarkable stable and significant. We consider this as evidence that the local randomization was indeed successful.

Clustering of hate crimes in time In Section 4, we argue that the clustering of hate crimes within counties in time may bias our results downwards. If individuals are multiply treated, i.e., if they experience several hate crimes preceding the interview, refugees' mental health may decrease, and our estimates would be attenuated. Therefore, we exclude those individuals who experience a second hate crime within a thirty-day window before the interview. One may, however, argue that this is a selective choice. Hence, as a robustness check, we subsequently exclude different time frames and estimate our treatment effect. Table 11 in the appendix illustrates estimation results. Overall, we find hate crimes to substantially impair refugees' mental health. In line with our argument, the estimates increase in size the more strictly we ban multiple treated. For instance, while the MCS score decreases by 37% of a standard deviation in our baseline specification, this value increases to 71% of a standard deviation if we exclude observations who experience a second hate crime in a ninety day window before the interview.

More flexible specification Furthermore, our results are robust to alternative and more flexible specifications. The trend in the running variable in our main specification is linear. In general, there is no reason to believe that the trend in our running variable is indeed linear. If we misspecify our model, our estimate could be potentially biased. Therefore, we also allow for a quadratic trend in the running variable and follow the recommendation of Gelman and Imbens (2019) to avoid higher order polynomials than order two.[17] Table 12 in the appendix

[17]Gelman and Imbens (2019) argument rests on the observation that a RD estimate is the difference of weighted averages of the outcome on the left and the right of the respective cutoff. In case of higher order polynomials larger than two, the odds are high that very high weights are assigned to observations further away from the cutoff.

displays the results for specifications with a quadratic trend in the running variable. For each outcome, we separately calculated asymptotically MSE-optimal bandwidths (Calonico et al., 2014). Again, the effects point toward a sizable negative effect of hate crimes on refugees' mental health. While the effect size for the MCS score remains relatively stable, the effect size for the PHQ-4 score increases from about 28% of a standard deviation to 34% of a standard deviation.

Inference In our main specification, we followed the literature and clustered on the level of the running variable. However, we show that our results are robust to clustering on the county level instead. This implies serial correlation of the regressor or error term on the county level. The results are displayed in Table 13 in the appendix. Table 13 displays the respective coefficient estimates with the standard errors, clustered on the county level. Again, our conclusion remain unchanged.

Placebo estimates If we assume that the event happened either thirty days before or after the focal hate crime our estimation results become null. Figure 11 in the appendix shows coefficient estimates for each of these specifications along with the accompanying 95% confidence intervals. Throughout, the coefficients are small and close to zero. Further, the confidence intervals suggest that we cannot reject the absence of any effect.

5.4 Interaction with geographical distance

The previous results rely on the county of residence as the relevant geographical unit. We chose this in order to avoid *ad hoc* assumptions about the relevant distance in, for instance, radius matching. However, a natural question that arises is, whether it is actual hearsay or the fact that the refugees are directly affected by the respective hate crime. To further characterize our estimates, we calculate the actual geographic distance between the place where the focal hate crime in the county of residence took place and the refugees' place of residence. For the IAB-BAMF-SOEP Survey of Refugees, the exact geo-location is available within a specialized secure setting at the Research Data Center of the SOEP. Unfortunately, we do not have the exact GPS data of the refugee accommodations that were attacked. We only have the location, e.g., city or municipality. We assign each attack the centroid of the respective location. However, in some cases, it is the district of a city. Thus, some measurement error is associated with the distance calculation. To minimize measurement concerns, we distinguish between refugees living close by and further away by means of a median split.

The results clearly indicate that those refugees living closer to the focal hate crime show greater effects than those living further away. Table 4 shows the corresponding estimates. For those refugees closer to the hate crime, the effect sizes are more than twice as large as for those living further away. In addition, the estimates become insignificant for refugees living further away. However, our estimates are not precise enough to formally reject the null hypothesis of no difference between refugees living closer to the focal hate crime and those living further away. Overall, our conclusions are similar to the conclusions drawn by Clark et al. (2020), who also find that residents who live more closely to the Boston Marathon Bombing are more severely affected.

5.5 Country-specific human capital as a mediator

In the following, we investigate Becker and Rubinstein's (2011) hypothesis that individuals with higher ability are less likely to display large emotional responses to hate crime. Becker and Rubinstein (2011) argue that higher ability allows individuals to align the subjective and objective likelihood of being harmed by hate crime better, i.e., that individuals with higher ability are potentially more likely to obtain and process relevant information necessary to align the objective and subjective likelihood of being harmed by hate crimes. Moreover, for migrants, or more precisely, refugees, we argue that it is country-specific human capital that matters for this process. Thus, we test whether language proficiency and contact to native residents moderates the mental health response to hate crimes.

