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DISCUSSION PAPER

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Do Refugees Impact Crime? Causal Evidence From Large-Scale Refugee Immigration to Germany

Do Refugees Impact Crime? Causal Evidence from Large-Scale Refugee Immigration to Germany

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Abstract

Does large-scale refugee immigration affect crime rates in receiving countries? We address this question based on the large and unexpected refugee inflow to Germany that peaked in 2015–2016. Arriving refugees were dispersed across the country based on a binding dispersal policy, yet we show that systematic regional sorting remains. Our empirical approach examines spatial correlations between refugee inflows and crime rates using the administrative allocation quotas as instrumental variables. Our results indicate that crime rates were not affected during the year of refugee arrival, but there was an increase in crime rates one year later. This lagged effect is small per refugee but large in absolute terms and is strongest for property and violent crimes. The crime effects are robust across specifications and in line with increased suspect rates for offenders from refugees' origin countries. Yet, we find some indication of over-reporting.

Keywords: Crime; Immigration; Refugees; Dispersal Policy
JEL classification: F22, J15, K42, R10

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1 Introduction

In recent years, the number of forcibly displayed individuals worldwide has risen steadily, reaching more than 82 million in 2020 (UNHCR, 2021). International migration flows have presented numerous challenges, particularly for countries receiving large numbers of refugees. Despite political support in host countries for providing humanitarian assistance to refugees, concerns have also been raised that refugee inflows are associated with rising crime rates (e.g., Bell et al., 2013).

In the case of refugee migration to Europe around 2015, such concerns were further fueled by the fact that refugee immigrants were quite young, predominantly male, and had low educational attainment—all factors that correlate with criminal activity (e.g., Freeman, 1999; Pfeiffer et al., 2018). More generally, refugees are different from other migrants in several respects that may affect criminal activity.[1] For instance, exposure to violence at origin countries could "breed violence" at destination countries (Couttenier et al., 2019). In addition, the literature has found crime to increase from immigration if immigrants have poor prospects on the labor market (Bell et al., 2013; Piopiunik and Ruhose, 2017). Refugees enter the labor market on average slower than other migrants (Chin and Cortes, 2015; Brücker et al., 2020), which may affect criminal activity. There are, however, also mitigating factors at work that make it less likely for refugees to commit crime relative to other migrants. Usually refugees have no incentives nor the option for return migration, which increases pressure to integrate in host countries (Cortes, 2004; Chin and Cortes, 2015). Whether crime really increases due to refugee immigration is, thus, an empirical question.

In order to elucidate this research question, this study analyzes the effect from refugee immigration on crime in the wake of the 2015–2016 refugee inflow to Germany, the country that received the largest absolute number of refugees in Europe.[2] The setting of refugee immigration to Germany is well suited to analyze the relationship between refugee arrivals and crime. First, the influx of refugees was substantial. Between 2015 and 2018, 1.6 million asylum seekers applied

[1] Note that all refugees have been asylum seekers upon arrival, but not all asylum seekers receive a protection status. Nonetheless, we will use the term 'refugees' throughout this study, with which we subsume asylum seekers, refugees, and those having received a rejection of their asylum application.

[2] Between 2015 and 2018, 3.9 million asylum applications were filed in the European Union—41% of these in Germany. Worldwide, Germany is ranked fifth of countries in terms of hosting refugees (UNHCR, 2021).

for refuge in Germany, thus increasing the resident population by about 2% (BAMF, 2019). Second, refugees are subject to a binding dispersal policy that allows us to examine causal effects from refugee settlement on crime rates.

Our study focuses on refugee immigration to Germany between 2013 and 2018. Linking novel administrative data on refugee arrivals to local criminal activity, we use a spatial correlation approach to estimate the impact of new refugee arrivals on crime. Specifically, we relate the annual inflow of refugees at the district level to year-to-year changes in crime rates. In order to distribute the burden of hosting newcomers, German law stipulates that arriving refugees be dispersed throughout the country. However, various factors have impaired the even allocation of refugees, including lack of housing capacity, self-selection of refugees, reporting errors, and political lobbying. In order to address these non-random deviations from the dispersal policy, we have developed an instrumental variable (IV) approach leveraging *ex-ante* fixed administrative allocation quotas. The combination of novel administrative data on refugee arrivals with newly collected allocation quotas is unique to this paper and a companion paper (Berbée et al., 2022).

Using our IV strategy, we quantify the causal impact of refugee arrivals on changes in crime rates. As crime effects of immigrant arrivals are not necessarily immediate and may take time to manifest (e.g., Piopiunik and Ruhose, 2017), we estimate the contemporaneous and the lagged crime effects. Our estimates suggest no relationship between refugee arrivals and crime rates during the year of refugee arrival, but there was an increase with a one-year lag. Specifically, we find that total crimes increased by approximately two cases per 100,000 residents for every three incoming refugees per 100,000 residents in the previous year. The increase in total crimes is driven by property crimes and by violent crimes. We estimate crime elasticities with respect to refugee arrivals in the previous year at 0.09 for property crimes and 0.16 for violent crimes. These results remain robust to a variety of checks including alternative estimation models, an alternative instrumental variable, alternative measures of refugee immigration, and controlling for spatial spillovers and police effectiveness.

Previous studies about the effect of immigration on crime show mixed results. Part of the literature finds the effects of immigration on crime in host countries to be close to zero in general (Butcher and Piehl, 1998a,b; Bianchi et al., 2012; Nunziata, 2015; Özden et al., 2018). Other studies tend to conclude that immigration increases crime under certain circumstances, particularly if immigrants have poor prospects on the labor market (Alonso-Borrego and Garoupa, 2012; Bell et al., 2013; Spenkuch, 2013; Piopiunik and Ruhose, 2017), or if they face labor market restrictions (Bell et al., 2013; Freedman et al., 2018). However, refugees are different from labor migrants and only a few studies have focused on refugee immigration. For refugee resettlement in the US, no connection between the presence of refugees and crime rates has been found (Amuedo-Dorantes et al., 2021; Masterson and Yasenov, 2021). Our study shows that crime can indeed increase as a consequence of refugee immigration—even though formal labor market access is quite liberal in Germany.

We contribute to the literature that assesses the impact of refugee immigration on crime by jointly addressing three aspects that are shown to be important for this relationship. First, we investigate the 2015–2016 case of refugee immigration to Europe, i.e. the period of the by far largest refugee inflow to Europe in the recent past. Previous studies about refugee immigration to Europe often study crime outcomes only up until 2015.[3] The inflow of Syrian refugees to Turkey appears to have increased crime cases at destination (see Akbulut-Yuksel et al., 2022, for the years 2012–2016).[4] For the refugee inflow to Germany around 2015–2016, contradictory results have been found including insignificant same-year effects (Huang and Kvasnicka, 2019; Maghularia and Übelmesser, 2019), and significant increases in crime (Gehrsitz and Ungerer, 2016; Dehos, 2021). These studies analyze outcomes up to 2015 or 2016. This short-term perspective is problematic for two reasons. It misses out on the very large refugee inflow in 2015 and 2016 and it truncates potential lagged effects of these substantial inflows on crime rates. To account for this, our investigation covers the period up to 2018.

Second, we show that distinguishing between the contemporaneous and lagged impact of refugee inflows on crime is relevant. The aforementioned literature on the inflow of refugees has largely focused on same-year effects. However, the study by Piopiunik and Ruhose (2017) has illustrated that it may take time before crime effects materialize. This seems to be particularly relevant for refugees, who arrive in Germany without a network and a job, and in a setting

[3] Fasani et al. (2019) in a cross-country comparison across Europe (1995–2015) find no statistically significant effect from refugee populations on crime rates. By contrast, evidence for rising property crime rates due to asylum immigration have been found in the UK for the period of 2002 to 2009 (Bell et al., 2013).

[4] Akbulut-Yuksel et al. (2022, Fig. 3) stress the importance of using all reported crime cases rather than only court cases to avoid selectivity. Using Higher Criminal Court cases and Basic Criminal Court cases as outcomes, Kayaoglu (2022) finds no effects from Syrian refugees on crime in Turkey.

where refugees remain in limbo for a significant period as they wait for decisions on asylum applications and face ongoing uncertainty regarding their legal status. Studies that solely focus on contemporaneous crime effects may miss out important lagged impacts of refugee immigration on crime. Our period of investigation spans sufficiently far into the future to be able to detect potential lagged crime effects.

Third, we expand the literature that focuses on impacts of refugee arrivals on host countries by developing a novel identification strategy. Our study explicitly addresses the endogeneity in refugee arrivals due to the regional sorting that remains despite the dispersal policy in place. Gehrsitz and Ungerer (2016) argue that despite deviations that have occurred from the dispersal policy, the assignment of asylum seekers to local areas was uncorrelated with economic performance indicators. Based on this argument, they use records of refugee assignments from state authorities to estimate an intention-to-treat effect in an OLS framework. We show empirically that the actual allocation of refugees is indeed correlated with demographic and economic trends of regions. This means that the actual allocation of refugees is subject to endogeneity concerns. Dehos (2021) addresses the endogeneity issue for the group of recognized refugees by employing a classical past settlement IV approach. However, this approach is not applicable to refugees who recently arrived without a meaningful network of co-nationals and to refugees who are prohibited from moving. Thus, the identification strategies employed so far either neglect important endogeneity concerns or speak only to a subset of the refugee inflow.[5] We solve this issue by developing a novel identification approach instrumenting the actual inflow of all incoming refugees by the administrative allocation quotas. Our IV approach delivers estimates of the local average treatment effect (LATE) based on districts that host refugees according to the dispersal policy—an important policy parameter. This approach lends itself to future research.

This study is structured as follows. The next section provides an overview about the arguments on potential crime incentives for refugees and on the institutional background of the asylum system in Germany. Within this section, we also discuss the dispersal policy and the systematic deviations from it. Our empirical model and approach is based on these considerations as laid out in Section 3. This is followed by a description of the data in Section 4. The results are presented

[5] Akbulut-Yuksel et al. (2022) use the distance from Turkish provinces to Syrian governorates as an instrument. However, this does not translate to the German case under study.

in Section 5, while Section 6 summarizes the sensitivity checks. Section 7 concludes.

2 Institutional Background and Refugee Allocation

2.1 Criminal Incentives of Refugees

Why could refugees have different incentives for criminal activity than other migrants or natives? Based on the seminal works of Becker (1968) and Ehrlich (1973), a crime is committed when expected returns exceed expected costs. To gauge this relationship for refugees, one needs to take account of a few specific aspects that are different for refugees than for natives or other migrants (Chin and Cortes, 2015 for an overview).

