Do immigrants affect crime? Evidence from panel data for Germany

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Abstract

The paper analyses the empirical relationship between immigrants and crime using panel data for 391 German administrative districts between 2003 and 2016. Employing different standard panel estimation methods, we show that there is no positive association between the immigrant rate and the crime rate. We assess the robustness of this result by considering the heterogeneity of immigrant groups with respect to gender, age, country of origin and – if applicable – refugee status, and study naturalized immigrants. We also take into account possible spillover effects of immigrants on criminal activities by Germans, omitted variables and spatial correlation. Furthermore, taking advantage of the panel-structure of the data set we employ an instrumental variable approach that deals with the possibly endogenous allocation of immigrants and allows for causal interpretation of the estimates. There is no evidence that immigrants increase crime.

JEL-Classification: F22, J15, K42, R10

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1 Introduction

In destination countries, immigration is often a controversial issue for different reasons. Natives are concerned about the threats which immigrants might pose for economic prosperity or cultural identity. Furthermore, the perception that immigrants are more likely to engage in criminal activity is shaping the attitudes towards immigrants and consequently immigration policy (see, e.g., Fitzgerald et al., 2012; Nunziata, 2015). For example, in Germany, according to SVR (2018), about 26% of the surveyed individuals without migration background agree with the statement that immigrants increase the crime rate and about 48% do so when the crime rate is linked to refugees. According to Köcher (2016), 80% believe that refugees significantly or somewhat increase crime in Germany.

From a theoretical point of view, the relation between immigrants and crime is not clear. On the one hand, the economic theory of crime suggests that immigrants are more likely to participate in criminal activities because they have lower opportunity costs given their poorer labour market prospects. On the other hand, if immigrants commit a crime they might face deportation. This increases the cost of punishment and may have an important deterrence effect.

The empirical results found so far vary across countries, crime categories and immigrant groups. For example, Butcher and Piehl (1998) find no significant relationship between immigrants and crime for the Unites States (U.S.) at the city-level while Spenkuch (2014) finds a positive effect of immigrants on property crime but not on violent crime in U.S. counties. Freedman et al. (2018) using data for Bexar County in the U.S. find an increase in the felony charges against immigrants whose employment opportunities were worsened by the Immigration Reform and Control Act of 1986 and point at the importance of legal earning opportunities for the criminal behaviour of immigrants. For European countries, Nunziata (2015) shows that immigrants have no effect on crime using individual-level data from the European Social Survey. Alonso-Borrego et al. (2012) find similar results when considering the effect on total crime and on crime subcategories using annual data on reported offences and convictions at the province level in Spain. In Bianchi et al. (2012), on the other hand, there is a positive effect of immigrants in Italy, but only for robbery.

Distinguishing between immigrant groups, Chalfin (2014) finds no significant relationship between Mexican immigrants and different crime categories in U.S. metropolitan areas. Bell et al. (2013), using data for the United Kingdom, show that asylum seekers increase property crimes but do not affect the rates for violent crime, while economic immigrants have no effect on either crime category. Piopiunik and Ruhose (2017) find that the immigration of ethnic Germans after the collapse of the Soviet Union increased the total crime rate in Germany. Overall, the existing empirical studies on the relationship between immigrants and crime mostly find no significant effect or a significant effect of immigrants only on property crimes.

This paper examines the relationship between immigrants and crime in a comprehensive way using data on administrative districts in Germany for the period 2003-2016. The panel-structure of the data allows addressing the problem of unknown heterogeneity, spatial correlation and endogeneity of immigrants' location decision. Employing different specifications, we conclude that there is no significant positive association between immigrants and the total crime rate. This also holds for crime subcategories.

Robustness tests confirm these results. Looking at immigrant groups differentiated by gender, age, country of origin or refugee status, we do not find a significantly positive association between most immigrant groups and subcategories of crime. This also holds for naturalized immigrants. Our results suggest that their higher incentive to commit crimes compared to non-naturalized immigrants due to the absence of the risk of deportation and their lower incentive due to the positive selection into naturalization balance one another. In addition, we consider possible spillover effects, that is whether an increase in the number of immigrants leads to an increase in crimes committed by natives. Also, we address two possible sources of bias: omission of important explanatory variables and spatial correlation. None of these extensions change the basic results.

Lastly, we address the issue of endogeneity of immigrants' choice of location by employing an instrumental variable approach based on different specifications. This allows for a causal interpretation of the findings. Depending on the construction of the instrument, we find a moderate positive effect of immigrants on drug offences and damage to property. There is no evidence that immigrants increase the total crime rate.

The paper contributes to the literature in several ways: First, we provide a comprehensive picture of the relation between immigrants and crime by studying the total immigrant population as well as subgroups including refugees and naturalized immigrants. Second, we consider the direct effect of immigrants on crime and the indirect effect which could work via an increase in criminal activities by natives in the presence of a larger number of immigrants. Third, the panel-structure enables us to address the issues of unknown heterogeneity, spatial correlation and endogeneity.

The paper is structured as follows: Section 2 introduces the theoretical framework. Section 3 presents the data. In Section 4, the empirical specifications are introduced. Section 5 presents the basic results of the relationship between immigrants and crime using panel estimation methods and addresses the robustness of the results. In Section 6, an instrumental-variable strategy is employed. Lastly, Section 7 concludes.

2 Economic Model of Crime

Starting with the seminal paper by Becker (1968), the economics of crime explains criminal behaviour in the market setting where a cost-benefit analysis is used to study the decision to participate in criminal activities. A more comprehensive model is proposed by Ehrlich (1973) who models the decision to engage in crime as a time allocation problem of an individual making an occupational choice under uncertainty. The central element of the model is – similar to Becker (1968) – the concept of opportunity costs of criminal activities that stem from the foregone legal income and add to the cost of punishment if caught.

In a one-period model, an individual can choose to spend time on non-market activities and two market activities, a legal one, l, and a criminal one, c. The income from each activity is a monotonically increasing function of time t devoted to it. The income from legal work is certain and is denoted by $W_l(t_l)$. The criminal activity is risky and the income is state contingent. There are two possible states of the world, $s = a, b$: State a, where the offender is not detected and receives the net income from criminal activity, $W_c(t_c)$, and state b, where the offender is caught and the net income from criminal activity is reduced by the punishment for the crime committed, $F(t_c)$. It is assumed that the probability of apprehension, p, is independent of the amount of time spent in either activity.

The indirect utility function of an individual in any given state of the world, s, is defined as

$$U_s = U(X_s, t_{nm}), \tag{1}$$

where X_s is the state contingent stock of the composite market good and t_{nm} is time devoted to non-market activity, e.g., consumption or schooling. Denoting all incomes in terms of the composite good X, with probability $(1 - p)$ a crime is undetected and X_a is obtained or with probability p a crime is detected and the individual gets X_b. Formally,

$$X_a = W + W_c(t_c) + \pi W_l(t_l), \tag{2}$$
$$X_b = W + W_c(t_c) - \phi F(t_c) + \pi W_l(t_l), \tag{3}$$

where W is the market value of the individual assets at the beginning of the period; ϕ and π, with $\phi, \pi > 0$, allow for differences between immigrants' and natives' legal earning opportunities and severity of punishment, respectively. Given these wealth constraints, the individual maximizes the expected utility of a one-period consumption prospect

$$\max_{t_c, t_l, t_{nm}} \quad EU(X_s, t_{nm}) = (1-p)U(X_a, t_{nm}) + pU(X_b, t_{nm}) \tag{4}$$
$$s.t. \quad t_0 = t_c + t_l + t_{nm} \tag{5}$$
$$t_c \geq 0, \qquad t_l \geq 0, \qquad t_{nm} \geq 0, \tag{6}$$

where Equation (5) is a time constraint with t_0 being total time available to the individual and Equation (6) presents nonnegativity requirements. Substituting Equations (2) and (3) in Equation (4), the Kuhn-Tucker first-order optimality conditions are

$$\frac{\partial EU}{\partial t} - \lambda \leq 0, \tag{7}$$
$$\left(\frac{\partial EU}{\partial t} - \lambda\right) t = 0, \tag{8}$$
$$t \geq 0, \tag{9}$$

where t denotes the optimal values of t_c, t_l and t_{nm}, respectively, and λ is the marginal utility of time spent for non-market activities. Given the amount of time allocated to non-market activities, t_{nm}, the optimal allocation of the working time between legal and criminal activities, in case of an interior solution, is given by the first-order condition

$$(1-p)U'(X_a)\frac{\partial W_c}{\partial t_c} + pU'(X_b)\frac{\partial W_c}{\partial t_c} = \pi\left[(1-p)U'(X_a)\frac{\partial W_l}{\partial t_l} + pU'(X_b)\frac{\partial W_l}{\partial t_l}\right] + \phi pU'(X_b)\frac{\partial F}{\partial t_c}. \tag{10}$$

According to the first-order condition, the expected marginal benefit from additional time allocated to crime is equal to its expected marginal cost.[1] The left-hand side of Equation (10) is the expected marginal benefit of allocating one more unit of time to criminal activities and it is equal to the sum of the expected marginal increase in criminal income in the two possible states of the world, each weighted with the marginal utility of income in the respective state. The right-hand side of Equation (10) is the expected marginal cost associated with a marginal increase in time allocated to criminal activities. In other words, it represents the opportunity cost of crime. It arises, firstly, because the amount of time allocated to legal activities decreases and the income is lost that could have been earned otherwise. This loss occurs irrespective of the state of the world, i.e. whether or not the individual is apprehended. Secondly, the marginal cost of punishment has to be considered as well. However, the individual faces the punishment only if he or she is apprehended, that is, only in the

[1]The second-order condition for a strict local maximum is

$$\frac{\partial^2 EU}{\partial t_c^2} = (1-p)U''(X_a)\left(\frac{\partial W_c}{\partial t_c} - \pi\frac{\partial W_l}{\partial t_l}\right)^2 + (1-p)U'(X_a)\left(\frac{\partial^2 W_c}{\partial t_c^2} - \pi\frac{\partial^2 W_l}{\partial t_l^2}\right)$$
$$+ pU''(X_b)\left(\frac{\partial W_c}{\partial t_c} - \phi\frac{\partial F}{\partial t_c} - \pi\frac{\partial W_l}{\partial t_l}\right)^2 + pU'(X_a)\left(\frac{\partial^2 W_c}{\partial t_c^2} - \phi\frac{\partial^2 F}{\partial t_c^2} - \pi\frac{\partial^2 W_l}{\partial t_l^2}\right) < 0$$

state of the world b.