For this, we distinguish between refugees who speak, read, and write German at least

Table 4: The effect of xenophobic attacks on refugees' mental health, accounting for geographic distance to hate crime

	(1) Low distance	(2) High distance
MCS	-0.569***	-0.281
	(0.165)	(0.196)
PHQ-4	-0.435**	-0.206
	(0.178)	(0.143)
Life satisfaction	-0.109	0.021
	(0.140)	(0.148)
Intention to stay	0.035	0.015
	(0.038)	(0.028)
Number of observations	585	620

Note: Table 4 displays the effect of xenophobic attacks on refugees' mental health, disaggregated by geographic distance to the focal hate crime. Column (1) displays point estimates for refugees with low geographic distance to the focal hate crime, while column (2) displays results for refugees with high geographic distance. The coefficients correspond to coefficient estimates of a local linear regression of the mental health outcome on an indicator which is equal to one if a xenophobic attack occurred as well as the temporal relative distance to the attack, allowing for differential trend before and after the xenophobic attack. Throughout, we use triangular kernels and the bandwidth around the cutoff is ± 90 days. The outcomes have been standardized to have mean zero and standard deviation one. The standard errors are clustered on the day relative to the xenophobic attack and are displayed in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Source: SOEP, v34.

averagely from those who report lower levels of language proficiency. With respect to social capital, we distinguish between refugees who have at least weekly contact with German natives and those who have less contact.[18] Additionally, we distinguish between human capital that is difficult–more costly–to acquire and low-cost human capital. This also allows us to interact the country specific human capital with "general ability" and test for complementarities in the respective relationship. We argue that reading German and having frequent contact with Germans are easier to acquire (low-cost) dimensions of country-specific human capital than writing and speaking German (high-cost).

[18]Note that the frequency of having contact with German natives may change as a result of the hate crimes. However, we theorize that this may not be the case regarding the command over the German language.

Table 5 and Table 6 display the results for the stock of low- and high-cost country-specific human capital, respectively. In Table 5, columns (1) and (2) show the results for having at least weekly contact with Germans versus less frequent contact. Columns (3) and (4) display the results for being able to read German at least averagely versus worse than averagely. The results in Table 5 suggest a lower effect of hate crime on the mental health of refugees who have frequent contact with Germans and are better able to read German. In effect sizes, the difference between refugees with low versus high country-specific human capital is 5.6 percentage points of a standard deviation for the MCS score. This corresponds to a difference of 15.9% relative to the effect size for those who have less frequent contact with Germans. On the other hand, the difference is 15.1 percentage points for between those who read German at least averagely and those who read German below averagely for the PHQ-4 score. This is equivalent to 42.5% relative to the effect size for those who read less than averagely German. However, the difference between those who read German at least averagely and those who read German less than the average is smaller and points in the opposite direction.

In Table 6, columns (1) and (2) display results for individuals who write German at least averagely and less than averagely, while columns (3) and (4) display the results for individuals who speak German at least averagely and less than averagely. Here, the pattern is even more pronounced. The differences uniformly suggest that effect sizes are considerably smaller for refugees with high levels of country-specific human capital. For the PHQ-4 score, results suggest that we can not reject the absence of an effect of hate crime on the refugees' mental health for those who write and speak German at least averagely. Moreover, the difference in effect sizes amounts to 30.8 percentage points of a standard deviation for the PHQ-4 score between those who speak German at least averagely and those who speak German worse than averagely. This corresponds to 69% relative to the effect size for those who speak German worse than averagely. On the other hand, the difference in effect sizes is 14.7 percentage points for the MCS score between those who speak German at least averagely and those who

Table 5: The effect of hate crime on refugees' mental health, conditioning on low cost country-specific human capital

	Contact with Germans		Reading German	
	Yes	No	Yes	No
	(1)	(2)	(3)	(4)
MCS	-0.349**	-0.415***	-0.410***	-0.369**
	(0.169)	(0.152)	(0.135)	(0.164)
PHQ-4	-0.250*	-0.344**	-0.204*	-0.355*
	(0.149)	(0.168)	(0.116)	(0.186)
Number of observations	654	550	622	582

Note: Table 5 displays the effect of hate crime on refugees' mental for refugees commanding over low-cost country-specific human capital. We conjecture that "Contact with Germans" and "Reading German" are low-cost country-specific human capital. We distinguish between refugees that command over the country specific human capital ("Yes") or not ("No"). Refugees have contact with Germans if they have contact with Germans on a weekly basis. Refugees command about the skill "Reading German" if they read German at least averagely. The coefficient estimates are based on a local linear regression, in which we regress the respective outcome on an indicator for a hate crime, a linear trend in the running variable, which is allowed to differ before and after the focal hate crime, and day of week as well as month of year indicators. We use triangular weights and a bandwidth of 90 days. The outcomes are standardized to have mean zero and a standard deviation of one. Standard errors are clustered on the running variable level, distance in days to the focal hate crime, and are displayed in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Source: SOEP, v34.

speak German worse than averagely. This is equal to 34.2% relative to the baseline.

Table 6: The effect of hate crime on refugees' mental health, conditioning on high cost country-specific human capital

	Writing German		Speaking German	
	Yes	No	Yes	No
	(5)	(6)	(7)	(8)
MCS	-0.277**	-0.449***	-0.283**	-0.430**
	(0.123)	(0.169)	(0.121)	(0.175)
PHQ-4	-0.133	-0.420**	-0.139	-0.447**
	(0.121)	(0.187)	(0.113)	(0.188)
Number of observations	603	601	647	557

Note: Table 6 displays the effect of hate crime on refugees' mental health for refugees commanding over high-cost country-specific human capital. We conjecture that "Speaking German" and "Writing German" are high-cost country-specific human capital. We distinguish between refugees that command over the country specific human capital ("Yes") or not ("No"). Refugees command about the skill "Reading German" or "Writing German" if they reply that they speak or read German at least averagely. The coefficient estimates are based on a local linear regression, in which we regress the respective outcome on an indicator for a hate crime, a linear trend in the running variable, which is allowed to differ before and after the focal hate crime, and day of week as well as month of year indicators. We use triangular weights and a bandwidth of 90 days. The outcomes are standardized to have mean zero and a standard deviation of one. Standard errors are clustered on the running variable level, distance in days to the focal hate crime, and are displayed in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Source: SOEP, v34.