On the one hand, there are several arguments for why crime rates are expected to be higher among refugees compared to other migrants. This is because, refugees typically do not flee due to economic factors and often have limited time for preparation. Further, they tend to have worse language skills and less often own formal certificates. All of these together with institutional barriers (such as employment bans and proof of precedence requirements) contribute to the general finding that refugees enter the labor market on average slower than other migrants (Brell et al., 2020; Brücker et al., 2020; Brücker et al., 2020; Chin and Cortes, 2015; Dustmann et al., 2017; Edin et al., 2003). Poor labor market prospects have been shown to increase the criminal activity of immigrants (Bell et al., 2013; Piopiunik and Ruhose, 2017). In addition, if "violence breeds violence" (Couttenier et al., 2019) and refugees had higher exposure to violence in their origin countries (in conflict or prosecution) or along the flight, then one would also expect a higher relative propensity to commit (violent) crimes by refugees. Furthermore, living under precarious conditions in a reception center may foster criminal activity among refugees (Christ et al., 2017; Pfeiffer et al., 2018; Giesing et al., 2019), which can be exacerbated by ethnic conflicts at origin (Couttenier et al., 2019). Last, the refugee population arriving in 2015 and 2016 was dominated by young males—the demographic group at highest risk of committing crime (e.g., Freeman, 1999; Pfeiffer et al., 2018).

On the other hand, there are reasons to believe that the higher expected relative propensity for refugees to commit crimes could be attenuated in the specific case under study. For one, refugees receive social benefits in Germany that should cover at least basic needs and reduce neediness. Second, refugees have oftentimes no incentives nor the option for return migration, which increases pressure to integrate in host countries (e.g., Cortes, 2004; Chin and Cortes, 2015). The probability of staying permanently in the host country is higher for many refugees than for labor migrants, translating into higher incentives to invest in destination country-specific human capital (Cortes, 2004; Chin and Cortes, 2015). Finally, and as argued in the previous paragraph, the over-representation of young males among the group of refugees from 2015–2016 makes this group of immigrants particularly prone to commit crime. Yet, this relationship could be attenuated by the fact that—relative to older migrants—young (refugee) immigrants may be faster in acquiring new skills, which likely leads to better job matches upon labor market entry. On top of this, the refugee migration episode under study happened at a time in which the German labor market was well suited to absorb this labor supply shock. These favorable economic conditions could play a vital role in reducing the propensity to commit crime.

With respect to the timing of immigration and crime, crime could react to immigration with a delay. Piopiunik and Ruhose (2017) argue that this is indeed the case as immigrants usually arrive throughout the year. Later within a year there is little time to affect crime rates for newly arrived immigrants. This argument is particularly relevant for the episode under study where refugee inflows peaked at historical levels in *late* 2015. Furthermore, Piopiunik and Ruhose (2017) argue that newly arrived immigrants need time to get familiar with their surrounding and to build criminal networks, all of which leads to a delayed effect of immigration on crime. In addition, for the specific case for major refugee inflows around 2015–2016, administrative approval of asylum applications came with a large delay due to the large number of applications that had to be processed. This long waiting period aggravates the insecurity surrounding the application process for asylum. Insecurity about the perspective of staying is further aggravated by the temporariness of protection statuses. By and large, Syrian refugees in Germany received a subsidiary protection status granting only temporary protection for a period of one year after which renewal for another two years is possible. Family reunion was also paused in response to the 2015 peak inflow for individuals under subsidiary protection. All these measures add to a notion of temporariness that hinders labor market integration, among other things (Dustmann et al., 2017). Refugees might have learned only step by step about these institutional barriers to integration. Alongside, frustration may have grown slowly over time. This may lead to a delayed growth in criminal activity among refugees. We will therefore analyze empirically at what point in time crime rates potentially reacted to the inflow of refugees.

2.2 2015 Refugee Immigration and its Institutional Setup

In 2015, when hundreds of thousands of people sought refuge in Europe, Germany received by far the largest absolute number of refugees of any European country.The number of individuals arriving to Germany and claiming asylum was of historical size, unexpected, and concentrated in a very short interval of time (Figure A1 in the Online Appendix). The demographic characteristics of the newly arriving refugees had clear attributes: the majority of these immigrants was young and male. The share of males among all first time applicants for asylum in Germany in 2016 was 65.7%, and the share of applicants below the age of 30 was 73.8% (BAMF, 2017, p. 21).

Upon arrival, refugees register and file an application for asylum, which is then processed by the Federal Office for Migration and Refugees (Bundesamt für Migration und Flüchtlinge, BAMF).[6] In recent years, almost half of the applicants for asylum received recognition for their asylum applications, while one-third received a rejection and the remaining applications were withdrawn or cleared for other reasons (e.g. Brücker et al., 2016). For recognized refugees, protection is often granted for a period of three to five years and then has to be renewed. Rejected applicants usually receive a suspension of the requirement to leave, often for a one-year period (or less), which subsequently has to be renewed. Generally, refugees are allowed to take up any kind of employment three months after their arrival in Germany.

Germany has a binding dispersal policy for the allocation of newly arriving refugees. Upon arrival at the border, refugees are first assigned to a federal state based on the so-called "EASY" registration system, an algorithm that distributes applicants in real time.[7] Then, within each state, refugees are immediately sent to an initial reception facility (IRF) where they submit their asylum application. In a second step, after a few months, refugees are allocated to specific districts within each state. The within-state distribution quotas differ between states. While all states assign based on population size, four states also use additional characteristics to calculate

[6] The process of reaching a decision following submission of an asylum application took an average of 7.9 months in 2015, and 8.7 months in 2016 (BAMF, 2016; BAMF, 2017, also Brenzel and Kosyakova, 2019). In addition, newly-arrived refugees were required to wait an average of 4.5 months before being able to submit their application (Deutscher Bundestag, 2017).

[7] The regional distribution is based on the population share and tax revenue of the states, i.e. the so-called "Königssteiner key".

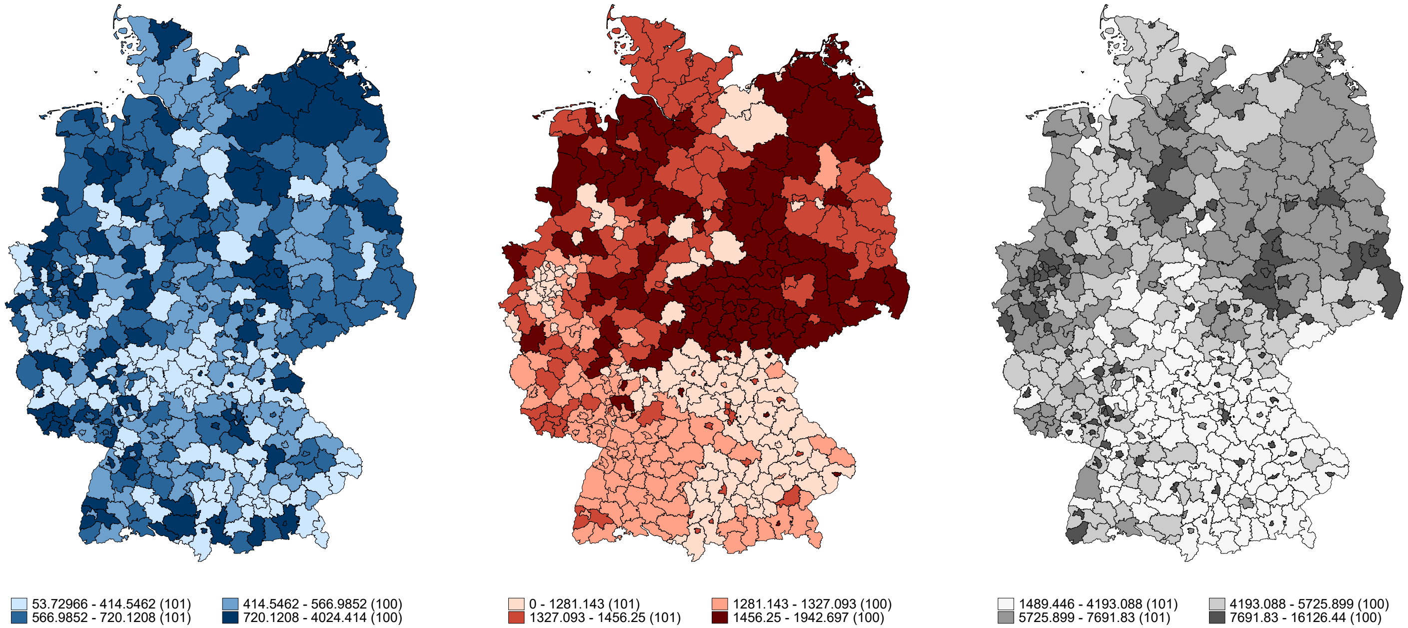
their quotas (see Table A1 in the Online Appendix). Quotas differ across states and over time due to differing base years for the population numbers and different deduction rules (e.g. for districts hosting an IRF). Importantly, refugees face a strict residence obligation, meaning they are not allowed to move residency to another place, else they lose entitlement to their social benefits.[8] This residence obligation makes the initial regional allocation binding.

Despite the dispersal policy's establishment of fixed quotas, deviations from those have occurred. We attribute these deviations to the following four reasons. First and foremost, local authorities were overwhelmed by the sheer number of arrivals and had to take pragmatic decisions to avoid homelessness. Accordingly, they sent newcomers to any place where housing was still available. This led at least in the short run to disproportionally more refugees being assigned to areas with large vacant premises, e.g. vacant military compounds (for instance Brücker et al., 2020). Second, a substantial share of refugees did not arrive at the places they originally had been assigned because, say, they preferred to continue their journey and apply for asylum elsewhere. This attrition is another risk of endogenous location choice. Third, there is likely non-random measurement error in the central registry of foreigners data set ("Ausländerzentralregister", AZR). It is documented that in late 2015, the total number of refugees was systematically under-reported (Statistisches Bundesamt, 2020a,b). This was because local authorities were overworked at the time, and had insufficient resources to properly report numbers to the registry. Fourth, there is anecdotal evidence about political lobbying by districts to host more or fewer refugees.[9]

[8] They may only be allowed to move if they can afford to support themselves. On top of this, if they move to another place without permission from local authorities, they can face legal repercussions, including imprisonment.

[9] Some cities lobbied for hosting more refugees than foreseen, such as Cottbus, Goslar, and Hettstedt, while others aimed at hosting fewer refugees, e.g. Göttingen. (Weblinks last retrieved 15.12.2022.)

Figure 1: Actual Refugee Inflows, Administrative Refugee Assignment, and Total Crime in 2015



(a) Actual refugee inflow per population (explanatory variable)

(b) Predicted refugee inflow per population (instrumental variable)

(c) Total crime per population (dependent variable)

Notes: The figure presents the district-level distributions of actual refugee inflows, predicted refugee inflows, and total crimes for the year 2015. All variables are normalized by population size in $t-1$, all per 100,000 residents.

As a result, the administrative allocation quotas have not been strictly met. Figure 1 shows the regional distribution of the actual inflow of refugees per capita, i.e. the explanatory variable in our empirical analysis (left map). Had the actual allocation of refugees strictly followed the administrative quotas from the dispersal policy, we would see no variation within a state at a given point in time, or only between districts with v. without an initial reception facility. However, Figure 1a shows substantial variation within all federal states, even within those states that only use population to determine their quotas. This is visual evidence clearly showing that the quotas have not been met.

This finding is further substantiated by Table 1. It shows how the inflow of refugees correlates on the district level with past year's district characteristics. The inflow of refugees is normalized by past year's population—often the only determinant of the quotas. State-fixed effects account for differences between federal states, e.g. with respect to their refugee allocation policies. This leaves variation across districts within federal states over time. Had the administrative allocation quotas been met, there should be no statistically significant correlations in Table 1.