Equation (10) identifies the variables which affect an individual's decision to participate in criminal activities.[2] It can be easily shown that the effect of the probability of apprehension, p, on the amount of time allocated to criminal activities is negative. An increase in p means an increase in the probability of the state of the world when the offender is caught and the income from market activities, both criminal and legal, is decreased by the amount of the punishment. Therefore, all else equal, the increase in p decreases the expected marginal benefit and increases the expected marginal cost. Consequently, the incentive to commit a crime decreases and so does the number of offences committed. Similarly, the effect of punishment, F, on the participation in criminal activities, all else equal, is negative. The intuition is straightforward. The more severe the punishment, the larger the cost of crime in case of apprehension. As a result, the incentive to engage in criminal activities and the number of offences decrease.

In contrast, an increase in the criminal income, W_c, increases the amount of time allocated to criminal activities. By increasing the benefits from crime, it increases the opportunity cost of legal work. Lastly, an increase in the income from legal work, W_l, increases the opportunity cost of crime and, therefore, the amount of time allocated to criminal activities, all else equal, decreases.

The economic model of crime implies that if immigrants and natives face different legal income options and sanctions for criminal behaviour, they will have a different propensity to commit a crime. It has been documented that immigrants have poorer labour market opportunities than natives. For example, Algan et al. (2010) find that there are significant immigrant-native wage and employment gaps that persist over generations. Therefore, the opportunity cost of criminal activities is smaller for immigrants than for natives.[3] At the same time, the cost of committing a crime is higher for immigrants because they face the risk of deportation. According to the Federal Ministry of Justice and Consumer Protection (2018), a foreigner who has been expelled or deported is banned to re-enter Germany or reside there. The length of the ban depends on the grounds of deportation and can vary between five and ten years. For refugees, having committed an offence decreases the chance of receiving asylum and can be a reason for rejection of their application (Federal Ministry of Justice and Consumer Protection, 2017).

Setting $\pi = \phi = 1$ in (10) for natives, poorer earning opportunities and more severe punishment for immigrants translate into $\phi > 1$ and $\pi < 1$, respectively. Overall, the effect predicted by theory is ambiguous. Lower outside options of immigrants suggest that, on average, they have a larger incentive to commit crimes, while, at the same time, the higher cost of punishment likely has a deterrent effect on prospective offenders.

3 Data

The data are collected for 391 German administrative districts (NUTS-3) for the years 2003-2016.[4] There were changes in the administrative division in 2007 in Saxony-Anhalt, in 2008 in Saxony, in 2011 in Mecklenburg-Vorpommern and in 2016 in Niedersachsen. To keep the time series consistent, the pre-2016 districts have been merged so that they correspond to the post-2016 administrative

[2]The conclusions of the economic model of crime primarily apply to property crimes and less to violent crimes or crimes of passion given that pecuniary benefits are less important for these two types of crime.

[3]However, Adsera and Chiswick (2007) conclude that the wage gap is lowest in Germany compared to other destination countries in Western Europe.

[4]The large inflow of refugees in 2015 and 2016 could potentially bias our results. For this reason, we rerun all estimations (see Sections 5 and 6) without these two years. The results do not change qualitatively and are available from the authors upon request.

Table 1: Descriptive statistics

Variable	Obs.	Mean	Std. Dev.	Min	Max
Crime Rates (crime / population × 100000)					
Total Crime	5,474	6,699.00	3,200.92	385.43	103,413.10
Burglary	5,474	121.19	96.57	2.92	791.96
Damage to Property	5,474	797.63	339.70	56.58	4,203.24
Drug Offences	5,474	306.23	176.66	21.22	2,394.19
Street Crime	5,474	1,481.35	833.03	80.45	5,814.68
Clear-up Rates (solved crime cases / all crime cases × 100)					
Total Crime	5,474	59.77	7.51	34.80	96.80
Burglary	5,474	23.06	12.81	0.00	203.80
Damage to Property	5,474	27.23	6.41	8.60	90.80
Drug Offences	5,474	95.62	3.50	31.90	109.00
Street Crime	5,474	20.99	5.90	4.30	63.90
Immigrant Rates (immigrants / population × 100000)					
Immigrants	5,474	7,353.24	5,043.34	589.98	39,742.67
Immigrants male	5,474	3,833.19	2,631.12	349.47	21,285.99
Immigrants female	5,474	3,520.05	2,425.30	240.51	18,460.70
Immigrants age under_14	5,474	873.79	581.08	49.76	4,643.14
Immigrants age 15_19	5,474	457.39	282.67	22.36	1,731.82
Immigrants age 20_24	5,474	628.31	415.03	36.38	2,812.76
Immigrants age 25_29	5,474	771.80	525.03	64.30	3,599.03
Immigrants age 30_34	5,474	818.98	523.95	78.27	3,602.89
Immigrants age 35_39	5,474	801.15	489.35	57.14	3,355.03
Immigrants age over_40	5,474	3,184.55	2,046.06	283.46	15,569.59
Immigrants from Africa	5,474	272.92	311.57	10.45	2,493.19
Immigrants from America	5,474	219.88	193.22	5.93	1,527.97
Immigrants from Asia	5,474	972.24	759.76	152.91	10,962.75
Immigrants from Australia/Oceania	5,474	10.38	11.80	0.00	133.58
Immigrants from Europe	5,474	5,821.97	4,178.78	326.88	33,650.64
Immigrants from top asylum countries	5,474	1,704.22	1,667.44	19.72	10,781.26
Persons seeking protection	3,910	730.17	603.01	28.11	13,001.67
Naturalized immigrants	2,346	105.28	78.78	2.02	609.77
District Characteristics					
Population	5,474	204,879.70	233,318.60	33,944	3,574,830
Male15_39 (%)	5,474	15.13	1.57	11.08	21.56
Germans male 15_39 (%)	5,474	13.68	3.03	6.79	35.60
GDP	5,474	29,898.44	13,485.54	11,397.84	178,804.40
Unemployment (%)	5,474	7.87	4.15	1.20	25.40
Politics	5,474	0.65	0.43	0	1
West	5,474	0.81	0.39	0	1
City	5,474	0.27	0.44	0	1
Suspect Rates – Foreigners (foreign suspects / population × 100000)					
Total Crime	1,564	955.62	2,031.73	82.96	42,485.64
Burglary	1,564	8.27	7.33	0.00	40.75
Damage to Property	1,564	22.43	13.68	0.00	74.96
Drug Offences	1,564	50.64	52.06	0.00	441.36
Street Crime	1,564	52.23	38.61	1.41	238.98
Suspect Rates – Germans (German suspects / population × 100000)					
Total Crime	1,564	1,999.58	656.83	842.59	4,678.82
Burglary	1,564	14.23	10.87	0.00	72.32
Damage to Property	1,564	146.44	63.19	41.12	442.25
Drug Offences	1,564	238.72	114.09	59.60	904.85
Street Crime	1,564	176.88	82.00	36.70	599.13

division. Furthermore, districts of Saarland had to be dropped because there is only one foreigners' authority that is responsible for all districts in the state and the number of immigrants is not available at the district level.

3.1 Dependent Variable

The dependent variable is the logarithm of the crime rate. It is defined as the number of offences reported to the police per 100,000 district inhabitants.[5] All measures of criminal activity are based on administrative data published in the Police Crime Statistics (PCS) of the Federal Criminal Police Office (2017). The PCS includes information on total offences, which is the total number of offences reported to the police, and on the following four crime subcategories among others: burglary, damage to property, drug offences and street crime.[6] Total crime refers to all types of crime reported to the police. Burglary comprises thefts committed by housebreaking. Damage to property includes crimes that involve destroying, damaging or making useless another individual's property. Drug offences refer to crimes associated with general violations of the narcotics law, drugs trafficking and illegal importation of drugs. The offences to be classified as street crime are exclusively or mainly committed on public roads or in public places, including public transport.

The crimes reported to the police very likely underestimate the true number of committed offences. This could lead to biased estimates if the reasons for underreporting are correlated with the determinants of crimes. If underreporting of crimes differs systematically across districts and reporting errors are not random, the reported crime rate of offences would be a poor approximation of the true crime rate. This problem is dealt with by taking logarithms of the crime rate and including district- and time-fixed effects.

Another issue is that a larger number of immigrants might lead to more reporting of crimes or, via an increased police presence, more detection of crimes. Subsequently, the presence of immigrants might be associated with an increase in crimes even though the true number of committed crimes has not changed. Regression estimates can thus be upward biased and we will come back to this issue later.