Our results indicate clearly that refugees, who have more country-specific human capital experience lower mental health responses to hate crime. Further, this is complementary to the costs of acquiring this country-specific human capital. This is consistent with Becker and Rubinstein (2011) and clearly suggests a high relevance for the ability to acquire information about the respective hate crimes.

6 Conclusion

Considering that both hate crimes and the number of refugees are strongly increasing, it is of utmost importance to estimate the costs associated with the hate crimes targeting refugees. Therefore, this paper shows that hate crimes have a strong and negative effects on refugees' mental health, as measured by the MCS and PHQ-4 score.

The effects are sizable in magnitude and stronger for refugees living in close geographic proximity to the focal hate crime. Our results suggest that mental health shocks exist only temporarily. However, since the feeling of increased uncertainty and insecurity leads to worse economic choices (Becker and Rubinstein, 2011; Schilbach et al., 2016) and because these shocks potentially extend to the next generation (Almond and Currie, 2011; Almond et al., 2018), we argue that these shocks may have important long-run consequences.

We also characterize Becker and Rubinstein's (2011) conjecture that it is high ability individuals who are less prone to hate crimes by showing how important it is to acquire information about these incidences. This "information channel" interacts with the opportunity costs of acquiring this ability adding to the evidence on the importance of country-specific human capital for migrants and refugees. This is particular important for refugees since these are permanent migrants, able to accrue the returns to country-specific human capital over longer time horizons (Dustmann and Glitz, 2011).

In contrast to mental health, we find no effect of hate crime on refugees' life satisfaction or intention to stay forever in Germany. This result stands in clear contrast to the previous literature, which considers the effect of hate crime on economic immigrants' integration (e.g., Deole, 2019; Steinhardt, 2018). The contrasting results may be explained by the inherent differences between refugees and economic migrants, thus reinforcing the importance of distinguishing between these groups.

Our results have very important policy implications. Mental health is a central determinant of individual's well-being, and physical integrity is a basic constitutional right. Further, impaired mental health as a result of perceived hate crimes may have substantial negative effects for refugees that may harm integration in the host country in the long-run. Besides this first-order effect, slow integration of refugees creates substantial negative externalities and fiscal costs for the host societies. As a consequence, our results ask for increased attention towards the mental health needs of refugees being victims of hate crime. In addition, refugees' integration success depends on the host societies' attitudes towards refugees (Ther, 2019) and hate crimes are the most severe form of refusal. If host countries wish to integrate refugees, they should make every effort to create equal opportunities and social cohesion.

7 Additional tables

Table 7: Attacks against refugee shelters

No.	Date	Place	State	Type of crime	Right-wing
1	01./01/2016	Nienburg/Saale	ST	Insult §185 StGB	x
2	01/01/2016	Merseburg/Saale	ST	Sedition §130 StGB	x
3	01/01/2016	Wernigerode	ST	Property damage §304 StGB	x
4	01/01/2016	Assamstadt	BW	Grievous bodily harm §224 StGB	x
5	01/01/2016	Werbach	BW	Use of symbols of unconstitutional organizations §86a StGB	x
6	01/01/2016	Ruppertshofen	BW	Use of symbols of unconstitutional organizations §86a StGB	
7	01/01/2016	Zeven	NI	Grievous bodily harm §224 StGB	
8	01/01/2016	Leverkusen	NW	Grievous bodily harm §224 StGB	x

Notes: This table is based on administrative data on hate crimes against refugee shelters (all entries for January 1, 2016), which is published by the German Federal government on a quarterly basis. Source: BKA data (2016-2018).

Table 8: Factors of a principal component analysis of the subscales of the SF-12 questionnaire

	PCS score (1)	MCS score (2)
Physical Fitness	0.791	0.084
General Health	0.740	0.281
Bodily Pain	0.831	0.194
Role Physical	0.823	0.313
Mental Health	0.155	0.823
Role Emotional	0.544	0.605
Social Functioning	0.494	0.577
Vitality	0.108	0.700

Note: Table 8 displays the factor loadings of a principal component analysis of the subscales of the SF-12 questionnaire. The factor analysis has been performed on all first time respondents of the IAB-BAMF-SOEP Refugee Survey in 2016 and 2017. Column (1) displays the corresponding factor loadings for the first factor, which corresponds to the PCS score. Column (2) displays the factor loadings of the second factor, which corresponds to the MCS score.

Table 9: Factor loadings on the first factor of a principal component analysis of the items of the PHQ-4 inventory

	PHQ-4 score
	(1)
Little Interest	0.598
Melancholy	0.845
Anxiety	0.844
Worrying	0.786

Note: Table 9 displays the factor loadings of a principal component analysis on the items of the PHQ-4 inventory. scale under consideration. Column (1) displays the corresponding factor loadings for the first factor. The factor analysis has been performed on the first time respondents of the IAB-SOEP-BAMF Refugee Survey in 2016.

Table 10: The effect of hate crime on refugees' mental health, life satisfaction and intention to stay, controlling for predetermined characteristics

	(1)	(2)	(3)
MCS	-0.337***	-0.361***	-0.329***
	(0.118)	(0.119)	(0.119)
PHQ-4	-0.291***	-0.284***	-0.290***
	(0.107)	(0.102)	(0.105)
Life satisfaction	-0.030	-0.031	-0.023
	(0.097)	(0.103)	(0.097)
Intention to stay	0.029	0.023	0.030
	(0.022)	(0.023)	(0.022)
Refugees' predetermined characteristics	✓		✓
Regional predetermined characteristics		✓	✓
Number of observations	1215	1215	1215

Note: Table 10 displays the effect of hate crime on refugees' mental health, life satisfaction and intention to stay, controlling for individual or county level characteristics. Columns (1), (2), (3), and (4) display point estimates and corresponding standard errors for estimations including predetermined individual, regional as well as individual and regional characteristics, respectively. The coefficient estimates are based on a local linear regression, in which we regress the respective outcome on an indicator for a hate crime, a linear trend in the running variable, which is allowed to differ before and after the focal hate crime, and day of week as well as month of year indicators in addition to the respective predetermined characteristics. We use triangular weights and a bandwidth of 90 days. Non-binary outcomes are standardized to have mean zero and a standard deviation of one. Standard errors are clustered on the running variable level, distance in days to the focal hate crime, and are displayed in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Source: SOEP, v34.