In reality, refugee inflows correlate significantly with changes in the log population (column (4)) as well as with the presence of military vacancies (column (6)) that oftentimes have been used as makeshift reception centers. These trends may be related to trends in crime rates, hence introducing endogeneity concerns. Column 9 of Table 1 even presents a weakly statistical significant correlation between refugee inflows and last-year's drug crime rate. From this, we argue that using the refugee inflow as an explanatory variable in a simple OLS framework does not suffice to estimate causal effects on crime rates.

To solve this issue, we develop a novel instrumental variable approach. More precisely, we instrument the actual allocation of refugees by means of the hypothetical allocation had the administrative quotas been met. This hypothetical, quota-based allocation of refugees is shown in map (b) of Figure 1. The advantage of these hypothetical quota-based allocations is that they abstract from the irregularities in the actual allocation that otherwise induce endogeneity in the regional refugee distribution.

Table 1: Refugee Inflow on Lagged Covariates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Δ GDP per Capita	0.003							0.002	0.002
	(0.004)							(0.003)	(0.003)
Δ Unemployment Rate		7.843						12.850	11.247
		(26.403)						(27.766)	(26.621)
Δ Unemployment Rate, Foreigners			-1.151					-1.465	-1.068
			(0.957)					(0.920)	(0.856)
Δ Log of Population				3700.430**				2927.560	2656.836
				(1663.173)				(1971.458)	(1917.826)
Δ Share of Males < 35					30.861			20.021	15.940
					(27.937)			(18.096)	(16.454)
Vacant Military Compound (Dummy)						114.788**		111.521**	110.323**
						(47.005)		(46.315)	(45.227)
Urban (Dummy)							17.460	-0.342	1.240
							(22.277)	(24.944)	(24.624)
Δ Total Crime									-0.005
									(0.019)
Δ Property Crime									0.030
									(0.041)
Δ Violent Crime									0.252
									(0.190)
Δ Drug Crime									0.279*
									(0.142)
Δ Street Crime									-0.063
									(0.042)
R^2	0.31	0.31	0.31	0.32	0.31	0.33	0.31	0.34	0.35
No. Obs.	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table shows OLS estimates of actual refugee inflows on one-year lagged first-differenced covariates and crime rates. All continuous variables are normalized by population from $t-1$. Actual refugee inflow includes only those who immigrated within the past 12 months. SE clustered by district, *p<.10; **p<.05; ***p<.01.

3 Empirical Approach

3.1 Estimation Model

We estimate the effect of the large-scale immigration of refugees to Germany on crime rates across a panel of districts. As a result of the exceptional size of the inflow around 2015 and the German dispersal policy, every region experienced a substantial rise in incoming refugees in our observation period (2013-2018). We therefore use a first-differences model as is common in the literature (Bell et al., 2013; Piopiunik and Ruhose, 2017; Fasani et al., 2019; Dehos, 2021; Masterson and Yasenov, 2021; Amuedo-Dorantes et al., 2021).[10] First-differencing eliminates unobserved time-invariant heterogeneity between districts and is well suited to incorporate flows of immigrants in a spatial-correlations approach. We start from the following regression model:

$$\Delta \frac{crime_{d,t}}{pop_{d,t-1}} = \alpha + \beta \frac{\text{Inflow}R_{d,t}}{pop_{d,t-1}} + \Delta X_{d,t}\gamma + \phi_t + \theta_s + u_{d,t}\,, \tag{1}$$

where $\Delta \frac{crime_{d,t}}{pop_{d,t-1}}$ refers to annual changes in a specific crime rate, i.e. the number of crime cases per 100,000 residents, in district d in year t. $\frac{\text{Inflow}R_{d,t}}{pop_{d,t-1}}$ denotes the annual inflow of refugees R *per capita*.[11] Using the actual inflow of refugees rather than differences in stocks assures that only the group of recently arrived refugees is measured. Given the nature of refugee immigration in the period under study, we believe that this group is the relevant policy parameter.[12]

The choice of covariates $X_{d,t}$ follows Bell et al. (2013) and includes the unemployment rate, the share of the population below 35 years of age, and the log of population. Controlling for population on the right hand side is important to make sure that the numerator rather than

[10] We refrain from estimating a (dynamic) difference-in-differences approach because that approach requires many assumptions that do not appear to be satisfied in our context. As every region is treated with a significant inflow of refugees, a true control group of never-treated units is missing. In addition, changes in the doses of treatments over time complicate a meaningful comparison of crime trends within districts.

[11] We normalize by past rather than current population because the latter approach would introduce the number of recently arrived refugees into both, the dependent and the explanatory variable. For details see Akbulut-Yuksel et al. (2022).

[12] As newly arrived refugees are the main issue in public and political debates, we believe that measuring inflows rather than stocks is the more relevant approach (even in the presence of outflows). This group is also more homogeneous than what would be measured by net differences of stocks. This is because measuring differences in stocks on the district level would include refugees who have been in the country for a some time and who are therefore free to move across districts (entailing a high risk of endogenous regional sorting). Finally, our instrumental variable strategy is most accurate for the inflow of refugees having recently arrived to the country. Employing differences in the stock or the inflow of refugees as explanatory variable does not affect any of our results qualitatively, see Section 6 and Table A8 in the Online Appendix.

the denominator of the term of interest drives the results.[13] We refrain from adding further variables—at least in the main specifications—as these could potentially introduce a bad control problem (Angrist and Pischke, 2009). ϕ_t refers to a set of dummy variables for each calendar year. Some specifications will contain federal state-specific fixed effects θ_s to control for unobserved state-specific trends, e.g. in policing, refugee allocation, administration, or asylum policies.

The first differencing removes any unobserved time-constant confounders between districts in levels. This leaves within-district variation as well as variation across districts (within states) over time to identify β.

3.2 Instrumental Variable Approach

We are interested in identifying β as the effect of an increase in the inflow by one additional refugee (per capita) on crime changes (per capita). In order for β to estimate a causal effect, the allocation of refugees must be orthogonal to time-varying local crime shocks. Had the dispersal policy been rigorously implemented, we could directly interpret $\hat{\beta}$ from equation (1) as a causal effect. However, we have shown in Section 2 that deviations from the quotas have occurred and that these deviations entail some endogeneous regional sorting of refugees.

As an instrument, we use the hypothetical number of assigned refugees to districts based on the *ex ante* fixed administrative allocation quotas (see also Berbée et al., 2022). For this purpose we multiply the number of refugees assigned to the respective state through the EASY allocation algorithm by the within-state district-specific quotas that were effective at the time. That is,

$$\text{IV}_{d,t} = \text{Hypothetical allocation} = \text{Quota}_{d,t} \times \frac{\text{Abs. assignments to state}_{s,t}}{pop_{d,(t-1)}} \tag{2}$$

The number of hypothetically assigned refugees based on the administrative quotas cannot be influenced by local authorities nor by refugees themselves. This removes potential biases resulting from regional selection, e.g. through attrition along the way from the border. In addition, the EASY allocations were recorded automatically and are therefore less prone to measurement errors and delayed recording than the AZR data. The quotas are defined centrally in each state

[13]Controlling for current population as a control variable further avoids a mechanical effect: The arrival of refugees increases the local number of people that potentially are able to commit crimes. This population increase itself may increase the crime rate, if crime rates are calculated by past years population. Thus, the population increase by the arrival of refugees could mechanically raise the crime rate, even if the newcomers had the same or even a lower probability of committing crimes than natives. This mechanical effect is controlled for by adding the change in the log population as a control variable (as in Bell et al., 2013, among others).

and are based on objective criteria, mainly population (see Table A1 in the Online Appendix). Variation stems from deduction rules for districts hosting an IRF, from population development over time and lagged updating of the quotas, and from four federal states that take additional criteria into account.

After normalizing by population and adding covariates (and state-fixed effects), we argue that the quotas are unlikely to reflect local crime conditions. Nevertheless, these criteria could be correlated with our outcome variables by chance. For this reason, we provide empirical evidence that pre-treatment changes in crime rates are not correlated with the administrative quotas. Table 2 shows the results of pre-trends regressions with the actual (columns (1)-(3)) v. predicted (columns (4)-(6)) refugee inflow as the dependent variable and lagged changes in crime rates as explanatory variables. Crime trends in drug offenses and street crimes are correlated with the actual refugee inflow (columns (1)-(3)), implying that refugees are located disproportionately in districts with diverging crime trends. Once the predicted refugee inflow is used instead, i.e. columns (4)-(6), all correlations diminish and are no longer statistically significant.

For the estimation of our main results, we use a two-stage least-squares procedure based on equation (1) and cluster standard errors at the district level. The first-stage F-test statistics exceed the rule-of-thumb of 10 and will be reported together with the second-stage results in Section 5.[14]

[14]The full table of the first-stage regression results as well as the reduced form results of our main specification can be found in Table A3 and Table A4 in the Online Appendix, respectively.

Table 2: Pre-Trends Regressions

	Refugee Inflow			Predicted Refugee Inflow		
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Total Crime	-0.004	-0.006	-0.004	0.004	0.004	0.002
	(0.018)	(0.018)	(0.019)	(0.008)	(0.008)	(0.008)
Δ Property Crime	0.040	0.052	0.039	-0.012	-0.014	-0.011
	(0.048)	(0.047)	(0.045)	(0.012)	(0.011)	(0.012)
Δ Violent Crime	0.197	0.283	0.246	0.091	0.107	0.110
	(0.211)	(0.201)	(0.194)	(0.087)	(0.087)	(0.089)
Δ Drug Crime	0.286*	0.304*	0.285*	-0.049	-0.050	-0.045
	(0.154)	(0.155)	(0.150)	(0.030)	(0.031)	(0.030)
Δ Street Crime	-0.096**	-0.081*	-0.073	-0.001	-0.001	-0.002
	(0.049)	(0.046)	(0.045)	(0.017)	(0.017)	(0.017)
R^2	0.30	0.32	0.32	0.95	0.95	0.95
No. Obs.	1,970	1,970	1,970	1,970	1,970	1,970
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	Yes	Yes	No	Yes	Yes
Control Variables	No	No	Yes	No	No	Yes

Note: The table shows OLS estimates of actual and predicted refugee inflows on one-year lagged, first-differenced crime rates. All variables are normalized by population from $t-1$. Actual refugee inflow includes only those who immigrated within past 12 months. Predicted refugee inflow is based on asylum seeker registration data and assignment quotas. Control variables are first-differenced log population, unemployment rate, and share of males below age 35. SE clustered by district, *p<.10; **p<.05; ***p<.01.

4 Data

Our analysis is based on an annual panel of 394 districts[15] corresponding to the NUTS 3 level with an average of about 200,000 inhabitants.[16] The analysis covers the 2013–2018 period and combines novel data sets on refugees in Germany and the dispersal policy with official crime statistics.

To measure refugee inflow, we retrieved a customized extract from the central registry of foreigners (AZR). This database is administered by the Federal Office for Migration and Refugees (BAMF) and used by many public authorities. In contrast to other data sets or versions that

[15]"Kreise und kreisfreie Städte" are defined by German administrative boundaries. Some districts had to be joined in the empirical analysis due to only merged available refugee numbers. Section A.1 in the Online Appendix gives an overview about the aggregation.