3.2 Independent Variable

We use the logarithm of the immigrant rate as independent variable. It is defined as the stock of immigrants per 100,000 district inhabitants. The data on immigrants come from the Statistics of Foreigners that is based on the Central Register of Foreigners and published by the Federal Statistical Office (2018). All persons who do not have German citizenship are registered as immigrants. For robustness checks, immigrant rates by gender, age groups and nationality are used.[7]

[5]We cannot differentiate between crimes committed by foreigners or Germans. Who committed a crime is only known after the crime has been solved and the suspects have been recorded, but not at the time when the crime is reported. Data is available for the number of suspects holding German and foreign nationality for a limited number of years only. We analyse the effect of immigrants on the number of suspects in Section 5.2.4.

[6]There are also data on the subcategory vehicle theft. As this is a subcategory of street crime, it is however not part of the analysis. For the other subcategories, data are not available online.

[7]The numbers of immigrants by age groups do not add up to the total number of immigrants because the data on the total number of immigrants and the number of immigrants by gender and nationality published by the Federal Statistical Office (2018) are based on the Central Register of Foreigners while the data on the number of immigrants by age are population estimates published by the Federal Statistical Office and the Statistical Offices of the Länder (2018). The estimated data are rolled forward from the population census of 1987 in the former Federal Republic of Germany, the population register of 1990 in the former German Democratic Republic, and the census of 2011. Before 2011, the estimated data exceeded the registered data by about 7% with the only exception being 2003, where the difference is about 0.1%. After 2011, the estimated data falls short of the register data by about 8% but the difference is smaller in 2015 with about 5%. In the basic regressions, we will use register data; using data based on population estimates instead of register data does not change the conclusions.

A measurement error might arise due to the presence of illegal immigrants. Unfortunately, there is no data available for the number of illegal immigrants residing in Germany. To address this issue, also here, we take the logarithm of the immigrant rate and include district- and time-fixed effects.

3.3 Control Variables

The model discussed in Section 2 and the empirical literature on crime guide our choice of variables that may explain the crime rate. Therefore, we include the percentage of the cases solved among all cases that came to police notice during a given period, *clear-up rate*,[8] to proxy the probability of apprehension, and the logarithm of GDP per capita, *GDP*, and the unemployment rate, *unemployment*, to capture legal income opportunities.

In addition, the following demographic variables are included in most specifications: the logarithm of the population of the district, *population*, and the share of men aged 15-39, $male15\_39$. The reason for the inclusion of these demographic variables is the finding that young men commit crimes at an over-proportional rate and crime tends to be concentrated in densely populated areas (see, e.g., Butcher and Piehl, 1998; Freeman, 1999).

The data on demographic control variables are taken from the current updating of the Federal Statistical Office (2018). The source of the data on economic variables is the Federal Statistical Office and the Statistical Offices of the Länder (2018). Summary statistics of all variables are given in Table 1.

4 Model and Method

Based on the theoretical considerations in Section 2 and following Bianchi et al. (2012), the model to be estimated takes the following form

$$crime_{it} = \beta\ immigrants_{it} + \gamma X_{it} + \varphi_i + \varphi_t + \varepsilon_{it} \qquad \text{with } i = 1...391,\ t = 2003...2016, \tag{11}$$

where $crime_{it}$ is the logarithm of the crime rate in district i and year t; $immigrants_{it}$ is the logarithm of the immigrant rate in district i and year t; X_{it} is the vector of time-varying district-specific demographic and socioeconomic control variables; φ_i and φ_t are, respectively, district- and time-fixed effects and ε_{it} is the idiosyncratic error term. The coefficient of interest is β that represents the elasticity of the crime rate with respect to the immigrant rate.

A variety of panel methods can be applied to estimate Equation (11). As a benchmark we employ pooled ordinary least squares (OLS) but due to unobserved heterogeneity the estimate of β is expected to be inconsistent. If there are unobserved effects that are independent of the explanatory variables, then OLS is consistent but inefficient. If, furthermore, the unobserved effects are correlated with the regressors, then pooled OLS suffers from omitted variable bias.

An alternative to OLS is to use a random-effects (RE) specification. This specification assumes that the unobserved components are random variables independent of the regressors and deals with serial correlation in the composite error by employing, e.g., a generalized least squares estimator. The consistency of a random-effects specification depends on the assumption of independence of the unobserved effects, which can be evaluated by a Hausman specification test.

The estimation methods that allow for unobserved effects to be correlated with the explanatory variables are fixed-effects (FE) and first-differencing (FD) specifications. Both methods remove unob-

[8] A *clear-up rate* exceeding 100 can result if cases from previous years are solved in a later year.

served effects by transforming the data. The two-way fixed effects model removes omitted variables that are fixed across districts via year fixed effects and omitted variables that are fixed over time via district fixed effects, by demeaning the data. This way, the estimated effects capture within-district variation over time.

The first-differencing approach removes individual effects by subtracting one-year lagged values from each variable. Time-fixed effects still remain, which can be captured by including time dummies. Because the independent variable, the immigrant rate, is measured in stocks, by first-differencing Equation (11) we study the effect of changes in the immigrant rate on changes in the crime rate.

Lastly, we will also consider an autoregressive model to check if the contemporaneous crime rate can be explained by its past realizations. For this, lagged crime rates are included in Equation (11). Because of the presence of the endogenous explanatory variable, non of the above discussed methods are consistent. The method that allows for endogeneity is the generalized method of moments (GMM, see Blundell and Bond, 1998; Arellano and Bover, 1995). This estimator first-differences Equation (11) and then uses lagged values as well as lagged differences of each regressor as instruments assuming that the differenced error term is uncorrelated with the past realizations of the endogenous variables used as instruments.

5 Results

5.1 Basic Specification

Table 2 reports the results of the basic specification. In the estimations without fixed effects, dummies for West Germany, *west*, and for districts which in fact are cities (kreisfreie Stadt), *city*, are included. The first column shows OLS estimates. The coefficient of the immigrant rate is positive and significant. However, the Breusch-Pagan Lagrange multiplier test rejects the hypothesis of no significant time and individual effects (p-value 0.000). Therefore, pooled OLS is inefficient. Furthermore, it is reasonable to assume that there are unobserved omitted variables that affect both the crime rate and the immigrant rate. For example, some districts might have historically a higher crime rate and a higher immigrant rate compared to others. In addition, pooled OLS ignores the panel structure of the data and uses less information than available to calculate the coefficients. All these arguments point at the limitations of pooled OLS.

The random-effects estimate of the coefficient of the immigrant rate is positive and significant (Column 2). The Hausman specification test, which compares the random-effects model with the fixed-effects model, however, finds that the former is inconsistent (p-value 0.000).

Columns 3 and 4 present fixed-effects and first-difference estimates, respectively. Both suggest that there is no significant association between the immigrant rate and the total crime rate. The test suggested by Wooldridge (2002, 10.6.3) finds that there is a serial correlation in both fixed-effect errors (p-value 0.022) and first-differenced errors (p-value 0.000). The issue of serial correlation is addressed here by clustering standard errors at the administrative district level.

Finally, Column 5 reports results of the two-step system GMM estimation. The GMM instrument set is composed of the first to fourth lags of all explanatory variables. The Hansen test of overidentified restrictions and the Arellano-Bond test for error autocorrelation guide our choice of the lag structure. The coefficient suggests that there is no significant association between the immigrant rate and the total crime rate. The coefficients on the one- and two-year lagged crime rates are positive and significant. According to the p-value of the Hansen test the null hypothesis that the instruments are valid

Table 2: The Effect of Immigrants on Total Crime

	OLS	RE	FE	FD	GMM
	(1)	(2)	(3)	(4)	(5)
Immigrants	0.061**	0.043***	0.025	0.010	0.005
	(0.030)	(0.017)	(0.024)	(0.052)	(0.006)
Clear-up Rate	−0.006***	0.006***	0.010***	0.012***	0.003
	(0.002)	(0.002)	(0.002)	(0.004)	(0.002)
Population	0.038**	0.110***	−0.576***	−0.555	0.011
	(0.019)	(0.025)	(0.123)	(0.385)	(0.014)
Male 15_39	−0.020***	0.007	0.018**	−0.011	0.002
	(0.006)	(0.005)	(0.007)	(0.016)	(0.002)
GDP	0.186***	−0.146***	0.020	−0.070	0.018
	(0.048)	(0.040)	(0.050)	(0.214)	(0.024)
Unemployment	0.045***	0.009***	−0.002	0.004	0.003
	(0.003)	(0.002)	(0.003)	(0.005)	(0.002)
City	0.421***	0.688***			
	(0.039)	(0.040)			
West	−0.091**	−0.219***			
	(0.036)	(0.033)			
$Crime_{t-1}$					0.715***
					(0.095)
$Crime_{t-2}$					0.263***
					(0.066)
Constant	6.089***	7.959***			
	(0.503)	(0.416)			
District and year FE	no	no	yes	yes	yes
Observations	5,474	5,474	5,474	5,083	4,692
R^2	0.681	0.226	0.099	0.136	
Instruments					383
Hansen					366.734
(p-value)					(0.392)
AR(2)					−0.545
(p-value)					(0.586)

Note: Robust standard errors in parentheses are clustered at the administrative district level. GMM standard errors are the ones suggested by Windmeijer (2005). Significance level: * p<0.1; ** p<0.05; *** p<0.01

cannot be rejected. We also fail to reject the null hypothesis of no autocorrelation. Standard errors in parentheses are the ones suggested by Windmeijer (2005).