Table 11: The effect of xenophobic attacks on refugees' mental health, accounting for multiply treated

	(1) 14 days	(2) 30 days	(3) 45 days	(4) 60 days	(5) 90 days
MCS	-0.280***	-0.369***	-0.422***	-0.636***	-0.713***
	(0.099)	(0.118)	(0.143)	(0.131)	(0.145)
PHQ-4	-0.268***	-0.285***	-0.331***	-0.483***	-0.550***
	(0.100)	(0.104)	(0.125)	(0.110)	(0.121)
Life satisfaction	-0.153*	-0.040	-0.081	-0.063	-0.078
	(0.086)	(0.102)	(0.127)	(0.129)	(0.142)
Intention to stay	0.011	0.023	0.037	0.041	0.030
	(0.023)	(0.022)	(0.028)	(0.033)	(0.026)
Number of observations	1333	1215	1098	982	770

Note: Table 11 displays the effect of xenophobic attacks on refugees' mental health. We argue that estimates may be downward biased if gate crimes are clustered in time. Therefore, we drop observations who experienced a second hate crime shortly before the focal hate crime for different time periods. Column (1) drops observations who experience a second hate crime in a fourteen day period preceding the focal hate crime. Columns (2), (3), (4), and (5) display the results for a thirty (baseline estimation), forty-five, sixty, and ninety day period, respectively. The coefficients correspond to coefficient estimates of a local linear regression of the mental health outcome on an indicator which is equal to one if a xenophobic attack occurred as well as the temporal relative distance to the attack, allowing for differential trend before and after the xenophobic attack. Throughout, we use triangular kernels and the bandwidth around the cutoff is 90 days. The outcomes have been standardized to have mean zero and standard deviation one. The standard errors are clustered on the day relative to the xenophobic attack and are displayed in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Source: SOEP, v34.

Table 12: The effect of hate crime on refugees' mental health, allowing for a quadratic trend in the running variable

	MCS (1)	PHQ-4 (2)	LS (3)	ITS (4)
Effect of hate crime	-0.374**	-0.339***	0.080	0.028
	(0.155)	(0.129)	(0.151)	(0.031)
Number of observations	1393	1410	1244	1371
MSE-optimal bandwidth	114.445	117.850	92.039	109.783

Note: Table 12 displays the effect of hate crime on refugees' mental health, life satisfaction and intention to stay allowing for a quadratic trend. Columns (1), (2), (3), and (4) display point estimates and corresponding standard errors for the MCS score, the PHQ-4 score as well as life satisfaction and refugees' intention to stay, respectively. The coefficient estimates are based on a local polynomial regression, in which we regress the respective outcome on an indicator for a hate crime, a quadratic trend in the running variable, which is allowed to differ before and after the focal hate crime, and day of week as well as month of year indicators. We use triangular weights. Non-binary outcomes are standardized to have mean zero and a standard deviation of one. Standard errors are clustered on the running variable level, distance in days to the focal hate crime, and are displayed in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Source: SOEP, v34.

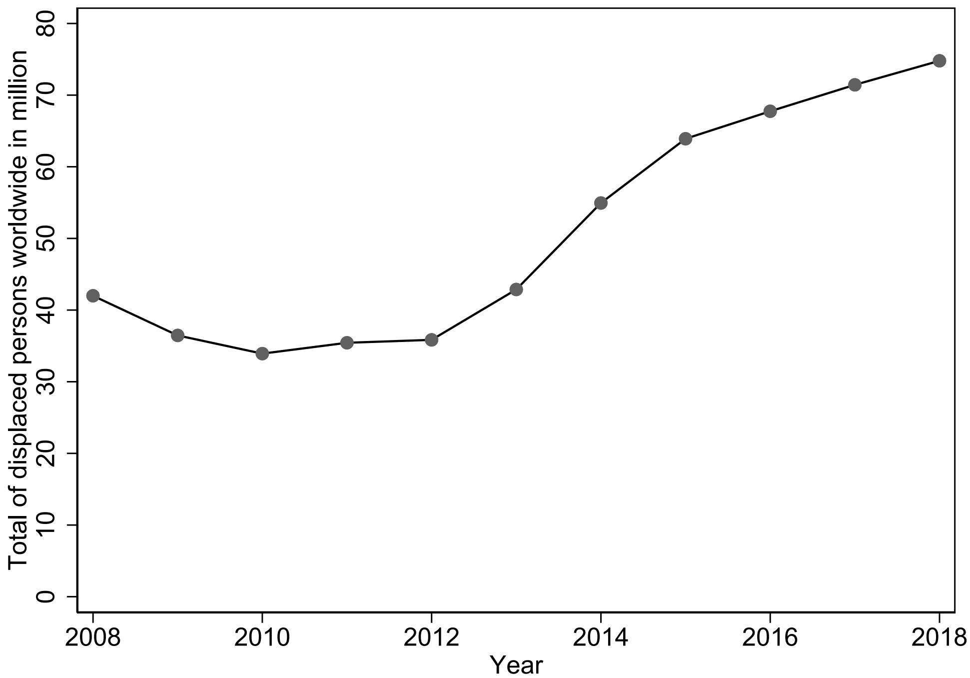
Table 13: Clustering on the county level