[16]NUTS stands for the *Nomenclature of Territorial Units for Statistics* of the European Union. NUTS 3 is approximately comparable to counties in the US or local government districts in the UK.

have been used in research so far, our extract has the advantage of supplying information on the arrival month of refugees to Germany. From this, we generate the annual inflow numbers of refugees.[17] In addition, we obtained a data set from BAMF with the exact monthly allocations of refugees to federal states by the EASY system. Finally, we obtained from ministries in all federal states the yearly quotas used to determine which district receives how many refugees. We document the newly collected allocation quotas for future research, see Table A1 in the Online Appendix. Using such detailed AZR data, regional EASY-allocations and within-state allocation quotas is a major improvement in the study of the impact of refugee arrivals to Germany.

Crime data were obtained from the Federal Criminal Police Office of Germany (Bundeskriminalamt, BKA). We use aggregate figures at the district level, based on the full sample of all crimes reported to the police. Each incident of crime is counted and entered in the database regardless of whether a suspect or offender is identified or charged. Our analysis focuses on total crime and on subcategories that are likely to react to a population inflow or that are of special concern to the public. For one, we inspect potential changes in property crimes because this is the largest crime category and—according to the economic theory of crime—the one likely to be most strongly affected. Given the public interest in crimes related to perceptions of public safety, we also consider violent crimes, drug offenses, and street crimes. We discard offenses against asylum laws.[18] The regional distribution of total crimes from 2015 is displayed in the right map of Figure 1 in Section 2. Generally, crime rates have declined strongly in Germany over time (see Figure A3 in the Online Appendix).

Furthermore, we make use of information available on the potential perpetrators of crime, i.e. a subset of all crime cases. By obtaining confidential data on crime suspects from the BKA, we are also able to investigate patterns in criminal activity according to nationality.[19] Suspect data is not counted per case, but by number of persons registered, thus capturing the perpetrator dimension of a crime.

[17] Please see Section A.1 in the Online Appendix for further details.

[18] These offenses include specific crimes such as not having identification documents or entering Germany without a residence permit, i.e., crimes that are of administrative nature and that can only be committed by immigrants and not by natives. See also Gehrsitz and Ungerer (2016) for a discussion.

[19] Specifically, we acquire data on the number and nationality of suspects for property crime, violent crime, drug offenses, and street crime. In the German criminal justice system, a suspect is a person who is considered by the police to have committed a specific crime. Suspects are usually the outcome of a police investigation and similar to arrest rates in other countries.

We use additional regional data at the district level maintained by the Federal Statistical Office on the unemployment rate and share of the population below 35 years of age, and (the log of) the overall population. Descriptive statistics on these variables can be found in Table A2 in the appendix.

5 Results

In this section, we present our main OLS and IV estimates of the large refugee immigration on crime in Germany around the years 2015–2016. We first investigate a contemporaneous relationship between crime rates and refugee inflows. Based on the arguments made in Section 2, we also investigate lagged crime effects of refugee arrivals. Finally, we investigate suspect rates that corroborate our findings on crime rates.

5.1 Contemporaneous Refugee Inflow

Table 3 reports the results of regressing crime rates on the contemporaneous (same-year) inflow of refugees per district by estimating equation (1). OLS estimation results in the top panel show small but negative coefficients from contemporaneous refugee immigration on local crime rates; these estimates are not statistically different from zero. IV estimation results (in the bottom panel) switch signs and are of a much larger magnitude than the OLS results. Put differently, OLS appears strongly downward biased which we attribute to regional sorting. When the quota is violated, refugees sort disproportionately into large cities which also have higher but more strongly declining crime rates than other districts (see Table 1 on the regional selection of refugees and Figure A2 in the Online Appendix for a visual inspection of crime trends). In first differences, OLS would thus correlate a higher inflow of refugees with a stronger decline in crime rates, thus mistakenly reporting a very small or even negative coefficient. By contrast, our IV approach takes into account regional sorting by considering only variation that can be explained by the administrative allocation quotas.

We prefer the specification including state fixed effects because refugee allocation and police forces are both managed at the state level. In this same-year analysis, total crimes increase by nearly 0.5 for each additional refugee per 100,000 residents assigned to a district (column 2). The largest coefficient for a single crime category applies to property crimes (0.20). However, none of the coefficients of interest is statistically different from zero.

In order to make these estimates comparable to each other and to the literature, we estimate the corresponding crime elasticities. Given an average inflow of refugees of 338 and a level of 6007 total crimes per year (all normalized by 100,000 residents), the increase is estimated to be 2.8% (i.e. 338 additional refugees * 0.491 over 6007 total crime cases). Meanwhile, the inflow of refugees was 77.4% (i.e. an inflow of 338 over an average stock of 436 refugees). From this, the elasticity of the total crime rate is 0.036 (=2.8% / 77.4%). The elasticities for the other crime categories are all comparatively small as well (0.041 for property crime, 0.052 for violent crime, -0.004 for drug crimes, and 0.004 for street crimes).

Finding insignificant results from the contemporaneous inflow of refugees on crime is well in line with findings from Italy (Bianchi et al., 2012) and from Germany (Maghularia and Übelmesser, 2019; Huang and Kvasnicka, 2019; Dehos, 2021), and also for ethnic Germans from the former Soviet countries who immigrated to Germany (Piopiunik and Ruhose, 2017). At the same time, Dehos (2021) finds large crime increases for the group of recognized refugees in Germany based on a past settlement IV approach. Similarly, Akbulut-Yuksel et al. (2022) find smaller, yet significant, crime increases from Syrian refugee immigration to Turkey. Overall, our estimates appear to lie in between those previous studies.

We conclude that hosting refugees does not appear to drive up contemporaneous crime rates. Yet contemporaneous estimates of refugee arrivals may not be the most informative with respect to committing crime as refugees may need time to adapt to their new circumstances. Therefore, we also analyze lagged crime effects in the next subsection.

Table 3: Contemporaneous Effects on Crime Rates

	Crime		Property		Violence		Drugs		Street	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
OLS										
Refugee Inflow	-0.046	-0.035	-0.026	-0.020	0.001	0.001	0.004	0.005	-0.008	-0.001
	(0.050)	(0.046)	(0.032)	(0.029)	(0.003)	(0.003)	(0.011)	(0.011)	(0.026)	(0.024)
R^2	0.11	0.12	0.17	0.19	0.09	0.10	0.02	0.03	0.06	0.08
IV										
Refugee Inflow	0.353	0.491	0.149	0.204	0.025	0.023	-0.005	-0.003	-0.025	0.008
	(0.300)	(0.345)	(0.167)	(0.188)	(0.020)	(0.022)	(0.046)	(0.055)	(0.082)	(0.087)
First Stage Estimate	0.348	0.327	0.348	0.327	0.348	0.327	0.348	0.327	0.348	0.327
First Stage SE	0.096	0.095	0.096	0.095	0.096	0.095	0.096	0.095	0.096	0.095
First Stage F-Stat	13.26	11.91	13.26	11.91	13.26	11.91	13.26	11.91	13.26	11.91
No. Obs.	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970
Control Variables	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X
State FE		X		X		X		X		X

Note: 2SLS of first-differenced crime rates on annual refugee inflows. All main variables normalized by population from $t-1$. Refugee inflow includes only those wo immigrated within the past 12 months. Control variables are the first-differenced log population, unemployment rate, and share of males below age 35. SE clustered by district, *p<.10; **p<.05; ***p<.01.

5.2 Lagged Refugee Inflow

We estimate the effect of past year's inflow of refugees on current crime rates. In other words, we are now considering InflowR_{t-1} instead of InflowR_t in equation 1. The top panel of Table 4 shows positive but rarely significant results from the OLS specification, while instrumenting the inflow of refugees gives large and often statistically significant coefficients (bottom panel). These differences in OLS and IV results may to a large extent be based on regional sorting into large cities with declining crime trends as argued in Section 5.1. According to our preferred specification, which includes state-fixed effects, the total crime rate increases by 0.72 for every additional refugee per 100,000 residents assigned to a district. This is again driven mainly by property crimes (0.44) while violent and street crimes also increase notably. Yet, the effects are again small when measured in percentage responses to a 1% increase in refugee immigration, i.e. in elasticities.[20] Violent crimes again exhibit the largest effect when measured in terms of elasticities. Drug crimes do not change significantly on account of the refugee immigration.

These estimated crime effects are smaller than those found in previous studies when considering the effect relative to the size of the immigrant inflow. For instance, Bell et al. (2013) estimate a crime elasticity from asylum seekers on total crime in the UK of 0.16 and, likewise, Dehos (2021) estimates an elasticity about 0.16 for recognized refugees in Germany. Piopiunik and Ruhose (2017), who study immigration from former Soviet countries by ethnic Germans, report an even higher crime elasticity of 0.39 for the lagged effect of immigration on total crime. We measure an elasticity for total crime of 0.05—only a fraction of the effect found by previous studies. At the same time, the pattern of the timing of effects is in line with literature that has argued that there is a lagged effect from immigration on crime. In particular, Piopiunik and Ruhose (2017) find no same-year effect, but a significant increase in crimes in the first year after arrival of the immigrants. Interestingly, the coefficients of one-year lagged immigration inflows on total crimes are of comparable magnitudes (0.492 in the most comparable specification by Piopiunik and Ruhose, 2017, p. 267).[21] Nevertheless, the recent inflow of refugees has been much larger

[20] The elasticities are 0.052 for total crime, 0.087 for property crime, 0.162 for violent crime, -0.045 for drug crime and 0.071 for street crime.

[21] In Table A5 in the Online Appendix, we also regress crime rates on the inflow of refugees from $t-2$, following again Piopiunik and Ruhose (2017). As in Piopiunik and Ruhose (2017), we do not estimate statistically significant coefficients of immigration on crime at $t-2$. This implies that there is no additional increase in crime rates that goes beyond the increase estimated in period $t-1$.

than the past inflow of ethnic Germans, thus resulting in smaller elasticities in our case. We conclude from this that refugee inflows do increase crime rates, but only with a certain time lag. We further discuss the implications of this finding in Section 7.

Importantly, due to the IV framework, these results represent local average treatment effects (LATE) and are thus driven by the compliers. In the setup under study, one could interpret districts as compliers if they host refugees according to their quota-based assignment. Put differently, these districts host more refugees if (and only if) the administrative allocation process requires them to do so. Complier districts neither undercut the assignments nor do they attract an excess number of refugees. Therefore, our estimated effect is likely driven by 'average' districts, i.e. not the most and not the least attractive places in Germany. This interpretation is reassuring as it also means that the estimated effects rest on a broad set of districts and not on outliers. We conclude that hosting refugees leads to increasing crime rates at least in those districts that comply with the allocation quotas.

