

Immigration, crime and police response

Pierre Robiglio - 454349

Abstract

Despite the null effect of immigration on crimes rates established by the literature, immigrants are over-represented in the prison population. Employing panel data on the universe of Dutch municipalities, I leverage the persistence of immigrants setting decisions in a shift-share strategy to study the causal effect of immigration. When applying the latest econometric refinements, I confirm the finding of the literature of no effect of immigration on crime. The rise in clear-up rate of property offences could partly conciliate the null effect with the over-representation. I also find, using survey data, that immigration increases the fear of becoming victim of pickpocketing. This finding contributes to the growing literature on crime misperceptions. Further research is needed to establish what exactly drives this rise in clear-up rates.



Erasmus School of Economics

Rotterdam University

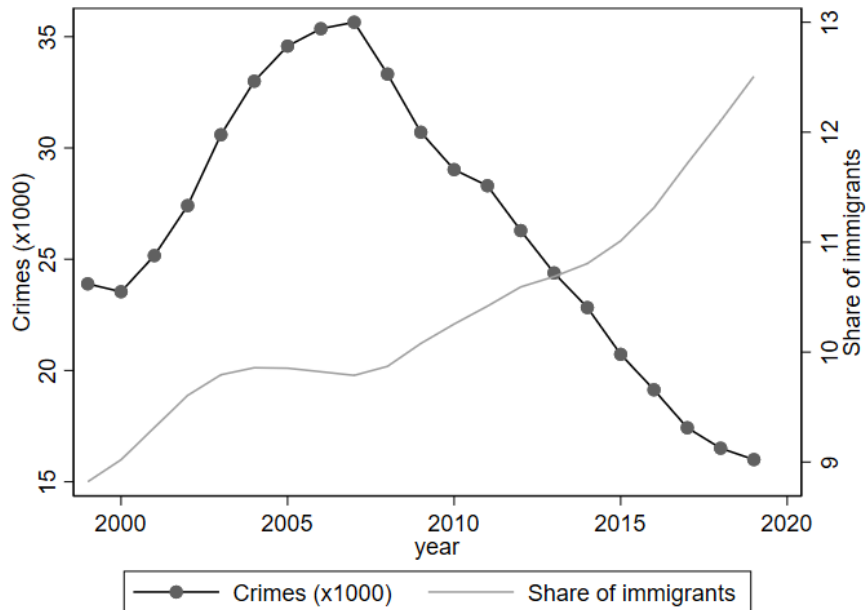
June 15, 2021

Contents

1	Introduction	2
2	Related literature	5
2.1	Immigration has no detectable effect on crime	5
2.2	Legal status has a significant crime-reducing impact	6
2.3	Immigration increases perceptions of crime	6
3	Data	7
4	Methodology	9
4.1	OLS regression and endogeneity concerns	9
4.2	Shift-share solution and the exogeneity assumption	10
5	Results	12
5.1	Main results	12
5.2	Survey results	14
6	Robustness	16
7	Conclusions	20