	MCS	PHQ-4	LS	ITS
	(1)	(2)	(3)	(4)
Effect of hate crime	-0.368*	-0.284**	-0.040	0.023
	(0.192)	(0.130)	(0.116)	(0.023)
Number of observations	1215	1215	1215	1215

Note: Table 13 displays the effect of hate crime on refugees' mental health, life satisfaction and intention to stay, clustering the standard errors on the level of the county. Columns (1), (2), (3), and (4) display point estimates and corresponding standard errors for the MCS score, the PHQ-4 score as well as life satisfaction and refugees' intention to stay, respectively. The coefficient estimates are based on a local linear regression, in which we regress the respective outcome on an indicator for a hate crime, a linear trend in the running variable, which is allowed to differ before and after the focal hate crime, and day of week as well as month of year indicators. We use triangular weights and a bandwidth of 90 days. Non-binary outcomes are standardized to have mean zero and a standard deviation of one. Standard errors are clustered on the county level and are displayed in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Source: SOEP, v34.

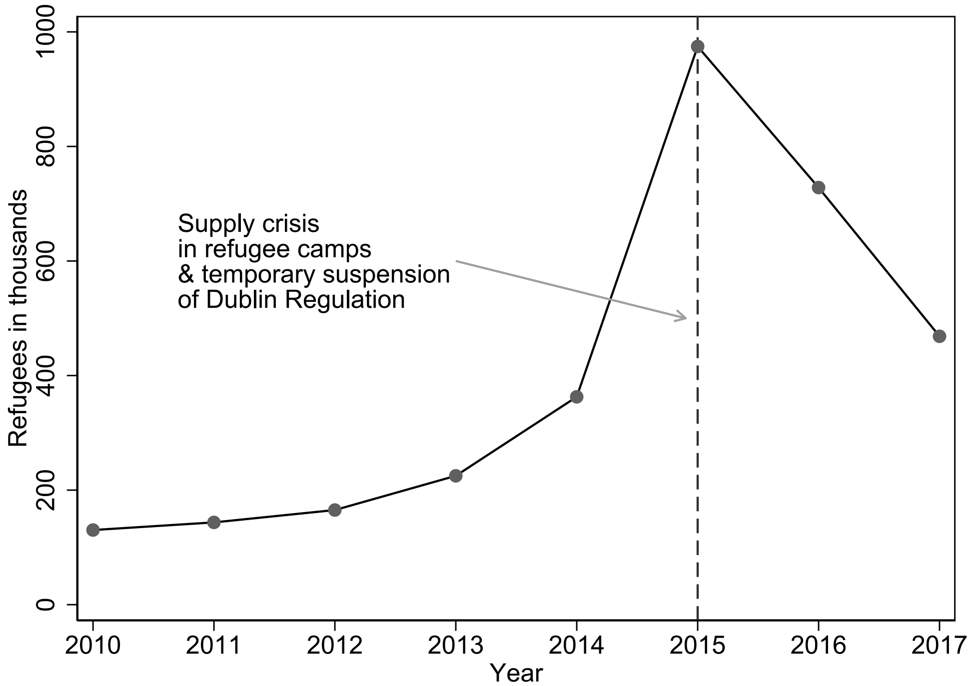
8 Additional figures

Figure 4: Time trend in the number of displaced persons



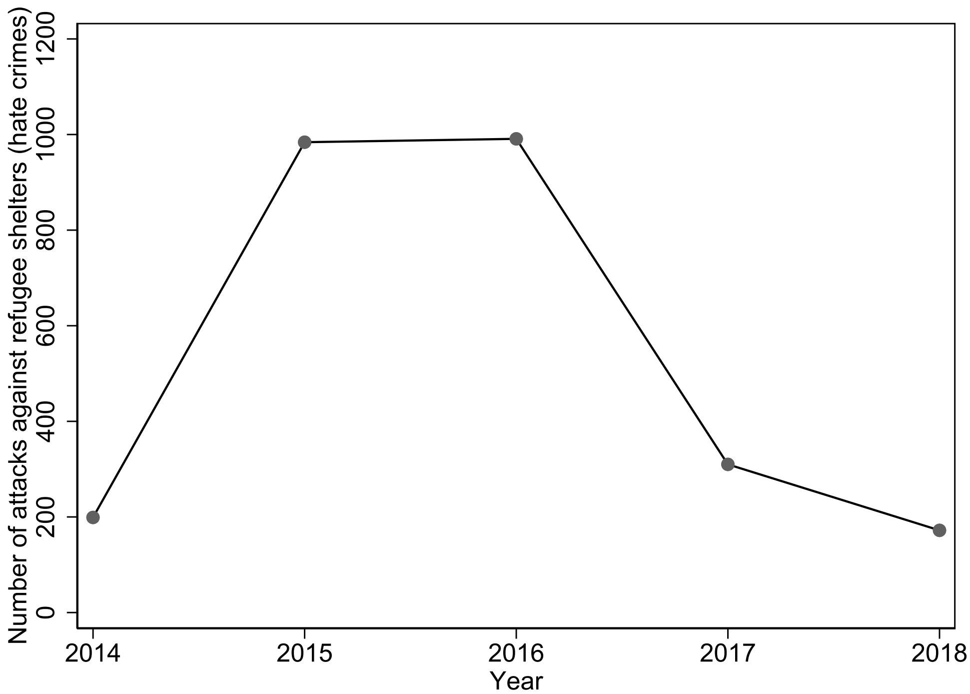
Note: Figure 4 plots the number of displaced persons worldwide from 2008 to 2018. Source: UNHCR (2009) to UNHCR (2019).