Table 4: Lagged Effects on Crime Rates ($t-1$)

	Crime		Property		Violence		Drugs		Street	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
OLS										
Refugee Inflow ($t-1$)	0.082	0.103	0.017	0.029	0.004	0.005*	0.015**	0.017**	-0.009	-0.000
	(0.070)	(0.063)	(0.040)	(0.035)	(0.003)	(0.003)	(0.007)	(0.007)	(0.034)	(0.031)
R^2	0.11	0.13	0.17	0.19	0.09	0.11	0.02	0.03	0.06	0.08
IV										
Refugee Inflow ($t-1$)	0.597	0.716*	0.378**	0.436**	0.069***	0.073***	-0.038	-0.038	0.159*	0.196**
	(0.387)	(0.402)	(0.191)	(0.192)	(0.019)	(0.020)	(0.051)	(0.051)	(0.089)	(0.087)
First Stage Estimate	0.370	0.353	0.370	0.353	0.370	0.353	0.370	0.353	0.370	0.353
First Stage SE	0.098	0.095	0.098	0.095	0.098	0.095	0.098	0.095	0.098	0.095
First Stage F-Stat	14.20	13.71	14.20	13.71	14.20	13.71	14.20	13.71	14.20	13.71
No. Obs.	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970
Control Variables	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X
State FE		X		X		X		X		X

Note: 2SLS of first-differenced crime rates on annual refugee inflows. All main variables normalized by population from $t-1$. Refugee inflow includes only those who immigrated within the past 12 months. Control variables are the first-differenced log population, unemployment rate, and share of males below age 35. SE clustered by district, *p<.10; **p<.05; ***p<.01.

5.3 Suspect Rates

The previous results show significant increases in crime rates for regions that had been assigned larger numbers of refugees. Even though crime rates are the key policy variable for public security, they miss important information on who is actually committing the crimes. In order to address this lack of information, we complement our previous analysis by using suspect rates as an alternative outcome variable.

Technically, while crime rates measure all reported crimes, suspects are only found for a subset of those crimes.[22] That is, suspect rates depend on clearance rates which may differ by the nationality of the offender. Specifically, when many refugees enter a region, suspect rates may increase due to changed reporting or policing behavior. While the latter could be partly picked up by the inclusion of state-fixed effects, we cannot control for the former. Hence, the following results have to be treated with caution, because they cannot be interpreted directly as evidence for changed criminal activity. They are, however, informative about which group of offenders could be increasing their criminal activity in response to the inflow of refugees.

Table 5 shows estimation results from our IV approach using the suspect rate for two groups of suspects as a dependent variable: first for suspects from refugee countries (top) and second for native suspects (bottom). The number of suspects from refugee countries increases significantly with the arrival of refugees. It is reassuring to see that the increase in suspects from refugee countries in total crimes is again driven mainly by property crimes. The estimated elasticities are substantially larger now than they were before (0.172 for total crime, 0.378 for property crime, 0.328 for violent crime, 0 for drug crime and 0.419 for street crime). This is plausible, as suspect rates measure only a selected subset of crimes (i.e. those for which the police found a likely offender). This may explain the finding of higher elasticities in this context compared with the main results (Table 4).

[22]See Akbulut-Yuksel et al. (2022) for a detailed discussion. For example, the study by Huang and Kvasnicka (2019) analyzes only crimes committed by foreigners from the top refugee-sending countries against natives. Thus, effects on crimes committed by natives or other foreigners, or by anyone *against* foreigners are excluded by design from their study.

Table 5: Alternative Dependent Variable: Suspect Rates by Group (IV)

	Crime		Property		Violence		Drugs		Street	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Suspect Rates for Refugee Origin Countries										
Refugee Inflow $(t-1)$	0.120*	0.123*	0.064*	0.065*	0.029***	0.030***	-0.003	-0.000	0.028***	0.029***
	(0.066)	(0.068)	(0.037)	(0.039)	(0.010)	(0.010)	(0.007)	(0.007)	(0.009)	(0.010)
Suspect Rates for Natives										
Refugee Inflow $(t-1)$	0.086	0.120	0.004	0.020	0.021*	0.023*	-0.005	-0.003	-0.006	0.000
	(0.114)	(0.123)	(0.028)	(0.029)	(0.012)	(0.013)	(0.033)	(0.032)	(0.026)	(0.029)
First Stage Estimate	0.370	0.353	0.370	0.353	0.370	0.353	0.370	0.353	0.370	0.353
First Stage SE	0.098	0.095	0.098	0.095	0.098	0.095	0.098	0.095	0.098	0.095
First Stage F-Stat	14.20	13.71	14.20	13.71	14.20	13.71	14.20	13.71	14.20	13.71
No. Obs.	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970
Control Variables	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X
State FE		X		X		X		X		X

Note: 2SLS of first-differenced main refugee country suspect rates on annual refugee inflows. All main variables normalized by population from $t-1$. Refugee inflow includes only those who immigrated within the past 12 months. Control variables are the first-differenced log population, unemployment rate, and share of males below age 35. SE clustered by district, *p<.10; **p<.05; ***p<.01.

The results for natives are small and insignificant in all but violent crimes.[23] Finding an increase in violent crimes for natives could be explained by increasing anti-refugee hate crimes due to the 2015–2016 refugee inflow in Germany (as shown by Entorf and Lange, 2023). Yet, estimates for native suspects are not always significantly different from the estimates for suspects from refugee-sending countries. These estimation results by and large suggest that natives' criminal activity does not respond to the refugee immigration.

These results show clearly that the increase in crime rates goes hand in hand with increased suspects rates from refugee countries. Hence, it is very likely that the incoming refugees indeed commit additional crimes that are then reflected in an increase in crime rates.

6 Robustness Checks

We now turn to inspecting the robustness of our empirical results by varying the empirical model, the instrumental variable, the measure of refugee immigration, the control variables, and a potential heterogeneity by accommodating refugees, respectively. Finally, we review our results with respect to a potential over-reporting bias.

First, we check the robustness of our results with respect to varying the empirical model. Instead of using a first-differenced model, we now estimate a model in levels including district-fixed and year-fixed effects (as used by Butcher and Piehl, 1998a; Spenkuch, 2013; Özden et al., 2018). The results are presented in Table A6 and are very close to our previous main results in Table 4. This is a strong indication that the results reported here do not depend on the choice of empirical model but hold across specifications.

Second, we rebuild our main instrumental variable by replacing refugee arrivals from the EASY registration data by the refugee inflow according to the AZR data. This aggregate state-wide inflow is then multiplied with the district-specific allocation quotas in order to obtain an instrument that mirrors our baseline IV. Using the AZR arrivals instead of the EASY registrations may come at the advantage of having a more accurate measure of the number of refugees that stay in Germany for a longer time. Yet, it may also come at the expense of measurement errors due to delayed registrations that shift the arrival of refugees to later years within our data set. It is unclear whether the latter argument invalidates the exclusion restriction. The following

[23] All elasticities are below 0.01.

analysis should thus be taken with a grain of salt.

Table A7 shows the results for our main specification but using the alternative instrument based on aggregate refugee inflows from the AZR data. The F-statistic for instrument relevance increases notably. We attribute this to the fact that the predicted distribution of refugee arrivals based on the AZR are closer to the actual distribution of refugees than using the original instrument based on EASY registrations. The results remain somewhat similar, but we estimate much smaller coefficients than in our baseline IV approach. In particular, we cannot find a statistically significant effect for property and street crimes, while we still find a statistically significant increase in total and violent crimes.

Third, we use an alternative variable to measure the arrival of refugees, the difference in refugee populations. While the main analyses used the inflow of refugees in the past 12 months, we now use the difference in stocks of refugees. Table A8 shows first stage F-test statistics of only around 10, which is lower than before. This is not surprising given that this measure of the explanatory variable is less precise because it also contains outflows from the districts. Still, the results given by this refugee measure are extremely close to our main results for the lagged refugee inflow (Table 4).

Fourth, we turn to including additional covariates in the empirical model that have been suggested by the literature. One potential concern about the analysis so far could be that our estimates suffer from spatial spillovers in the dependent variable. For instance, refugees allocated to one district could commit crimes in a neighboring district. If this were the case, we would underestimate the impact of refugee immigration on crime. Following Zenou (2003) and Piopiunik and Ruhose (2017), we include spatial lags of the crime rates as additional control variables in our empirical model (1). These are measured as the number of crimes in the respective crime category per 100,000 residents of all other districts, weighted by the travel time by car between districts' centroids. The results in the top panel of Table A9 are again very close to our main results (Table 4). This suggests that spatial crime spillovers do not affect our estimates.

As another additional covariate, we add crime clearance rates to our empirical model. This should account for the potential concern that clearance rates might have changed in response to the refugee inflow, which could artificially bias our results upward. Instead, the results shown in the bottom panel of Table A9 are again extremely close to our main results (Table 4).

Fifth, we analyze whether our main findings depend on the way refugees are hosted within the district as previous studies documented the importance of refugee accommodations for the impact of refugee immigration on the incumbent population (Steinmayr, 2021; Bredtmann, 2022; Gehrsitz and Ungerer, 2016). In particular, Gehrsitz and Ungerer (2016) find an increase in drug offenses that is concentrated in districts that host initial refugee reception facilities (IRF). These state-run, large scale reception facilities host many refugees at the same time for a short period. On the one hand, districts with such reception facilities may be more likely to witness a surge in crime, because these facilities host large groups of refugees that are not allowed to work and restricted in moving freely across Germany. On the other hand, stays in these facilities are by and large transitional, which may restrain efforts and chances for refugees to undertake illegal actions.

In order to analyze whether districts that host refugees in an IRF see larger increases in crime rates, we expand our baseline empirical strategy by including information on the presence of state-run IRFs in a district in our empirical model. Running such IRFs may be correlated with local factors, i.e. IRFs are usually placed in urban areas with low housing demand. We, therefore, instrument the presence of an IRF with the availability of vacant military premises (e.g. analogous to Steinmayr, 2021; Berbée et al., 2022). We augment our baseline estimation equation by including an interaction term of the refugee inflow with a dummy variable $IRF_{d,2015}$ that indicates whether a district ran an IRF in 2015:

$$\Delta\frac{crime_{d,t}}{pop_{d,t-1}} = \alpha + \beta_1\frac{\text{Inflow}R_{d,t}}{pop_{d,t-1}} + \beta_2\frac{\text{Inflow}R_{d,t}}{pop_{d,t-1}} * IRF_{d,2015} + \Delta X_{d,t}\gamma + \phi_t + \theta_s + u_{d,t}\,. \quad (3)$$

In order to identify our main coefficients of interest, we use the same instrument for refugee inflows as presented in Section 3 and additionally include an interaction of this instrument with a dummy variable that captures the presence of large vacant military buildings. The intuition for using this instrument is that due to the rising numbers of asylum applications many new IRFs opened across Germany. As large scale vacant buildings that are suitable for hosting many people are scarce, vacant military buildings have been converted in order to meet the housing demand of refugees. Whether a district features such military buildings or not should be unrelated to refugee immigration because these premises are usually old and have been closed some years prior to the rise in immigration (please consult Berbée et al., 2022, for a more detailed discussion).

Table A10 presents the OLS and IV results of the model described by equation (3). While each coefficient of interest is statistically not distinguishable from zero when estimating OLS, we find large and statistically significant estimates for the refugee inflow when applying our IV strategy. In fact, the results for the non-interacted term are very close to our main results in Table 4. At the same time, the estimates for the interaction term are always comparatively small and statistically insignificant. We conclude from this analysis that the effect of refugee inflows on crime does not differ between districts with or without large-scale IRFs. Put differently, the presence of large scale reception centers does not appear to drive the crime effect of refugee immigration.