As both the fixed-effects and the first-difference models allow for unobserved effects to be correlated with the explanatory variables and are supported by various tests, Table 3 displays the results for those two models for the crime subcategories. With the fixed-effects specification (see Table 3.1), the effect of the immigrant rate on the drug offence rate is positive, but insignificant. In addition, there seems to be a significant negative association between the immigrant rate and the rates for damage to property and street crime. However, these results are sensitive to the choice of the estimation methodology. The first-difference estimates of the effect of the immigrant rate on the crime subcategories are all insignificant (see Table 3.2). All GMM estimates are sensitive to alternative lag structures and therefore results are not robust.[9]

To summarize, in this subsection we presented results of five different approaches to estimate the association between the immigrant rate and the crime rate. Each estimator makes different assumptions about unobserved effects. Methods that do not control for unobserved effects or assume them to be random, estimate significantly positive correlations between both rates. In contrast, all methods that

[9] Results are available from the authors upon request.

Table 3: The Effect of Immigrants on Crime Subcategories

3.1: Fixed Effects

	Burglary	Damage to Property	Drug Offences	Street Crime
	(1)	(2)	(3)	(4)
Immigrants	−0.102	−0.054**	0.079	−0.079***
	(0.078)	(0.025)	(0.058)	(0.029)
District and year FE	yes	yes	yes	yes
All control variables	yes	yes	yes	yes
Observations	5,474	5,474	5,474	5,474
R^2	0.040	0.036	0.027	0.026

3.2: First-Difference

	Burglary	Damage to Property	Drug Offences	Street Crime
	(1)	(2)	(3)	(4)
Immigrants	0.080	−0.026	−0.066	−0.017
	(0.079)	(0.033)	(0.050)	(0.030)
District and year FE	yes	yes	yes	yes
All control variables	yes	yes	yes	yes
Observations	5,083	5,083	5,083	5,083
R^2	0.055	0.136	0.052	0.045

Note: Control variables include *clear-up rate*, *population*, *male* 15_39, *GDP* and *unemployment*. Robust standard errors in parentheses are clustered at the administrative district level. Significance level: * p<0.1; ** p<0.05; *** p<0.01

control for district and year fixed effects yield insignificant or negative and significant coefficients for the immigrant rate. The results vary somewhat across the subcategories of crime. Two of the fixed-effects estimates are negative and significant while the first-difference estimates are all insignificant. In the remaining part of the paper we focus on fixed-effects specifications. The results for first-difference specifications are available from the authors upon request.

5.2 Robustness Checks

In the previous section, we have shown that there is no clear relationship between the immigrant rate and the crime rate at the district-level. In the following, we want to assess the robustness of this finding by addressing the following issues: heterogeneity of immigrant groups and naturalized immigrants, spillover on natives, omitted variables and spatial correlation. See Table 1 above for summary statistics of the additional variables used in the following.

5.2.1 Immigrants' Characteristics

Table 4 presents the results of fixed-effects estimations where immigrants are disaggregated according to gender and age groups. The variable *immigrants male* is the logarithm of the male immigrant rate, i.e., the total number of male immigrants per 100,000 district population, and *immigrants female* is, respectively, the logarithm of the female immigrant rate. Furthermore, we study the crime effect of immigrants for seven age groups. Each age group is defined as the logarithm of immigrants of that particular age group per 100,000 district population.

Table 4.1 shows that there is no significantly positive association between crime and immigrants of either gender. Table 4.2 shows that some age groups are positively correlated with some crime categories while in other cases the correlation is negative or insignificant. There is a positive correlation

Table 4: The Effect of Immigrants on Crime by Gender and Age Groups

4.1: Gender

	Total Crime	Burglary	Damage to Property	Drug Offences	Street Crime
	(1)	(2)	(3)	(4)	(5)
Immigrants male	−0.059	0.106	0.0003	0.093	−0.089*
	(0.054)	(0.184)	(0.049)	(0.104)	(0.052)
Immigrants female	0.088	−0.206	−0.091	−0.035	−0.010
	(0.068)	(0.264)	(0.065)	(0.145)	(0.067)
District and year FE	yes	yes	yes	yes	yes
All control variables	yes	yes	yes	yes	yes
Observations	5,474	5,474	5,474	5,474	5,474
R^2	0.096	0.064	0.032	0.029	0.016

4.2: Age

	Total Crime	Burglary	Damage to Property	Drug Offences	Street Crime
	(1)	(2)	(3)	(4)	(5)
Immigrants					
Aged under_14	−0.034*	−0.022	−0.069***	0.057	−0.088***
	(0.019)	(0.055)	(0.020)	(0.041)	(0.019)
Aged 15_19	0.058***	0.028	0.037*	0.078*	0.013
	(0.016)	(0.070)	(0.020)	(0.044)	(0.021)
Aged 20_24	−0.004	−0.055	0.013	−0.008	0.036
	(0.020)	(0.085)	(0.022)	(0.055)	(0.027)
Aged 25_29	−0.016	0.049	−0.031	0.058	−0.030
	(0.031)	(0.100)	(0.028)	(0.072)	(0.031)
Aged 30_34	0.032	0.021	0.038	0.017	0.023
	(0.032)	(0.110)	(0.033)	(0.074)	(0.034)
Aged 35_39	0.013	0.081	−0.021	−0.131*	0.065*
	(0.030)	(0.129)	(0.032)	(0.072)	(0.034)
Aged over_40	−0.086***	−0.068	0.003	−0.099	−0.084**
	(0.030)	(0.123)	(0.031)	(0.074)	(0.040)
District and year FE	yes	yes	yes	yes	yes
All control variables	yes	yes	yes	yes	yes
Observations	5,474	5,474	5,474	5,474	5,474
R^2	0.102	0.065	0.036	0.036	0.021

Note: Control variables include *clear-up rate*, *population*, *GDP* and *unemployment*. Instead of *male* 15_39 *Germans male*15_39 is included. Robust standard errors in parentheses are clustered at the administrative district level. Significance level: * p<0.1; ** p<0.05; *** p<0.01

between immigrants aged 15-19 and total crime, damage to property and drug offences, while immigrants aged 35-39 are positively correlated with street crime. In all other cases, the coefficients of the immigrants' age groups are either negative or not statistically different from zero.

Furthermore, considering interactions of age and gender, there is a positive correlation between immigrant men aged 15-19 and total crime, damage to property and street crime. Female immigrants aged 25-29, 35-39 and over 40 are positively and significantly correlated with drug offences, street crime and burglary, respectively (see Table B1 in the Appendix).

Another characteristic we consider is immigrants' nationality by regions.[10] The immigrant rate for a particular region is defined as the number of immigrants from that region per 100,000 district population. Table 5 shows the fixed-effects estimates of the effect of immigrants from different regions

[10]The Federal Statistical Office (2018) classifies Cyprus and Turkey as European countries because the former is a member of the European Union and the later is a candidate country.

on the crime rate. A positive and moderately significant correlation is found between the immigrant rate from Africa and burglary. An increase in the immigrant rate from America is associated with an increase in total and street crime. Also, a positive and significant correlation is found between the immigrant rate from Europe and drug offences. In contrast, there is no positive significant association between immigrants from Asia or Australia and Ociania and either crime categories. Overall, there is no clear picture. For most immigrant-groups and subcategories of crime, no positive association can be found.

Table 5: The Effect of Immigrants on Crime by Nationality

	Total Crime	Burglary	Damage to Property	Drug Offences	Street Crime
	(1)	(2)	(3)	(4)	(5)
Immigrants from:					
Africa	0.028	0.057^{*}	0.002	-0.010	-0.016
	(0.017)	(0.034)	(0.012)	(0.028)	(0.012)
America	0.049^{**}	-0.146^{*}	-0.003	0.043	0.060^{***}
	(0.022)	(0.079)	(0.020)	(0.050)	(0.023)
Asia	0.023	0.008	-0.020	-0.088^{**}	-0.037^{*}
	(0.026)	(0.065)	(0.018)	(0.041)	(0.022)
Australia and Oceania	-0.009	0.030	-0.007	-0.003	-0.002
	(0.006)	(0.022)	(0.006)	(0.014)	(0.007)
Europe	-0.002	-0.128^{*}	-0.036	0.124^{*}	-0.047
	(0.027)	(0.076)	(0.028)	(0.064)	(0.032)
District and year FE	yes	yes	yes	yes	yes
All control variables	yes	yes	yes	yes	yes
Observations	5,474	5,474	5,474	5,474	5,474
R^2	0.107	0.045	0.036	0.030	0.030

Note: Control variables include *clear-up rate*, *population*, *male*15_39, *GDP* and *unemployment*. Robust standard errors in parentheses are clustered at the administrative district level. Significance level: * p<0.1; ** p<0.05; *** p<0.01

5.2.2 Top Asylum Countries and Persons Seeking Protection

The recent inflow of persons seeking protection in Germany has also led to concerns in the population about raising crime rates. Even though our period of observation does not cover the very last years, we consider two different modifications of our immigrants variable to shed light on this. First, we separately study the crime effect of immigrants from the top 5 asylum countries. The variable *top asylum countries* is defined as the sum of the stock of immigrants from the top 5 asylum countries in a given year per 100,000 district population from Federal Office for Migration and Refugees (2013, 2017), where we take the changing composition of the top asylum countries over the years into account (see Table B2 in Appendix B). Table 6.1 shows that there is no positive significant association between immigrants from the top asylum countries and either crime categories. All the other immigrants, i.e. those not coming from the top 5 asylum countries, are associated with a moderately significant increase in total crime. The coefficients for the subcategories are all insignificant or negative.