Figure 5: Number of asylum seekers in Germany



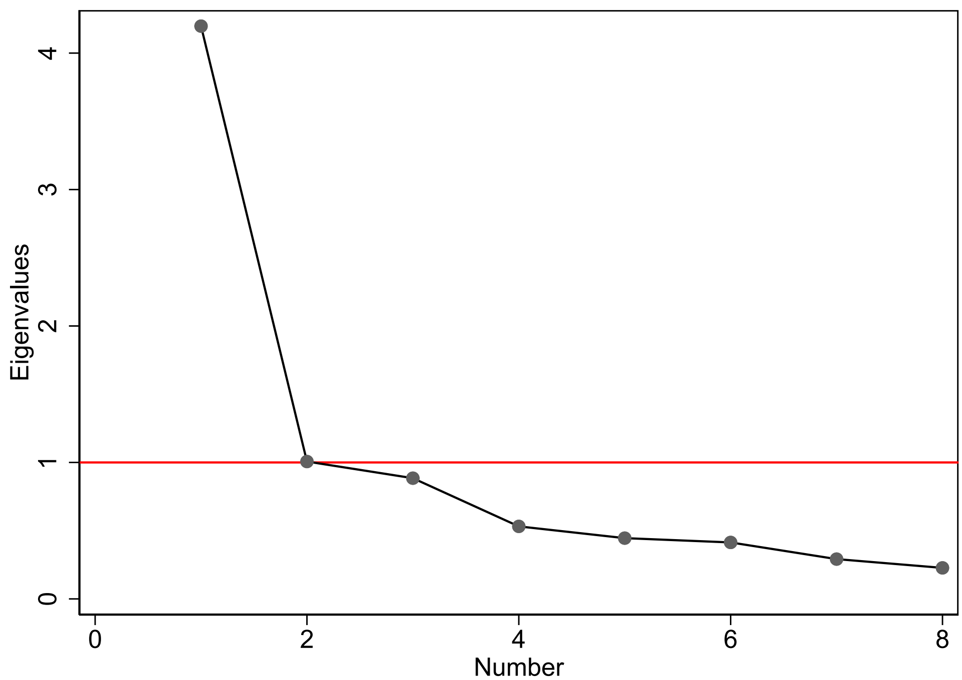
Note: Figure 5 plots the number asylum seekers from 2010 to 2017. Source: Federal Statistical Office of Germany (2019).

Figure 6: Number of attacks against refugee shelters over time



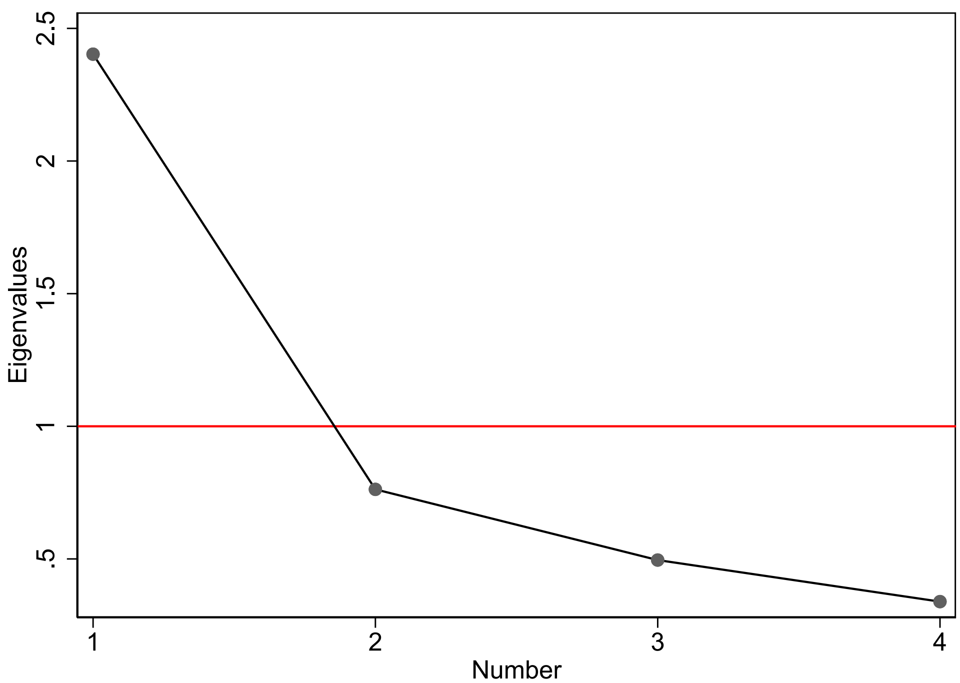
Note: Figure 6 plots the number of attacks against refugee shelters. Source: Bundestag (2016).

Figure 7: Scree plot for principal component analysis of the subscales of the SF-12 questionnaire



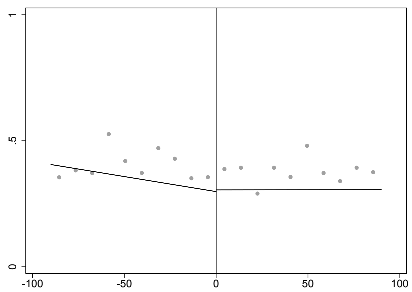
Note: Figure 7 plots the factors and the corresponding Eigenvalues after a principal component analysis of the subscales of the SF-12 questionnaire. The horizontal red line corresponds to Eigenvalues of one. Source: SOEP, v34.