Finally, we exploit whether the crime effects from refugee immigration are subject to an over-reporting bias. An over-reporting bias may be relevant in our case if in districts with larger inflows of refugees, victims report crimes more often than in districts with fewer refugee arrivals. This bias could come into effect when people attribute crimes to a specific group of people based on prejudice. As the largest number of arriving refugees comes from origin countries that have populations that are ethnically distinct from the majority of the native German population, an over-reporting bias based on prejudice in appearance is plausible.

In order to empirically test for an over-reporting bias in reporting behavior or in police attention, we consider suspect rates of Turks. They constitute one of the largest groups of foreigners living in Germany. If the increased number in crimes was at least partly driven by over-reporting, Turks may experience an increase in their suspect rate. This may be the case due to an arguably great overlap of visible ethnic markers such as hair and skin color between refugees from Middle Eastern countries and Turks. Therefore, we utilize suspect data on Turks and perform the same regression as in Section 5.3.

Table A11 summarizes the regression results with the suspect rate of Turks as dependent variable. For total crime and property crime, we find very small but statistically significant relationships between refugee arrivals and increased suspect rates for Turks in Germany. This result may be interpreted as a sign that over-reporting for those crime categories may indeed be present.[24] However, as these estimates are rather small, they can explain neither the larger

[24]Another interpretation could be that Turks perform more property crimes in districts with larger refugee arrivals, presumably victimizing the newcomers.

effects that we found for suspects from refugee origin countries, nor the increases in violent and street crimes.

Overall, our estimated crime effects are robust to a variety of different empirical sensitivity checks. Over-reporting appears to explain a small part of the overall crime effect.

7 Discussion and Concluding Remarks

The impact of refugee immigration on receiving countries is a contested topic around the world (e.g. Edo et al., 2020). This article contributes to this debate by investigating the impact of the recent large-scale immigration of refugees to Europe on crime. The effect of refugees on crime is unclear *ex ante*. While this group of immigrants was predominantly young and male—factors that go along with higher criminal activity—refugees often cannot return to their origin countries, which increases pressure to integrate in host societies (Cortes, 2004; Chin and Cortes, 2015). In the German case under study, the settlement of the large and unanticipated number of refugees was governed by a binding dispersal policy. However, this dispersal policy was not strictly adhered to, at least not in the short run or during the peak of the inflow. We show empirically how deviations from the quota correlated with economic and demographic trends. Therefore, we argue that using the actual allocation of refugees as the explanatory variable is subject to endogeneity concerns. Employing an instrumental variable estimation strategy that instead rests on pre-defined refugee assignment quotas across German districts, we quantify the impact of recent refugee arrivals on crime rates.

Similar to other studies that investigate the immigration–crime nexus, we do not find a same-year impact on crime rates from refugee arrivals. However, focusing on lagged inflows of refugees, we estimate statistically significant increases in crime rates in regions with larger refugee arrivals in the previous year. The increase in total crimes seems to be driven by property crimes and violent crimes. This lagged effect of refugee arrivals on crime is intuitive, as it may take a certain amount of time for refugees to engage in criminal behavior. In the German context in the 2015–2016 period, refugees stayed a significant time in limbo as they waited for their asylum application decisions. Once an asylum application is decided, the legal status has to be renewed regularly every few years, threatening medium- to long- term employment perspectives. This ongoing uncertainty about their legal status hampers refugees' language learning investments and labor market integration. This holds despite a comparatively liberal formal labor market access. Potentially, refugees may need some time to adjust and may only slowly become disenchanted by their prospects and turn to criminal activity as an alternative way to generate income.

Putting our results into perspective, our estimated crime effects are small when considered relative to the immigrant inflow per refugee. For instance, Bell et al. (2013) estimate a crime elasticity for asylum seekers in the UK of 0.16 and Dehos (2021) of about 0.16 for recognized refugees in Germany. Piopiunik and Ruhose (2017) report an even higher crime elasticity of 0.39 for the lagged effect of immigration on total crime. We measure an elasticity for total crime of 0.05—only a fraction of the effect found by previous studies. Akbulut-Yuksel et al. (2022) also find substantial crime effects for Syrian refugees in Turkey that are, however, still smaller than our estimates. Overall, the absolute crime increases implied by our estimates appear to lie somewhere in between those estimates.

One potential explanation for finding comparatively small crime effects could be provided by the relatively high welfare benefits in Germany compared with other countries. This circumstance may mitigate potentially larger impacts on crime from refugee arrivals. Couttenier et al. (2019) suggest that better labor market access for asylum seekers largely reduces the crime effect. Future research should investigate more into the channels that drive crime rates up when refugees enter a country. This should help designing better policies to prevent crimes.

While we find that refugee arrivals are also associated with an increase in suspects from refugee origin countries, we cannot draw any conclusions about the victims of these crimes. Finding substantial increases in crime rates does not necessarily mean that natives are more often victimized than before. Instead, a large fraction of the additional crimes may take place between refugee groups (Couttenier et al., 2019; Huang and Kvasnicka, 2019). Furthermore, crime rates may increase in response to refugee immigration due to increasing police attention and victims reporting behavior, especially when the offender looks non-native. We find some empirical support for such over-reporting. While this line of argument may apply to some types of property crime, it is less applicable to violent crimes where reporting rates are very high to begin with.

Finally, the results obtained in this study are based on a spatial correlation approach at the district level. Our estimated LATE provides information about the reaction in crime rates to inflows of refugees into districts that stick to the assignments. In this sense, our results speak to a large and policy-relevant group of districts that do not substantially undercut or overshoot the assignments. Districts that do deviate from the assignments do not drive our results but may exhibit different crime trends. Therefore, the crime effects reported in this study do not translate into an aggregate effect for all of Germany. The contribution of this article lies in the causal identification of the impact of refugees on crime in the absence of regional sorting.

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A Online Appendix

A.1 Data Appendix

This appendix describes further details on the data.

Our empirical analysis is on the district level. Since 2016, Germany has 401 districts. The BAMF refugee data entails accurate figures for 391 districts but reports only aggregate numbers for the entire state of Saarland (six districts) and for two districts within each of the states of Hesse (Stadt and Landkreis Kassel) and Brandenburg (Stadt Cottbus and Spree-Neiße Kreis). This results in 394 districts according to the BAMF classification as opposed to the 401 districts according to the official administrative classification. We aggregate all other regional data at the district level for those aggregated districts from the BAMF classification in order to have a consistent data set for our empirical analysis.

From the AZR data set, we include the following residence status levels to our measure of refugees: (1) Foreign nationals who expressed their wish to seek asylum at the border but have not yet formally filed an asylum application (Asylgesuch gestellt); (2) Foreigners whose asylum application is still processed (Aufenthaltsgestattung); (3) Refugees who have a residence permit because their asylum application has been (at least temporarily) accepted (Aufenthaltserlaubnis aus völkerrechtlichen, humanitären oder politischen Gründen); (4) Foreign nationals whose asylum application has been rejected but who are tolerated in Germany because they cannot be deported (Duldung). (5) Foreign nationals without any formal residence status if they possess the nationality of one of the eight most frequent asylum seeker countries of origin (Afghanistan, Eritrea, Iraq, Iran, Nigeria, Pakistan, Somalia, Syria).

A.2 Tables

Table A1: Assignment Rules by Federal State

Federal state	Criteria included in allocation key	Population data from	Name of state law	Data obtained from
Baden-Württemberg	Population	Previous year	Gesetz über die Aufnahme von Flüchtlingen (Flüchtlingsaufnahmegesetz - FlüAG)	Regierungspräsidium Karlsruhe (Referat 92 – Landesweite Steuerungsaufgaben)
Bavaria	Population & Urban indicator	2006 (until 2016)	Verordnung zur Durchführung des Asylgesetzes, des Asylbewerberleistungsgesetzes, des Aufnahmegesetzes und des §12a des Aufenthaltsgesetzes (Asyldurchführungsverordnung - DVAsyl)	Bayerisches Staatsministerium für Familie, Arbeit und Soziales
Berlin	*Not applicable bc. city state*			
Brandenburg	Population & Area & Share soc. sec. employees*	Previous year	Verordnung über die Durchführung des Landesaufnahmegesetzes (Landesaufnahmegesetz-Durchführungsverordnung - LAufnGDV)	Ministerium für Arbeit, Soziales, Gesundheit, Frauen und Familie des Landes Brandenburg
Bremen	*Not applicable bc. city state*			
Hamburg	*Not applicable bc. city state*			
Hesse	Population & Indicator for & large share of foreigners	Previous year 30.06.	Gesetz über die Aufnahme und Unterbringung von Flüchtlingen und anderen ausländischen Personen (Landesaufnahmegesetz) & Verordnung zur Änderung der Verteilungs- und Unterbringungsgebührenverordnung	Hessisches Ministerium für Soziales und Integration
Mecklenburg-West Pomerania	Population	Previous year 31.12.	Landesverordnung zur Bestimmung von Zuständigkeiten auf dem Gebiet der Zuwanderung und zur Durchführung des Flüchtlingsaufnahmegesetzes (Zuwanderungszuständigkeitslandesverordnung - ZuwZLVO M-V)	Mecklenburg-Vorpommern Ministerium für Inneres, Bau und Digitalisierung
Lower Saxony	Population	Unspecified	Gesetz zur Aufnahme von ausländischen Flüchtlingen und zur Durchführung des Asylbewerberleistungsgesetzes (Aufnahmegesetz - AufnG)	Niedersächsisches Ministerium für Inneres und Sport
North Rhine-Westphalia	Population & Area	Unspecified	Gesetz über die Zuweisung und Aufnahme ausländischer Flüchtlinge (Flüchtlingsaufnahmegesetz - FlüAG)	Information und Technik Nordrhein-Westfalen (IT.NRW)
Rhineland Palatinate	Population	Two years ago 31.12.	Landesaufnahmegesetz & Landesverordnung zur Durchführung des Asylverfahrensgesetzes (AsylVfGDVO)	Ministerium für Familie, Frauen, Jugend, Integration und Verbraucherschutz Rheinland-Pfalz
Saarland	Population	Considers deviations	Landesaufnahmegesetz (LAG) & Saarländische Aufenthaltsverordnung	Ministerium für Justiz
Saxony	Population	Previous year 30.06.	Sächsischen Flüchtlingsaufnahmegesetzes (SächsFlüAG)	Sächsisches Staatsministerium des Inneren
Saxony-Anhalt	Population	Half-yearly 15.01. & 15.07.	Verordnung über die Ausführung des Aufnahmegesetzes (Aufnahmegesetzausführungsverordnung - AufnGAVO)	Ministerium für Inneres und Sport des Landes Sachsen-Anhalt
Schleswig-Holstein	Population	Previous year 30.03.	Landesverordnung zur Regelung von Aufgaben und Zuständigkeiten der Ausländerbehörden und bei der Aufnahme von Spätaussiedlerinnen und Spätaussiedlern sowie ausländischen Flüchtlingen und zur Einrichtung und dem Verfahren einer Härtefallkommission (Ausländer- und Aufnahmeverordnung - AuslAufnVO)	Statistisches Amt für Hamburg und Schleswig-Holstein
Thuringia	Population	Unspecified	Thüringer Flüchtlingsverteilungsverordnung (ThürFlüVertVO)	Thüringer Ministerium für Migration, Justiz und Verbraucherschutz

Source: Own collection of federal state legislations.