Second, we also separately study the effect of persons seeking protection, i.e. foreigners living in Germany based on humanitarian reasons. The variable of interest is defined as the stock of persons seeking protection per 100,000 district population. The data, published by the Federal Statistical Office (2018), are available for the period 2007-2016. Table 6.2 shows a positive and moderately significant association between persons seeking protection and burglary. In all the other cases, the coefficients are either negative or insignificant.[11]

Table 6: The Effect of Immigrants on Crime by Refugee Status

6.1: The Effect of Immigrants from Top 5 Asylum Countries on Crime

	Total Crime	Burglary	Damage to Property	Drug Offences	Street Crime
	(1)	(2)	(3)	(4)	(5)
Top asylum countries	-0.001	0.000	-0.017^{**}	0.027	-0.019^{**}
	(0.007)	(0.022)	(0.007)	(0.017)	(0.008)
Other immigrants	0.045^{*}	-0.025	0.010	0.002	-0.047^{*}
	(0.025)	(0.066)	(0.022)	(0.047)	(0.026)
District and year FE	yes	yes	yes	yes	yes
All control variables	yes	yes	yes	yes	yes
Observations	5,474	5,474	5,474	5,474	5,474
R^2	0.100	0.039	0.036	0.028	0.026

6.2: The Effect of Persons Seeking Protection on Crime

	Total Crime	Burglary	Damage to Property	Drug Offences	Street Crime
	(1)	(2)	(3)	(4)	(5)
Persons seeking protection	0.034	0.051^{*}	-0.013	-0.008	-0.034^{***}
	(0.024)	(0.029)	(0.010)	(0.024)	(0.012)
Other immigrants	0.037	0.110	-0.012	0.064	-0.076^{**}
	(0.038)	(0.077)	(0.030)	(0.077)	(0.038)
District and year FE	yes	yes	yes	yes	yes
All control variables	yes	yes	yes	yes	yes
Observations	3,910	3,910	3,910	3,910	3,910
R^2	0.125	0.014	0.030	0.045	0.023

Note: Control variables include *clear-up rate*, *population*, *male* 15_39, *GDP* and *unemployment*. Robust standard errors in parentheses are clustered at the administrative district level. The regressions for persons seeking protection are run over the period 2007–2016. Significance level: * p<0.1; ** p<0.05; *** p<0.01

5.2.3 The Cost of Punishment: Naturalized Immigrants

So far we have only considered foreigners, i.e. immigrants holding foreign nationality. Once immigrants naturalize they are not counted any longer as foreigners but as Germans. Naturalized immigrants are not subject to deportation and subsequently their cost of punishment for committing a crime is lower compared to immigrants holding a foreign nationality. This "cost" effect suggests that, ceteris paribus, naturalized immigrants commit more crimes. However, it is likely that those who commit crimes before their naturalization have a lower probability to become naturalized. In other words, naturalized immigrants are selected from immigrants with a lower propensity to commit crimes. This "selection" effect counteracts the "cost" effect. Thus, it is interesting to study the relationship between naturalized immigrants and the crime rate. The variable of interest is defined as the logarithm of the number of naturalized immigrants in a given year per 100,000 district inhabitants. The data are published by the Federal Statistical Office and the Statistical Offices of the Länder (2018) for the period 2011-2016.

Table 7 shows that there is no significant correlation between the total number of naturalized immigrants and the crime rates, except for burglary with a moderately significant, positive correlation.[12]

[11]For comparison, we re-run the basic regression for the same reduced period 2007-2016. See Table B3 in Appendix B.

[12]For comparison, we re-run the basic regression for the same reduced period 2011-2016. See Table B4 in Appendix B.

Table 7: The Effect of Naturalized Immigrants on Crime

	Total Crime	Burglary	Damage to Property	Drug Offences	Street Crime
	(1)	(2)	(3)	(4)	(5)
Naturalized Immigrants	0.011	0.052*	−0.010	0.029	−0.004
	(0.012)	(0.030)	(0.010)	(0.021)	(0.012)
District and year FE	yes	yes	yes	yes	yes
All control variables	yes	yes	yes	yes	yes
Observations	2,346	2,346	2,346	2,346	2,346
R^2	0.132	0.019	0.013	0.022	0.01

Note: Control variables include *clear-up rate*, *population*, *male* 15_39, *GDP* and *unemployment*. The regressions are run over the period 2011–2016. Robust standard errors in parentheses are clustered at the administrative district level. Significance level: * p<0.1; ** p<0.05; *** p<0.01

There is an indication that the "cost" effect and the "selection" effect balance one another – if they are present at all.

5.2.4 Spillover on Natives

The crime effect of immigrants can be driven by the direct effect, i.e. immigrants commit crimes themselves, or by indirect, spillover effects. The latter may arise even if immigrants commit fewer crimes than natives but induce natives to commit more crimes by, for example, worsening their labour market opportunities or because natives commit hate crimes against foreigners.

Throughout this paper, we measure the crime rate by the number of offences reported to the police committed by both natives and immigrants. Until the reported crimes are solved and suspects are identified, we do not know who committed the crime and, therefore, we cannot differentiate between crimes committed by foreigners and Germans nor can we disentangle the direct effect of immigrants on crime and the indirect, spillover effect.

A possible, though not perfect way to deal with this issue is to use the number of suspects by nationality, distinguishing German and non-German suspects, as a dependent variable. However, it must be noted that this approach has its limitations. First, only a fraction of reported offences is solved and, second, suspects need not necessarily be those who committed the crime. Some suspects might be accused of an offence they did not commit by mistake or, in case of foreigners, because of discrimination. Furthermore, the data are published by the Federal Criminal Police Office (2017) for the period 2013–2016 only.[13] The results should be interpreted with this in mind.

Tables 8.1 and 8.2 report fixed-effects estimates of the effect of the immigrant rate on the rates of foreign and German suspects, respectively. The dependent variable is defined as the logarithm of the number of registered suspects (by nationality) per 100,000 inhabitants. An increase in the immigrant rate is associated with a significant increase in the number of foreign suspects of all crimes. This points towards discrimination, as this is only partially reflected in the correlation between the immigrant rate and the crime rate for this period (see Table B5). In contrast, there is a significant, negative correlation between the immigrant rate and the rate of German suspects of damage to property, while the correlation is insignificant for total, burglary and street crimes. A positive significant association is found between the immigrant rate and the rate of German suspects of drug offences. Given the data limitations, we tentatively conclude that there are no indications of spillover effects on natives for most of the crime subcategories.

[13] For comparison, we re-run the basic regression for the same reduced period 2013-2016. See Table B5 in Appendix B.

Table 8: Suspects and Immigrants

8.1: The Effect of Immigrants on the Number of Foreign Suspects

	Foreign Suspects Total Crime	Foreign Suspects Burglary	Foreign Suspects Damage to Property	Foreign Suspects Drug Offences	Foreign Suspects Street Crime
	(1)	(2)	(3)	(4)	(5)
Immigrants	0.800***	0.427*	0.973***	0.670***	0.891***
	(0.097)	(0.236)	(0.143)	(0.130)	(0.134)
District and year FE	yes	yes	yes	yes	yes
All control variables	yes	yes	yes	yes	yes
Observations	1,564	1,564	1,564	1,564	1,564
R^2	0.709	0.178	0.150	0.260	0.194

8.2: The Effect of Immigrants on the Number of German Suspects

	German suspects Total Crime	German suspects Burglary	German suspects Damage to Property	German suspects Drug Offences	German suspects Street Crime
	(1)	(2)	(3)	(4)	(5)
Immigrants	−0.009	0.054	−0.104***	0.049**	−0.053
	(0.019)	(0.148)	(0.034)	(0.022)	(0.041)
District and year FE	yes	yes	yes	yes	yes
All control variables	yes	yes	yes	yes	yes
Observations	1,564	1,564	1,564	1,564	1,564
R^2	0.076	0.162	0.285	0.890	0.269

Note: Control variables include *clear-up rate*, *population*, *male* 15_39, *GDP*, *unemployment* and the respective crime rates. The regressions are run over the period 2013–2016. Robust standard errors in parentheses are clustered at the administrative district level. Significance level: * p<0.1; ** p<0.05; *** p<0.01.

5.2.5 Omitted Variable Bias

Omitted variable bias could be an issue despite the two-way fixed-effects approach employed as main specification. One potential omitted variable is a measure of the political orientation of the political majority. For example, a very conservative party would probably devote more resources to crime deterrence and at the same time oppose immigrants. To control for this, we include the share of right-wing politicians in the governing majority in the parliaments at the state-level (Landtag) from the Federal Returning Officer (2018) as a measure of the impact of the ideology of the government.[14] The variable *politics* is defined over the interval $[0,1]$, where zero indicates a left-wing government and one indicates a right-wing government. The regression coefficient of this variable can be interpreted as the effect of replacing a left-wing government by a right-wing one.

Table 9 presents results of the inclusion of the political ideology as a control variable in the fixed-effects regressions. The coefficients are in most cases insignificant and their inclusion has a small effect on the coefficients of the immigrant rate, which are now all insignificant.

5.2.6 Spatial Correlation

A further issue concerns spatial correlation that arises if observations are correlated across space.[15] For example, given the relatively small size of the German administrative districts, mobility of immigrants between neighbouring districts, that are sometimes cities, can be non-negligible. If immigrants

[14] Standard errors are now clustered at the state-level.

[15] We do not consider dynamic spatial panel models, i.e., variables are not lagged in time but only in space and the errors are not serially autocorrelated but only spatially.