Figure 8: Scree plot for principal component analysis of the items of the PHQ-4 inventory

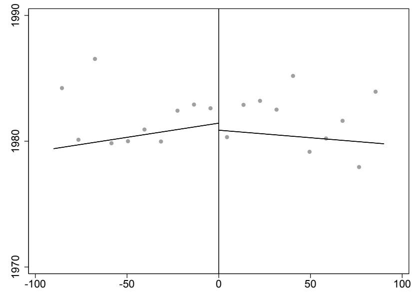


Note: Figure 8 plots the factors and the corresponding Eigenvalues after a principal component analysis of items of the PHQ-4 inventory. The horizontal red line corresponds to Eigenvalues of one. Source: SOEP, v34.

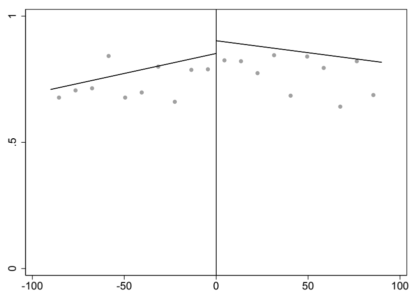
Figure 9: Test of continuity assumption



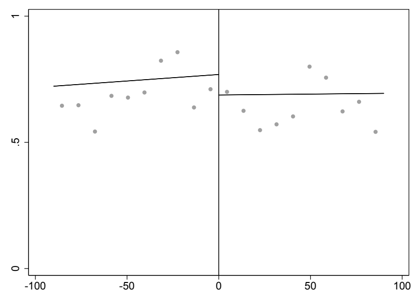
(a) Female



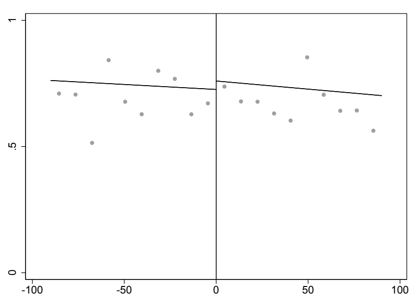
(b) Year of birth



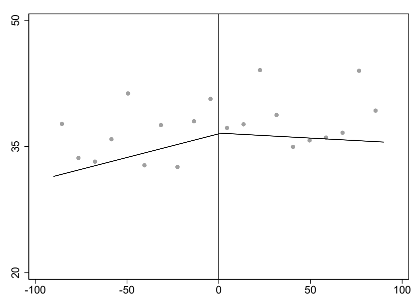
(c) Country of origin SIA



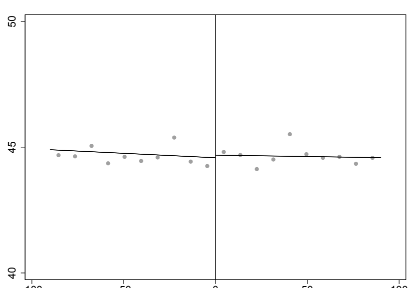
(d) Child



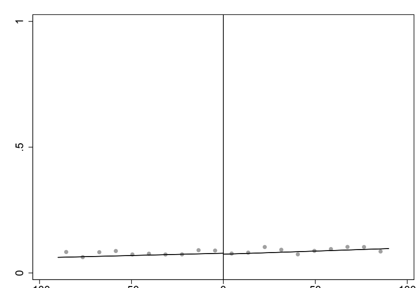
(e) Married



(f) BIP per capita in county



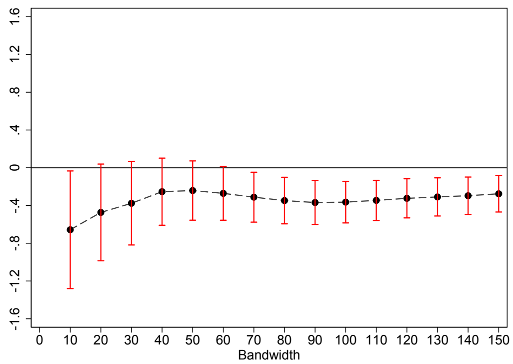
(g) Average age in county



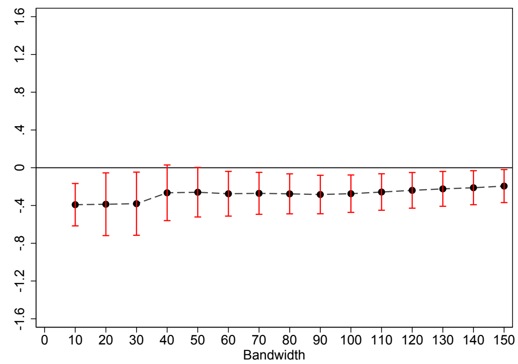
(h) Share foreign in county

Note: Figures 9a to 9h display visual results for the test for continuity of predetermined characteristics around the focal hate crime. Throughout, the bandwidth is chosen to be 90 days. The dots correspond to a binned scatterplots. The vertical bars are 95% confidence intervals for the means of the bins, based on standard errors that are clustered on the running variable. The linear fit corresponds to a local linear regression with a triangular kernel as in Equation 1.