Table A2: Summary Statistics

	Mean	SD	Median	Min	Max	N
Crime Cases per 100,000 Residents						
Total Crime	6,007.25	2,526.63	5,535.22	1,489.45	16,126.44	1,970
Property Crime	2,182.14	1,266.09	1,936.84	437.81	8,023.06	1,970
Violent Crime	196.75	98.34	167.96	44.58	657.94	1,970
Drug Offenses	367.36	193.33	317.85	69.75	1,505.09	1,970
Street Crime	1,218.32	698.27	1,063.47	236.14	4,553.00	1,970
Δ Total Crime	-133.98	500.74	-126.67	-5,517.34	3,806.82	1,970
Δ Property Crime	-106.78	272.22	-87.76	-2,045.32	1,337.30	1,970
Δ Violent Crime	0.42	27.99	0.62	-187.43	175.30	1,970
Δ Drug Offenses	22.26	85.10	16.02	-486.68	774.16	1,970
Δ Street Crime	-43.38	162.52	-39.91	-1,475.18	1,610.05	1,970
Suspects from Main Refugee Countries per 100,000 Residents						
Total Crime	313.21	208.18	255.08	34.78	1,275.79	1,970
Property Crime	74.69	62.84	56.75	2.62	496.38	1,970
Violent Crime	39.89	26.83	33.69	0.00	167.14	1,970
Drug Offenses	27.98	27.60	19.78	0.00	224.33	1,970
Street Crime	30.25	24.43	23.25	0.00	213.44	1,970
German Suspects per 100,000 Residents						
Total Crime	1,904.75	618.97	1,848.14	591.64	4,296.08	1,970
Property Crime	353.21	163.20	331.72	67.41	1,035.49	1,970
Violent Crime	135.11	60.67	125.87	30.37	496.09	1,970
Drug Offenses	254.57	121.00	222.24	56.48	911.47	1,970
Street Crime	167.39	76.66	163.89	36.61	605.28	1,970
Turkish Suspects per 100,000 Residents						
Total Crime	74.10	67.99	55.25	0.00	463.93	1,970
Property Crime	8.91	8.86	6.33	0.00	58.41	1,970
Violent Crime	9.11	9.33	6.26	0.00	61.80	1,970
Drug Offenses	8.73	9.39	5.88	0.00	101.59	1,970
Street Crime	7.07	7.51	4.87	0.00	49.88	1,970
Refugees per 100,000 Residents						
Actual Annual Refugee Inflow	337.97	294.88	246.03	15.83	4,024.41	1,970
Predicted Annual Refugee Inflow	478.27	449.68	290.84	0.00	1,942.70	1,970
Annual Difference in Refugee Stock	330.97	421.83	234.43	-3,126.93	6,495.10	1,576
IRF in 2015 [0/1]	0.37	0.48	0.00	0.00	1.00	1,970
Vacant Military Building [0/1]	0.20	0.40	0.00	0.00	1.00	1,970
Control Variables						
Share of Males under Age 35	18.57	1.86	18.67	13.76	25.05	1,970
Unemployment Rate	5.64	2.63	5.20	1.30	15.40	1,970
Log Population	11.98	0.67	11.95	10.43	15.11	1,970

Note: Table shows summary statistics of the main dependent, independent, and control variables for the years 2013 to 2018.

Table A3: First-Stage Regression Results

	Contemporary		Lagged		Lagged		IRF Interaction	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Hypothetical Inflow	0.317***	0.299***						
	(0.087)	(0.088)						
Hypothetical Inflow $(t-1)$			0.370***	0.353***	0.352***	0.336***	-0.071	-0.099
			(0.098)	(0.095)	(0.097)	(0.093)	(0.108)	(0.108)
Hypothetical Inflow $(t-1)$ × Military Vacancy					0.072	0.070	0.180***	0.192***
					(0.048)	(0.053)	(0.060)	(0.063)
Share of males under age 35	248.448**	247.560**	119.007	110.400	116.300	110.849	85.259	75.983
	(98.835)	(109.298)	(85.799)	(94.315)	(83.045)	(92.962)	(91.704)	(99.926)
Unemployment rate	-64.906***	-36.188	-21.338	5.289	-17.427	8.736	-60.120**	-48.294*
	(23.716)	(25.638)	(18.026)	(22.659)	(18.361)	(22.833)	(26.440)	(27.983)
Log population	1.969	27.137	-217.451	-188.863	-211.203	-189.219	-191.161	-195.378
	(199.679)	(226.060)	(171.818)	(191.666)	(166.820)	(188.926)	(182.822)	(203.332)
F-Stat	13.26	11.91	14.20	13.71	25.21	24.78	18.36	23.13
R^2	0.02	0.02	0.02	0.02	0.03	0.03	0.03	0.03
No. Obs.	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970
Year FE	X	X	X	X	X	X	X	X
State FE		X		X		X		X

Note: This table reports the full first-stage regression results for Table 3 in column (1) and (2), for Table 4 and 5 in column (3) and (4), and for Table A10 in columns (5) to (8). All continuous variables are normalized by population from $t-1$. SE clustered by district, *p<.10; **p<.05; ***p<.01.

Table A4: Reduced Form Regression Results on Crime Rates

	Crime		Property		Violence		Drugs		Street	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Hypothetical Refugee Inflow $(t-1)$	0.221	0.253	0.140*	0.154**	0.026***	0.026***	-0.014	-0.013	0.059*	0.069**
	(0.156)	(0.156)	(0.072)	(0.069)	(0.007)	(0.007)	(0.018)	(0.018)	(0.033)	(0.032)
R^2	0.11	0.13	0.17	0.19	0.10	0.11	0.02	0.03	0.06	0.08
No. Obs.	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970
Control Variables	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X
State FE		X		X		X		X		X

Reduced form regression results of crime rates using the IV from Section 3 as explanatory variable. All variables based on population from $t-1$. Control variables are first-differenced log population, unemployment rate, and share of males under age 35. SE clustered by district, *p¡.10; **p¡.05; ***p¡.01

Table A5: Lagged Effects on Crime Rates ($t-2$)

	Crime		Property		Violence		Drugs		Street	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
OLS										
Refugee Inflow ($t-2$)	-0.009	0.023	-0.039	-0.021	0.000	0.002	0.003	0.006	0.001	0.018
	(0.062)	(0.052)	(0.035)	(0.030)	(0.003)	(0.003)	(0.013)	(0.013)	(0.032)	(0.027)
R^2	0.06	0.09	0.13	0.16	0.08	0.09	0.02	0.03	0.03	0.06
IV										
Refugee Inflow ($t-2$)	-0.355	-0.259	-0.155	-0.093	-0.027	-0.026	0.067	0.072	-0.078	-0.039
	(0.360)	(0.367)	(0.205)	(0.205)	(0.025)	(0.026)	(0.059)	(0.064)	(0.103)	(0.099)
First Stage Estimate	0.344	0.335	0.344	0.335	0.344	0.335	0.344	0.335	0.344	0.335
First Stage SE	0.094	0.093	0.094	0.093	0.094	0.093	0.094	0.093	0.094	0.093
First Stage F-Stat	13.47	12.90	13.47	12.90	13.47	12.90	13.47	12.90	13.47	12.90
No. Obs.	1,576	1,576	1,576	1,576	1,576	1,576	1,576	1,576	1,576	1,576
Control Variables	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X
State FE		X		X		X		X		X

Note: 2SLS of first-differenced crime rates on annual refugee inflows. All main variables normalized by population from $t-1$. Refugee inflow includes only those who immigrated within the past 12 months. Control variables are the first-differenced log population, unemployment rate, and share of males below age 35. SE clustered by district, *p<.10; **p<.05; ***p<.01.

A.3 Tables on Robustness Checks

Table A6: Two-Way Fixed Effects IV Approach

	Crime (1)	Property (2)	Violence (3)	Drugs (4)	Street (5)
Refugee Inflow (t-1)	0.515**	0.248*	0.050**	-0.071	0.036
	(0.257)	(0.134)	(0.019)	(0.044)	(0.071)
First Stage Estimate	0.437	0.437	0.437	0.437	0.437
First Stage SE	0.085	0.085	0.085	0.085	0.085
First Stage F-Stat	26.35	26.35	26.35	26.35	26.35
No. Obs.	1,970	1,970	1,970	1,970	1,970
Control Variables	X	X	X	X	X
Year FE	X	X	X	X	X
District FE	X	X	X	X	X

Note: 2SLS of crime rates on annual refugee inflows with two-way fixed effects. All main variables are normalized by population from $t-1$. Refugee inflow includes only those who immigrated within the past 12 months. Control variables are the log population, unemployment rate, and share of males below age 35. SE clustered by district, *p<.10; **p<.05; ***p<.01.

Table A7: Alternative Instrumental Variable

	Crime		Property		Violence		Drugs		Street	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Refugee Inflow $(t-1)$	0.123	0.572*	-0.024	0.192	0.018	0.032**	-0.037	-0.001	-0.095	0.036
	(0.261)	(0.295)	(0.125)	(0.139)	(0.011)	(0.013)	(0.034)	(0.038)	(0.069)	(0.074)
First Stage Estimate	0.211	0.312	0.211	0.312	0.211	0.312	0.211	0.312	0.211	0.312
First Stage SE	0.114	0.130	0.114	0.130	0.114	0.130	0.114	0.130	0.114	0.130
First Stage F-Stat	52.62	46.75	52.62	46.75	52.62	46.75	52.62	46.75	52.62	46.75
First Stage R-squ.	0.07	0.05	0.07	0.05	0.07	0.05	0.07	0.05	0.07	0.05
No. Obs.	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970
Control Variables	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X
State FE		X		X		X		X		X

Note: 2SLS of first-differenced crime rates on annual refugee inflows. The alternative instrumental variable is based on AZR inflows as opposed to EASY registrations; see Section 6. All variables based on population from $t-1$. Control variables are first-differenced log population, unemployment rate, and share of males below age 35. SE clustered by district, *p<.10; **p<.05; ***p<.01.

Table A8: Alternative Explanatory Variable: Difference in Stocks of Refugees (IV)

	Crime		Property		Violence		Drugs		Street	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
OLS										
Refugee Inflow ($t-1$)	0.047	0.065	0.009	0.020	0.003	0.004	0.021*	0.023*	-0.019	-0.012
	(0.079)	(0.073)	(0.050)	(0.045)	(0.003)	(0.003)	(0.011)	(0.012)	(0.023)	(0.020)
R^2	0.11	0.14	0.15	0.18	0.10	0.11	0.03	0.04	0.06	0.08
IV										
Δ Refugees ($t-1$)	0.507*	0.634**	0.327**	0.403***	0.056***	0.060***	-0.027	-0.025	0.132*	0.177**
	(0.278)	(0.277)	(0.141)	(0.144)	(0.014)	(0.016)	(0.043)	(0.045)	(0.068)	(0.070)
First Stage Estimate	0.448	0.426	0.448	0.426	0.448	0.426	0.448	0.426	0.448	0.426
First Stage SE	0.140	0.141	0.140	0.141	0.140	0.141	0.140	0.141	0.140	0.141
First Stage F-Stat	10.17	9.07	10.17	9.07	10.17	9.07	10.17	9.07	10.17	9.07
No. Obs.	1,576	1,576	1,576	1,576	1,576	1,576	1,576	1,576	1,576	1,576
Control Variables	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X
State FE		X		X		X		X		X

Note: 2SLS of first-differenced crime rates on first-differenced refugee stocks. All variables based on population from $t-1$. Difference in refugee stocks only available between 2014 to 2017. Control variables are first-differenced log population, unemployment rate, and share of males below age 35. SE clustered by district, *p<.10; **p<.05; ***p<.01.