Table 9: Omitted Variables Bias

	Total Crime	Burglary	Damage to Property	Drug Offences	Street Crime
	(1)	(2)	(3)	(4)	(5)
Immigrants	0.026	−0.103	−0.052	0.079	−0.078
	(0.027)	(0.083)	(0.033)	(0.111)	(0.050)
Politics	−0.016	0.024	-0.031^{**}	−0.008	−0.022
	(0.024)	(0.087)	(0.013)	(0.034)	(0.039)
District and year FE	yes	yes	yes	yes	yes
All control variables	yes	yes	yes	yes	yes
Observations	5,474	5,474	5,474	5,474	5,474
R^2	0.100	0.040	0.040	0.027	0.028

Note: Control variables include *clear-up rate*, *population*, *male* 15_39, *GDP* and *unemployment*. Robust standard errors in parentheses are clustered at the State level. Significance level: * p<0.1; ** p<0.05; *** p<0.01

commit crimes in other districts than the one of their residence, regional spillovers could be of importance. In fact, both marginal and conditional Lagrange Multiplier tests by Baltagi et al. (2003) suggest that there is a spatial autocorrelation (p-value 0.000 in all cases).

We control for potential regional spillovers by employing the spatial Durbin error model (SDEM) with time- and individual fixed effects in order to account for spatial heterogeneity. The model is estimated using maximum likelihood estimation. The spatial lags included in the model are weighted averages of immigrant rates in neighbouring districts and error term. The weights are constructed as an inverse of the Euclidean geographic distance between district centroids in kilometres with a cut-off distance of 200 kilometres beyond which weights are restricted to zero. The spatial data are taken from GeoBasis-DE / Federal Agency for Cartography and Geodesy (2017).

Table 10 presents the impact estimates of the SDEM. The results suggest that, controlling for spatial spillovers and considering direct and indirect effects, there is no positive and significant correlation between the immigrant rate and crime categories except for drug offences where this is due to significant spillovers of immigrants on the drug offence rate in neighbouring regions.

Table 10: Spatial Panel Durbin Error Model

	Total Crime	Burglary	Damage to Property	Drug Offences	Street Crime
	(1)	(2)	(3)	(4)	(5)
Direct Effect					
Immigrants	0.011	−0.046	-0.043^{**}	0.039	-0.081^{***}
	(0 .017)	(0.049)	(0.019)	(0.039)	(0.019)
Indirect Effect					
Immigrants	− 0.027	-1.035^{***}	-0.421^{***}	0.448^{***}	-0.240^{***}
	(0.064)	(0.310)	(0.060)	(0.091)	(0.077)
Total Effect					
Immigrants	− 0.016	-1.082^{***}	-0.464^{***}	0.487^{***}	-0.321^{***}
	(0.063)	(0.312)	(0.057)	(0.084)	(0.075)
District and year FE	yes	yes	yes	yes	yes
All control variables	yes	yes	yes	yes	yes
Observations	5,474	5,474	5,474	5,474	5,474
Pseudo R^2	0.032	0.006	0.031	0.012	0.053

Note: Control variables include *clear-up rate*, *population*, *male* 15_39, *GDP* and *unemployment*. Standard errors are computed using delta-method. Significance level: * p<0.1; ** p<0.05; *** p<0.01

6 Causality

The evidence on the relationship between immigrants and crime presented so far does not allow for causal interpretation. It can be biased because of endogeneity. The identifying assumption of the methods employed above is that immigrants' location choice is independent of the crime rate. The estimates of the effect of immigrants on crime would be biased downwards if immigrants choose to locate in districts with a low crime rate. This could be because of direct or indirect effects. The direct effect could arise if immigrants are attracted to areas where the crime rate is low. There could also be an indirect effect if immigrants choose districts with better labour market conditions, which have a negative effect on crime – if this is not captured by the unemployment rate or the GDP per capita. In contrast, the coefficient of the immigrant rate would be upward-biased if immigrants move to areas with a high crime rate, for instance, because these areas have lower housing prices.

Because of the endogeneity concerns, the instrumental variable (IV) approach is employed. To be valid, the instrument must satisfy two requirements. First, it must be exogenous, that is uncorrelated with the error term. Second, it must be relevant, which means it must be correlated with the endogenous variable, in this case, with the immigrant rate.

We construct the instrument using the past distribution of immigrants into districts to predict the present one. Altonji and Card (1991) and Card (2001) pioneered this approach by studying labour market outcomes of immigrants. In the immigration and crime context, Spenkuch (2014), Bell et al. (2013) and Bianchi et al. (2012) apply the same idea. However, the difference between Spenkuch (2014) and Bell et al. (2013) on the one hand and Bianchi et al. (2012) on the other hand is that the former apply the approach without modification and base their instrument for the present stock of immigrants in a country on the past distribution of immigrants there. The latter, on the contrary, modify the method slightly and use the present immigrants to other European countries as the basis for their instrument for the present stock of immigrants. In the following, we will implement two instruments, one based on Spenkuch (2014) and Bell et al. (2013) and one based on Bianchi et al. (2012), to address the issue of causality in a comprehensive way.

6.1 Method

For constructing the instrument following Spenkuch (2014) and Bell et al. (2013), we use the observation that newly arrived immigrants tend to settle in ethnic clusters created by previous immigrants (see, e.g., Bartel, 1989). Thus, the past distribution of immigrants of given nationality across German districts can be used to predict the distribution of the current stock of immigrants of that nationality living in Germany. Formally, the logarithm of the predicted immigrant rate in district i in time t is

$$\widetilde{immigrants}_{it} = \ln\left(\frac{\sum_n \omega^n_{i,t-5} \times immigrants^n_{Germany,t}}{population_{i,t}} \times 100{,}000\right), \tag{12}$$

where $immigrants^n_{Germany,t}$ is the stock of immigrants of nationality n living in Germany in year t and $\omega^n_{i,t-5} = immigrants^n_{i,t-5}/immigrants^n_{Germany,t-5}$ represents the share of immigrants of nationality n living in district i in year $t-5$ in the total stock of immigrants of that nationality living in Germany in year $t-5$.

The first stage of the two-stage least squares (2SLS) estimation is

$$\widehat{immigrants}_{it} = \alpha\ \widetilde{immigrants}_{it} + \eta X_{it} + \mu_{it} \quad (I\ stage), \tag{13}$$

where $\widetilde{immigrants}_{it}$ is the logarithm of the predicted immigrant rate. Then, by inserting the instrumented logarithm of the immigrant rate, $\widehat{immigrants}_{it}$, from the first stage into the second stage of the 2SLS estimation, one gets

$$crime_{it} = \beta \ \widehat{immigrants}_{it} + \gamma X_{it} + \nu_{it} \ \ (II\ stage). \tag{14}$$

For the instrument to be exogenous, the past shares of immigrants of different nationalities in the districts, $\omega^n_{i,t-5}$, and the present stock of immigrants in Germany, $immigrant^n_{Germany,t}$, both should be exogenous. We cannot test the exogeneity restriction. However, it is reasonable to assume that the present crime rate in the districts did not affect the past distribution of immigrants into the districts and neither the total stock of immigrants of different nationalities that are living in Germany.

Bianchi et al. (2012) make the exogeneity argument even stronger given the way they construct the instrument. They argue that the supply-push factors, i.e. events in the country of origin that increase immigration, affect the number of immigrants in all destination countries. Following this approach, the variation in the stock of immigrants in countries other than Germany is used as an instrument for changes in the immigrant population across German administrative districts. Formally, the predicted log change of the immigrant rate in district i in time t is

$$\Delta\widetilde{immigrants}_{it} = \sum_n \omega^n_{i,t-\Delta t} \times \Delta\ln\ immigrants^n_t \tag{15}$$

where $\omega^n_{i,t-\Delta t} = immigrants^n_{i,t-\Delta t}/immigrants_{i,t-\Delta t}$ is the share of immigrants of nationality n in total stock of immigrants in district i in period $t-\Delta t$ and $\Delta\ln\ immigrants^n_t$ is the log change of the number of immigrants of nationality n in destination countries other than Germany.

Given this instrument, the first stage of the 2SLS estimation is

$$\Delta\widehat{immigrants}_{it} \ = \ \alpha\Delta\widetilde{immigrants}_{it} + \eta\Delta X_{it} + \Delta\mu_{it}, \qquad (I\ stage) \tag{16}$$

where $\Delta\widetilde{immigrants}_{it}$ is the instrument as defined in Equation (15). Then, by inserting the predicted log change of the immigrant rate in German administrative districts, $\Delta\widehat{immigrants}_{it}$, from the first stage into the second stage of the 2SLS estimation, one gets

$$\Delta crime_{it} \ = \ \beta\Delta\widehat{immigrants}_{it} + \gamma\Delta X_{it} + \Delta\nu_{it}, \qquad (II\ stage) \tag{17}$$

where all variables are defined as log changes between 2005, 2010 and 2015. The coefficient of interest, β, relates the predicted change in the logarithm of the immigrant rate to the change in the logarithm of the crime rate.

The data on the stock of immigrants in destination countries other than Germany is based on the estimates of the international immigrants' stock from the United Nations (2017) database. This database provides information on the stock of immigrants by countries of destination and origin for the years 2005, 2010, 2015. The coverage is limited and the number of destination countries included is determined by data availability. The instrument is constructed for 14 destination countries and 178 origin countries. The predictive power of the instrument relies on the importance of the supply-push factors and the similarity of the immigration patterns in Germany and the other destination countries. Therefore, the destination countries included are the ones that border Germany, have German as official language or are major immigration countries in Europe. The detailed list of countries of origin and destination can be found in Appendix A.

In the following, we consider changes over a 10-year period as well as over a 5-year period.

6.2 IV Results

Table 11 presents the results of the IV estimation following the approach of Spenkuch (2014) and Bell et al. (2013). The upper panel shows the second stage and the bottom panel shows the first stage of the 2SLS estimation. There is a positive and moderately significant effect of immigrants on drug offences. The coefficient of the immigrant rate is insignificant for all the other crime categories suggesting that immigrants do not increase the considered crime categories.