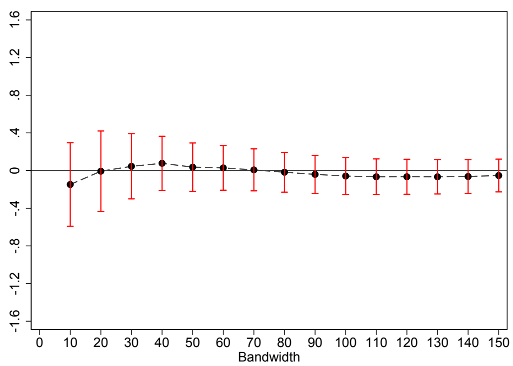
Figure 10: Sensitivity of the estimates to the bandwidth choice



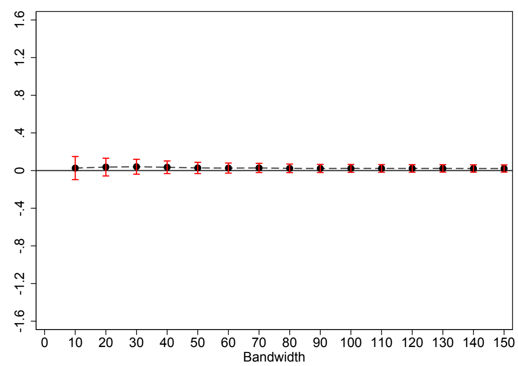
(a) MCS



(b) PHQ-4



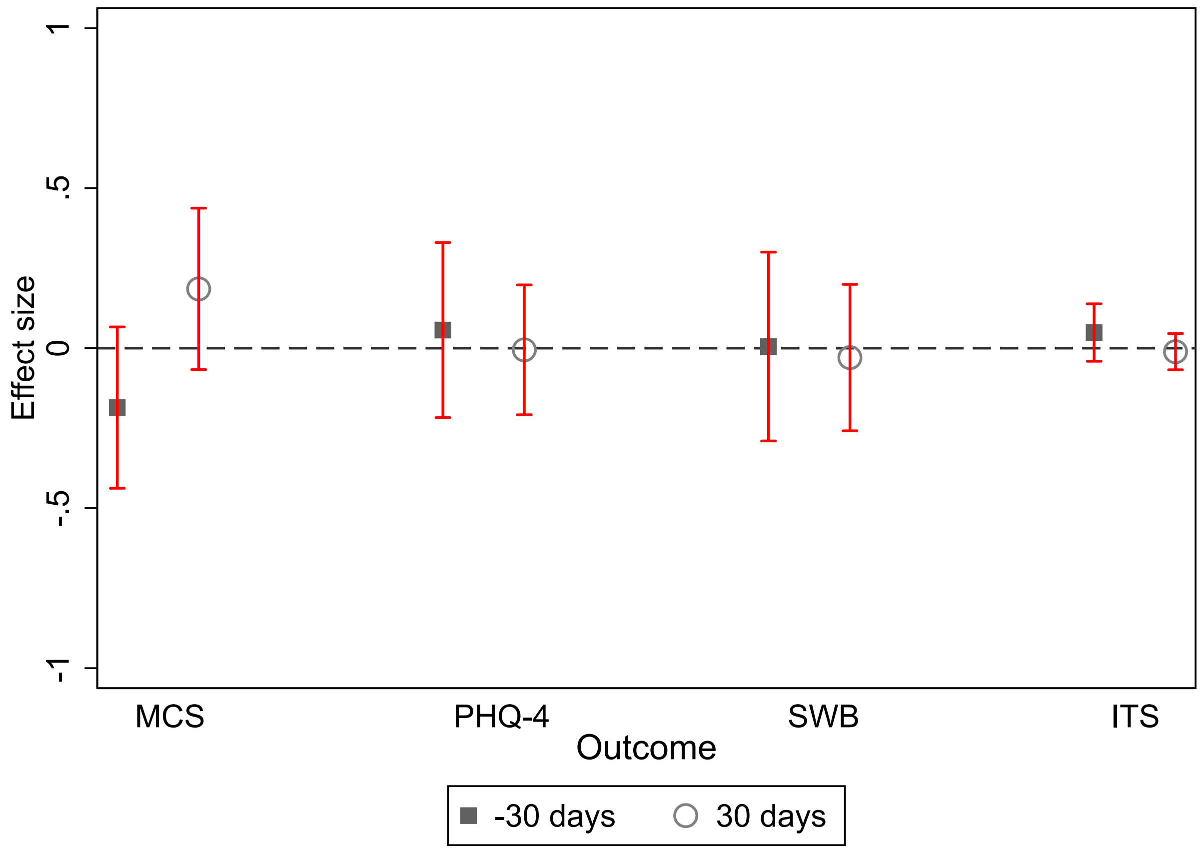
(c) Life satisfaction



(d) ITS

Note: Figures 10a to 10d display the effect of xenophobic attacks on the MCS and PHQ-4 score, life satisfaction as well as intention to stay conditional on the bandwidth choice, respectively. In each figure, a dot corresponds to a point estimate corresponding to bandwidth choice each. The estimates correspond stem from a local linear regression of the respective mental health outcome on an indicator for xenophobic attacks and a linear trend in the running variable, which is allowed to vary before and after the cutoff. We used triangular kernels. The red bars display 95% confidence bands. Throughout, we clustered standard errors on the relative distance to the xenophobic attack. Source: SOEP, v34.

Figure 11: Placebo estimates



Note: Figure 11 displays the point estimates and 95% confidence intervals of placebo tests. For each mental health outcome, the left estimates correspond to point estimates of placebo regressions, pretending the xenophobic attack happened 30 days before the actual xenophobic attack. The right estimates display the respective estimates pretending the xenophobic attack happened 30 days after the actual xenophobic attack. Source: SOEP, v34.

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