Table A9: Additional Covariates (IV)

	Crime		Property		Violence		Drugs		Street	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Spatially Lagged Crime Rates										
Refugee Inflow ($t-1$)	0.478	0.585	0.284	0.326*	0.069***	0.073***	-0.032	-0.034	0.140	0.174**
	(0.384)	(0.404)	(0.186)	(0.183)	(0.019)	(0.020)	(0.052)	(0.051)	(0.088)	(0.085)
Spatially Lagged Crime Rates	X	X	X	X	X	X	X	X	X	X
First Stage Estimate	0.370	0.353	0.370	0.353	0.370	0.353	0.370	0.353	0.370	0.353
First Stage SE	0.098	0.095	0.098	0.095	0.098	0.095	0.098	0.095	0.098	0.095
First Stage F-Stat	14.20	13.66	14.24	13.83	14.16	13.67	13.87	13.65	14.32	13.88
Crime Clearance Rates										
Refugee Inflow ($t-1$)	0.610	0.722*	0.377**	0.437**	0.070***	0.073***	-0.044	-0.045	0.160*	0.194**
	(0.380)	(0.394)	(0.191)	(0.192)	(0.020)	(0.020)	(0.051)	(0.051)	(0.089)	(0.086)
Crime Clearance Rates	X	X	X	X	X	X	X	X	X	X
First Stage Estimate	0.370	0.353	0.370	0.353	0.370	0.353	0.370	0.353	0.370	0.353
First Stage SE	0.098	0.095	0.098	0.095	0.098	0.095	0.098	0.095	0.098	0.095
First Stage F-Stat	14.26	13.75	14.15	13.67	14.11	13.62	14.20	13.69	14.18	13.70
No. Obs.	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970
Control Variables	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X
State FE		X		X		X		X		X

Note: 2SLS of first-differenced crime rates on annual refugee inflows. All main variables normalized by population from $t-1$. Refugee inflow includes only those who immigrated within past 12 months. Control variables are first-differenced log population, unemployment rate, and share of males below age 35. Top panel also includes a variable capturing spatially lagged crime rates of the respective crime category of the outcome variable. Bottom panel also includes crime clearance rates of the respective crime category of the outcome variable. SE clustered by district, *p<.10; **p<.05; ***p<.01.

Table A10: Heterogenous Lagged Effects on Crime Rates by Refugee Reception Facility (IRF)

	Crime		Property		Violence		Drugs		Street	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
OLS										
Refugee Inflow $(t-1)$	0.070	0.087	0.006	0.016	0.005	0.006	0.013	0.014	-0.027	-0.015
	(0.091)	(0.092)	(0.045)	(0.045)	(0.004)	(0.004)	(0.011)	(0.012)	(0.032)	(0.031)
Refugee Inflow $(t-1) \times \text{IRF}_{2015}$	0.014	0.019	0.013	0.014	-0.001	-0.001	0.002	0.003	0.020	0.017
	(0.059)	(0.063)	(0.034)	(0.035)	(0.003)	(0.003)	(0.009)	(0.009)	(0.019)	(0.020)
R^2	0.11	0.13	0.17	0.19	0.09	0.11	0.02	0.03	0.06	0.08
IV										
Refugee Inflow $(t-1)$	0.577	0.703*	0.377**	0.452**	0.068***	0.071***	-0.038	-0.035	0.157*	0.199**
	(0.372)	(0.379)	(0.185)	(0.201)	(0.017)	(0.017)	(0.049)	(0.047)	(0.085)	(0.086)
Refugee Inflow $(t-1) \times \text{IRF}_{2015}$	-0.270	-0.089	-0.012	0.100	-0.019	-0.015	0.006	0.024	-0.027	0.020
	(0.246)	(0.237)	(0.163)	(0.179)	(0.016)	(0.016)	(0.040)	(0.035)	(0.074)	(0.077)
SW F-Stat Inflow	25.21	24.78	25.21	24.78	25.21	24.78	25.21	24.78	25.21	24.78
SW F-Stat Interaction	18.36	23.13	18.36	23.13	18.36	23.13	18.36	23.13	18.36	23.13
No. Obs.	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970
Control Variables	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X
State FE		X		X		X		X		X

Note: 2SLS of first-differenced crime rates on annual refugee inflows and interacted by presence of an IRF. Refugee inflow includes only those who immigrated within the past 12 months. IRFs are captured in an indicator variable which is equal to one for districts with IRF and zero otherwise. Instruments are applied for the IV approach according to Section **??**. Control variables are the first-differenced log population, unemployment rate, and share of males below age 35. SE clustered by district, *p<.10; **p<.05; ***p<.01.

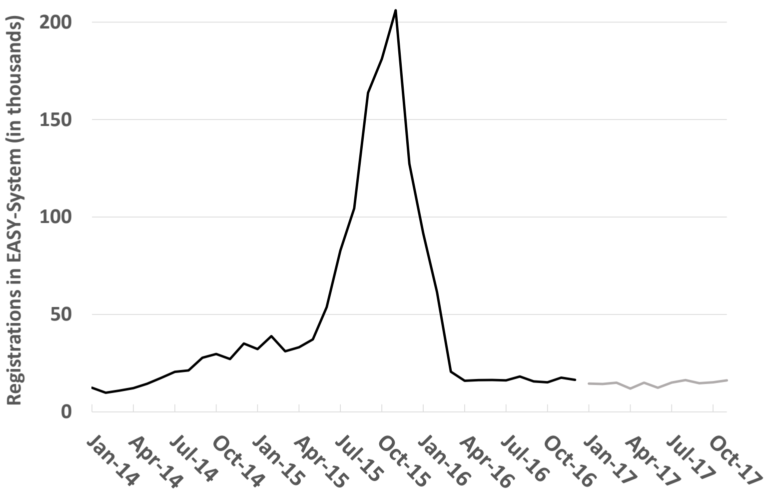
Table A11: Turkish Suspect Rates (IV)

	Crime		Property		Violence		Drugs		Street	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Refugee Inflow ($t-1$)	0.014	0.015*	0.003	0.004**	0.000	0.000	0.002	0.003	0.002	0.002
	(0.009)	(0.009)	(0.002)	(0.002)	(0.001)	(0.001)	(0.003)	(0.003)	(0.002)	(0.002)
First Stage Estimate	0.370	0.353	0.370	0.353	0.370	0.353	0.370	0.353	0.370	0.353
First Stage SE	0.098	0.095	0.098	0.095	0.098	0.095	0.098	0.095	0.098	0.095
First Stage F-Stat	14.20	13.71	14.20	13.71	14.20	13.71	14.20	13.71	14.20	13.71
No. Obs.	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970	1,970
Control Variables	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X
State FE		X		X		X		X		X

Note: 2SLS of first-differenced Turkish suspect rates on annual refugee inflows. All variables normalized by population from $t-1$. Control variables are first-differenced log population, unemployment rate, and share of males below age 35. SE clustered by district, *p<.10; **p<.05; ***p<.01.

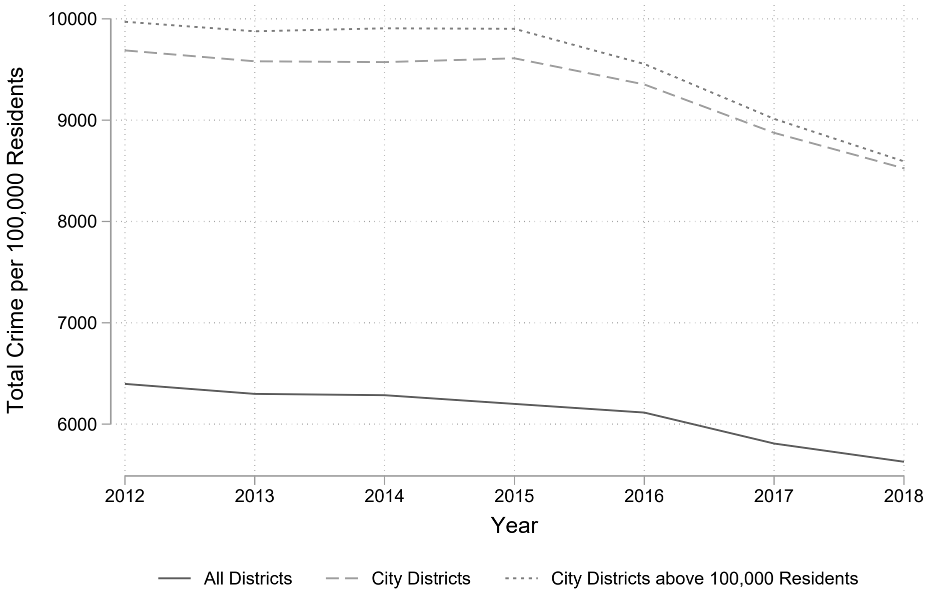
A.4 Figures

Figure A1: Number of Individuals Seeking Protection in Germany



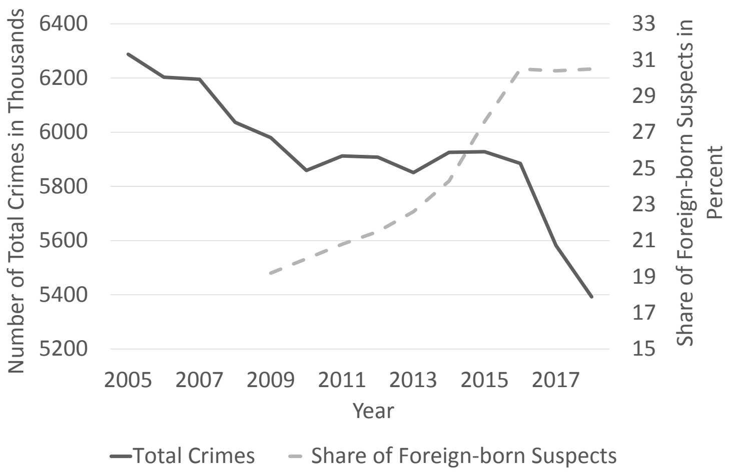
Note: The figure shows data on registrations in the EASY system retrieved from the Federal Ministry of Internal Affairs (Bundesministerium des Innern, für Bau und Heimat). From 2017 onwards there is a structural time series break due to changes in the registration procedure. Authors' depiction.

Figure A2: Average Total Crime Rates by District Size



Note: The figure presents the total crime rate by district size excluding offenses against asylum laws. Total crime is normalized by population size in $t-1$ and presented per 100,000 residents.

Figure A3: Total Crime and Share of Foreign-born Suspects



Note: The graph shows the absolute number of crimes excluding offenses against asylum laws and the share of foreign-born suspects. The data stem from the administrative police records of the Federal Police Office. Authors' depiction.

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