Following the approach of Bianchi et al. (2012), Table 12.1 presents the instrumental variable estimates on the cross-section of the 10-year difference data for the total crime rate and each crime category. The IV estimates of the effect of the immigrant rate are either insignificant or negative, suggesting that there is no positive causal relationship between immigrants and crime.

The negative effects found indicate that a larger number of immigrants does not cause an increase in the aggregate crime rate; on the contrary, their presence leads to a decrease. The absence of a positive effect implies that the smaller legal income opportunities of immigrants compared to natives have only a limited impact – if at all – on their decision to commit crimes. At the same time, two different channels might be responsible for the observed negative effects: First, the effect of punishment, in particular in the form of deportation, could play an important role in deterring criminal activities of immigrants. Second, a larger number of immigrants could cause an increase in police presence at the district-level, e.g., because of increased concerns of natives about them. The presence of the police could have a negative effect on crimes committed by both immigrants and natives.

Table 11: IV Results – Instrument of Spenkuch (2014) and Bell et al. (2013)

	Total Crime	Burglary	Damage to Property	Drug Offences	Street Crime
	(1)	(2)	(3)	(4)	(5)
Second–Stage					
$\widehat{immigrants}$	0.039	-0.139	-0.046	0.287^{*}	-0.114
	(0.062)	(0.256)	(0.068)	(0.157)	(0.076)
First–Stage					
$\widetilde{immigrants}$	0.424^{***}	0.425^{***}	0.426^{***}	0.426^{***}	0.426^{***}
	(0.042)	(0.042)	(0.042)	(0.042)	(0.042)
District and year FE	yes	yes	yes	yes	yes
All control variables	yes	yes	yes	yes	yes
Observations	5,474	5,474	5,474	5,474	5,474
F statistic (excl. instr.)	101.018	102.965	102.405	101.917	102.701

Note: Control variables include *clear-up rate*, *population*, *GDP* and *unemployment*. Instead of *male* 15_39, *Germans male* 15_39 is included. Robust standard errors are in parentheses. Significance level: * p<0.1; ** p<0.05; *** p<0.01

As the 10-year difference data neglects much of the variance, Table 12.2 shows the results of IV estimates using the 5-year differenced data.[16] The coefficients of the immigrant rate are insignificant for all crime categories except for damage to property. The coefficient of the latter is positive and moderately significant. Overall, different instruments relying on slightly different assumptions confirm the results of our main regressions.

[16]To see why there is a difference between the results of the 5-year and the 10-year approach, we estimate the IV regression with the 5-year span separately for each cross section, that is, first using only the 5-year difference between 2010 and 2005 and then between 2015 and 2010. The estimates of the immigrant rate are all insignificant and in most cases negative. Though, the F-statistic for the difference between 2010 and 2005 is very small suggesting that the instrument is not relevant for this time period. The results are available from the authors upon request.

Table 12: IV Results – Instrument of Bianchi et al. (2012)

12.1: 10-year Difference between 2005-2015

	Total Crime	Burglary	Damage to Property	Drug Offences	Street Crime
	(1)	(2)	(3)	(4)	(5)
Second–Stage					
$\Delta\widehat{immigrants}$	-0.432^{*}	-2.180^{***}	-0.360^{*}	0.415	-0.366^{*}
	(0.252)	(0.716)	(0.206)	(0.359)	(0.220)
First–Stage					
$\Delta\widetilde{immigrants}$	0.840^{***}	0.791^{***}	0.856^{***}	0.828^{***}	0.822^{***}
	(0.149)	(0.152)	(0.151)	(0.153)	(0.152)
All control variables	yes	yes	yes	yes	yes
Observations	391	391	391	391	391
F statistic (excl. instr.)	31.804	26.984	32.138	29.307	29.041

12.2: 5-year Differences between 2005-2015

	Total Crime	Burglary	Damage to Property	Drug Offences	Street Crime
	(1)	(2)	(3)	(4)	(5)
Second–Stage					
$\Delta\widehat{immigrants}$	0.264	1.468	0.528^{*}	0.258	0.169
	(0.271)	(1.299)	(0.304)	(0.720)	(0.289)
First–Stage					
$\Delta\widetilde{immigrants}$	1.379^{***}	1.353^{***}	1.368^{***}	1.386^{***}	1.355^{***}
	(0.371)	(0.370)	(0.371)	(0.371)	(0.372)
All control variables	yes	yes	yes	yes	yes
District and year FE	yes	yes	yes	yes	yes
Observations	782	782	782	782	782
F statistic (excl. instr.)	13.790	13.379	13.630	13.968	13.296

Note: Countries included are: Austria, Belgium, Denmark, Luxembourg, Liechtenstein, Netherlands, France, Finland, Norway, Sweden, Spain, Portugal, Greece, United Kingdom. Control variables include *clear-up rate*, *population*, *male* 15_39, *GDP* and *unemployment*. Robust standard errors are in parentheses. Significance levels: * p<0.1; ** p<0.05; *** p<0.01

7 Conclusions

The paper focuses on the effect of immgrants on crime. First, by employing standard panel estimation methods we find no positive and significant correlation between the immigrant rate and the crime rate. Next, disaggregating immigrants based on gender, age, country of origin and refugee status, we conclude that there is a significantly positive association between some immigrant groups and crime categories but in most cases the relationship is negative or insignificant and the overall picture is not clear. There is a positive association between naturalized immigrants and the burglary rate and immigrants and German suspects of drug offences. Overall, we find no supporting evidence for the possibly deterrent effect of deportation nor for the existence of spillover effects on natives. Furthermore, controlling for possible omitted variables and spatial correlation, there is no positive relationship between immigrants and crime either. In addition, the results need to be seen against the possible upward bias as argued before.

As there is a concern of endogeneity, we employ an instrumental variable approach which allows for causal interpretation of the results. We find moderate positive effect of the immigrant rate on drug offences and property damage but these findings are sensitive to the choice of the instrument. Overall, we find no evidence that immigrants increases crime.

Our result that immigrants do not increase the total crime rate is in accordance with the existing literature except for the results of Piopiunik and Ruhose (2017). The later study, however, a specific group of immigrants – ethnic Germans – for the period 1996–2005 and only in West Germany while our study considers immigrants in general for the whole of Germany. Ethnic German repatriates constitute a special group. They receive German nationality upon arrival and are thus not subject to deportation. As a consequence, their cost of crime is low. At the same time, Piopiunik and Ruhose (2017) argue that ethnic German immigrants do not match with the demand on the German labour market. The difficulty to integrate and poor legal earning options due to low language proficiency and low other skills decrease the opportunity cost of crime. Overall, both effects reinforcing each other and point at higher incentives for ethnic German repatriates to commit crimes compared to other immigrants.

Directly comparing the results for crime subcategories to previous studies is not possible. Most papers differentiate between violent and property crimes reporting positive effects of immigrants on the later. Among the crime categories we consider, the property related crimes are burglary and damage to property. We do not find a positive effect for most specifications. A similar result is found by Bell et al. (2013) for economic migrants in the United Kingdom.

Overall, we contribute to the literature about the relationship between immigrants and crime by providing comprehensive evidence for Germany for the period 2003–2016. Based on panel-data, which allow addressing the issues of unknown heterogeneity, spatial correlation and endogeneity, we conclude that immigrants do not increase the crime rate. Given the sometimes emotional coverage of this topic in the offline and online media – also against the background of the recent large inflow in Germany of persons seeking protection, this analysis seeks to provide facts for a more rational debate.

Appendix A. Countries Included in the Construction of the Instrument

Destination Countries

Austria, Belgium, Denmark, Luxembourg, Liechtenstein, Netherlands, France, Finland, Norway, Sweden, Spain, Portugal, Greece, United Kingdom.

Origin Countries

Afghanistan, Albania, Algeria, Andorra, Angola, Antigua and Barbuda, Argentina, Armenia, Australia, Azerbaijan, Bahamas, Bahrain, Bangladesh, Barbados, Belarus, Belize, Benin, Bhutan, Bolivarian Republic of Venezuela, Bosnia and Herzegovina, Botswana, Brazil, Brunei Darussalam, Bulgaria, Burkina Faso, Burundi, Cabo Verde, Cambodia, Cameroon, Canada, Central African Republic, Chad, Chile, China, Colombia, Comoros, Costa Rica, Cote d'Ivoire, Croatia, Cuba, Cyprus, Czech Republic, Democratic People's Republic of Korea, Democratic Republic of the Congo, Djibouti, Dominica, Dominican Republic, Ecuador, Egypt, El Salvador, Equatorial Guinea, Eritrea, Estonia, Ethiopia, Federated States of Micronesia, Fiji, Gabon, Gambia, Georgia, Grenada, Guatemala, Guinea, Guinea-Bissau, Guyana, Haiti, Honduras, Hong Kong, Hungary, Iceland, India, Indonesia, Islamic Republic of Iran, Iraq, Ireland, Israel, Jamaica, Japan, Jordan, Kazakhstan, Kenya, Kiribati, Kuwait, Kyrgyzstan, Lao People's Democratic Republic, Latvia, Lebanon, Lesotho, Liberia, Libya, Lithuania, Macau, Madagascar, Malawi, Malaysia, Maldives, Mali, Malta, Marshall Islands, Mauritania, Mauritius, Mexico, Monaco, Mongolia, Morocco, Mozambique, Myanmar, Namibia, Nauru, Nepal, New Zealand, Nicaragua, Niger, Nigeria, Oman, Pakistan, Palau, Panama, Papua New Guinea, Paraguay, Peru, Philippines, Plurinational State of Bolivia, Poland, Qatar, Republic of the Congo, Republic of Korea, Republic of Moldova, Romania, Russian Federation, Rwanda, Saint Kitts and Nevis, Saint Lucia, Saint Vincent and the Grenadines, Samoa, San Marino, Sao Tome and Principe, Saudi Arabia, Senegal, Serbia (with Kosovo) and Montenegro, Seychelles, Sierra Leone, Singapore, Slovakia, Slovenia, Solomon Islands, Somalia, South Africa, Sri Lanka, State of Palestine, Sudan and South Sudan, Suriname, Swaziland, Switzerland, Syrian Arab Republic, Tajikistan, Tanzania, Thailand, the former Yugoslav Republic of Macedonia, Timor-Leste, Togo, Tonga, Trinidad and Tobago, Turkey, Turkmenistan, Tuvalu, Uganda, Ukraine, United Arab Emirates, United States of America, Uruguay, Uzbekistan, Vanuatu, Viet Nam, Yemen, Zambia, Zimbabwe.

Note: For consistency, the number of immigrants from Serbia, Kosovo and Montenegro are summed up under "Serbia (with Kosovo) and Montenegro". Up until 2006, Montenegro was part of Serbia and gained independence in 2006. Kosovo, being a disputed territory, is not included as a sovereign country but as a part of Serbia. Similarly, South Sudan gained independence from Sudan in 2011. The number of immigrants from both countries are summed up under "Sudan and South Sudan".

Appendix B

Table B1: Age and Gender

	Total Crime (1)	Burglary (2)	Damage to Property (3)	Drug Offences (4)	Street Crime (5)
Immigrants male under_14	0.023	−0.007	−0.031	0.066	−0.042
	(0.026)	(0.103)	(0.031)	(0.073)	(0.035)
Immigrants male 15_19	0.060***	0.058	0.031*	0.025	0.041**
	(0.018)	(0.058)	(0.018)	(0.039)	(0.018)
Immigrants male 20_24	−0.012	−0.054	0.002	0.037	0.028
	(0.017)	(0.067)	(0.017)	(0.046)	(0.020)
Immigrants male 25_29	−0.034*	0.050	−0.031	−0.039	−0.051**
	(0.019)	(0.088)	(0.021)	(0.049)	(0.025)
Immigrants male 30_34	0.002	0.053	0.015	0.014	0.012
	(0.022)	(0.081)	(0.022)	(0.060)	(0.026)
Immigrants male 35_39	0.004	0.092	−0.004	−0.127**	0.018
	(0.024)	(0.096)	(0.027)	(0.059)	(0.026)
Immigrants male over_40	−0.046	−0.295**	−0.011	0.030	−0.057
	(0.040)	(0.145)	(0.040)	(0.082)	(0.044)
Immigrants female under_14	−0.068**	−0.002	−0.041	−0.021	−0.062*
	(0.027)	(0.101)	(0.031)	(0.074)	(0.033)
Immigrants female 15_19	−0.007	−0.063	0.008	0.060	−0.032
	(0.023)	(0.062)	(0.022)	(0.054)	(0.023)
Immigrants female 20_24	0.020	−0.025	0.010	−0.044	−0.001
	(0.019)	(0.085)	(0.021)	(0.053)	(0.023)
Immigrants female 25_29	0.024	−0.042	0.007	0.124*	0.044*
	(0.024)	(0.098)	(0.024)	(0.064)	(0.025)
Immigrants female 30_34	0.045	−0.101	0.025	0.020	0.018
	(0.031)	(0.130)	(0.031)	(0.066)	(0.031)
Immigrants female 35_39	0.015	0.077	−0.015	0.007	0.069**
	(0.028)	(0.108)	(0.032)	(0.076)	(0.031)
Immigrants female over_40	−0.053	0.290**	0.010	−0.152*	−0.034
	(0.041)	(0.136)	(0.040)	(0.082)	(0.052)
District and year FE	yes	yes	yes	yes	yes
All control variables	yes	yes	yes	yes	yes
Observations	5,474	5,474	5,474	5,474	5,474
R^2	0.107	0.070	0.037	0.039	0.026

Note: Control variables include *clear-up rate*, *population*, *Germans male* 15_39, *GDP* and *unemployment*. Instead of *male* 15_39 *Germans male*15_39 is included. Robust standard errors in parentheses are clustered at the administrative district level. Significance level: * p<0.1; ** p<0.05; *** p<0.01

Table B2: The Top 5 Asylum Countries

2003	2004	2005	2006	2007	2008	2009
Turkey	Turkey	Serbia	Serbia	Iraq	Iraq	Iraq
Serbia	Serbia	Turkey	Iraq	Serbia	Serbia	Afghanistan
Iraq	Russia	Iraq	Turkey	Turkey	Turkey	Serbia
Russia	Vietnam	Russia	Russia	Vietnam	Vietnam	Turkey
China	Iran	Vietnam	Vietnam	Russia	Iran	Iran

2010	2011	2012	2013	2014	2015	2016
Serbia	Afghanistan	Serbia	Russia	Syria	Syria	Syria
Afghanistan	Serbia	Afghanistan	Syria	Serbia	Albania	Afghanistan
Iraq	Iraq	Syria	Serbia	Eritrea	Serbia	Iraq
Iran	Iran	Iraq	Afghanistan	Afghanistan	Afghanistan	Iran
Macedonia	Syria	Macedonia	Macedonia	Albania	Iraq	Eritrea

Source: Federal Office for Migration and Refugees (2013, 2017)

Table B3: The Basic Regression for the Period 2007–2016

	Total Crime (1)	Burglary (2)	Damage to Property (3)	Drug Offences (4)	Street Crime (5)
Immigrants	0.030	0.143**	−0.031	0.009	−0.103***
	(0.029)	(0.072)	(0.027)	(0.068)	(0.031)
District and year FE	yes	yes	yes	yes	yes
All control variables	yes	yes	yes	yes	yes
Observations	3,910	3,910	3,910	3,910	3,910
R^2	0.119	0.012	0.030	0.045	0.020
Adjusted R^2	0.018	−0.102	−0.082	−0.066	−0.093
F Statistic	79.109***	7.278***	18.125***	27.372***	12.073***

Note: Control variables include *clear-up rate*, *population*, *male* 15_39, *GDP* and *unemployment*. The regressions are run over the period 2007–2015. Robust standard errors in parentheses are clustered at the administrative district level. Significance level: * p<0.1; ** p<0.05; *** p<0.01

Table B4: The Basic Regression for the Period 2011–2016

	Total Crime (1)	Burglary (2)	Damage to Property (3)	Drug Offences (4)	Street Crime (5)
Immigrants	0.072**	0.308***	0.018	0.048	−0.087**
	(0.036)	(0.084)	(0.033)	(0.073)	(0.037)
District and year FE	yes	yes	yes	yes	yes
All control variables	yes	yes	yes	yes	yes
Observations	2,346	2,346	2,346	2,346	2,346
R^2	0.134	0.027	0.013	0.022	0.016

Note: Control variables include *clear-up rate*, *population*, *male* 15_39, *GDP* and *unemployment*. The regressions are run over the period 2011–2015. Robust standard errors in parentheses are clustered at the administrative district level. Significance level: * p<0.1; ** p<0.05; *** p<0.01

Table B5: The Basic Regression for the Period 2013–2016

	Total Crime (1)	Burglary (2)	Damage to Property (3)	Drug Offences (4)	Street Crime (5)
Immigrants	0.042	0.236**	0.006	0.108	−0.138***
	(0.045)	(0.094)	(0.042)	(0.079)	(0.040)
District and year FE	yes	yes	yes	yes	yes
All control variables	yes	yes	yes	yes	yes
Observations	1,564	1,564	1,564	1,564	1,564
R^2	0.126	0.029	0.006	0.029	0.029

Note: Control variables include *clear-up rate*, *population*, *male* 15_39, *GDP* and *unemployment*. The regressions are run over the period 2013–2016. Robust standard errors in parentheses are clustered at the administrative district level. Significance level: * p<0.1; ** p<0.05; *** p<0.01

Table B6: 10-year Difference Regressions: OLS

	Total Crime (1)	Burglary (2)	Damage to Property (3)	Drug Offences (4)	Street Crime (5)
Δ *immigrants*	0.012	−0.381***	−0.159***	0.030	−0.186***
	(0.056)	(0.130)	(0.048)	(0.101)	(0.053)
All control variables	yes	yes	yes	yes	yes
Observations	391	391	391	391	391
R^2	0.256	0.106	0.090	0.076	0.078

Note: Control variables include *clear-up rate*, *population*, *male* 15_39, *GDP* and *unemployment*. Robust standard errors are in parentheses. Significance level: * p<0.1; ** p<0.05; *** p<0.01

Table B7: 5-year Difference Regressions

	Total Crime	Burglary	Damage to Property	Drug Offences	Street Crime
	(1)	(2)	(3)	(4)	(5)
Δ *immigrants*	0.032	1.106***	0.082*	0.289**	−0.098**
	(0.046)	(0.294)	(0.048)	(0.138)	(0.048)
All control variables	yes	yes	yes	yes	yes
District and year FE	yes	yes	yes	yes	yes
Observations	782	782	782	782	782
R^2	0.439	0.093	0.051	0.036	0.066

Note: Control variables include *clear-up rate*, *population*, *male* 15_39, *GDP* and *unemployment*. Robust standard errors are in parentheses. Significance level: * p<0.1; ** p<0.05; *** p<0.01

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