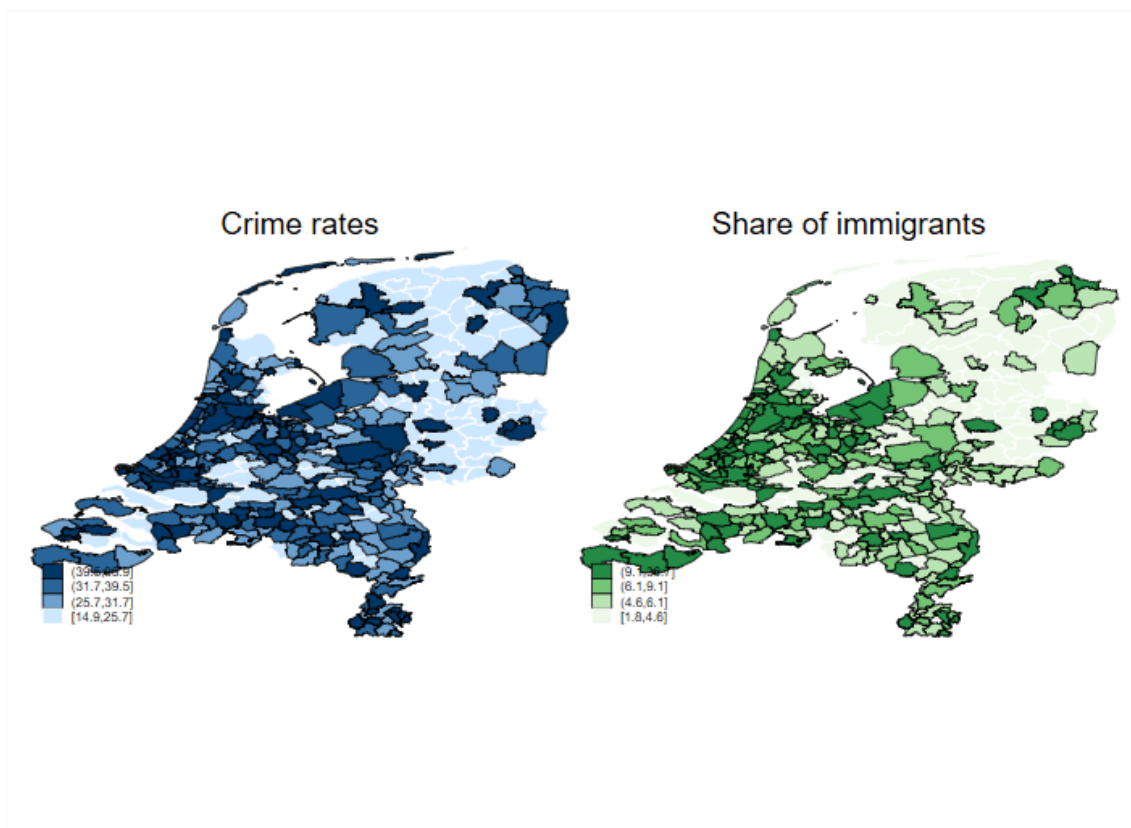
1 Introduction

In the 2017 wave of the European Value Survey, 52.7% of Dutch respondent agreed with the statement “Immigration increases crime”. This figure is even more striking if compared to the 29.7% of respondents for which “Immigration increases unemployment”, result in line with neighbouring European countries. Arguably, such remarkable agreement might underly the immigration-fueled rise of Far-right movements across Europe documented by a recent strain of literature (e.g. Halla et al., 2017[8]; Dustmann et al., 2019[17]; Roupakias & Chletsos 2020[23]; Edo et al. 2019[18]). The present paper focuses on the effect of immigration on criminal activity and its perceptions in the Netherlands. At first glance, when plotting the country-level statistics on immigration and crime, the trends seem opposite, as shown in Figure 1. In specific, while the share of immigrants has shown a steady increase in the past twenty years, crimes have dropped after reaching a peak around 2005. Importantly, Figure 1 might hide a strong correlation at the municipality level. To account for this, Figure 2 displays a map of the quartiles of crime rate and migrant share for all areas in our dataset in 2018 in.



Despite the apparent spatial correlation presented in Figure 2, the null effect of immigration on crime has been established by rigorous empirical literature. However, the lack of effect does not align with the overrepresentation of migrants in the prison population. In the Netherlands, first and second-generation immigrants represent 23% of the total population, but 65% of the prison population in 2019; if we consider only non-western immigrants, the unbalance is even starker, as

they amount to 13% of the population but 45% of those in prison¹.



How can we reconcile these two facts ? Based on the work of Marie and Pinotti (2021), I here outline three possible explanations:

1. Despite being equally involved in the crime market, immigrants are more likely to be arrested and face harsher punishments than natives.
2. The substitution hypothesis claims that if we consider crime as a segment of the labor market, it is natural to think that immigrants might substitute for natives (Borjas, 2003)[3].
3. Immigrants might be too few to cause a detectable change in crime. However, since my analysis is set in a high-immigration country, this explanation is less likely to be valid.

This paper explores the first of the above hypotheses. After confirming the null effect of immigration on levels, my main finding is an increase in the share of solved property offences and a converse decrease for violent crimes . Although the literature has focused on discrimination by the police or

¹Source: <https://opendata.cbs.nl//CBS/nl/dataset/82321NED/table?ts=1623513953063>

judicial system (see, for instance, Tuttle (2019)[19] and Knox Mummolo (2020)[22]), there could be other mechanisms underlying these results. Successfully (i.e. without being arrested) perpetrating an offence requires significant “criminal capital”, a large portion of which might be area and country-specific. If immigrants have less criminal capital than natives, they might be more likely to be caught when committing the same offence. The biased citizens’ perceptions about the criminal immigrant (Ajzenman et al. (2020)[20] and Couttenier et al. (2019)[16]) suggest two other possible explanations. Firstly, in response to citizens’ concerns about the rise in crime, the local police force might increase their investigating effort in types of offences more likely to be perpetrated by immigrants. Alternatively, concerned citizens might increase their collaboration with the police, increasing their witnessing or reporting efforts in response to an immigration wave.

Using a population survey carried out in the 50 largest municipalities in the Netherlands from 2010 to 2019, I analyze the effect of non-western immigration on perceptions of security and fear of victimization, finding a sizeable increase in fear of being a victim of pickpocketing. As no detectable increase is found for police or citizens-initiated contact, I cannot distinguish between the possible channels explained above. The survey results are to be interpreted with caution. The survey was only administered to the population of the 50 largest municipalities, meaning that, even though the sampling is random within the municipalities, it might not be representative of the whole Dutch population. Moreover, I find evidence of displacement, whereby immigration from outside causes internal emigration (i.e., another municipality). The few municipalities sampled also lead to issues of statistical power, as the surviving sample consists of roughly 200 observations, and the estimated confidence intervals are extremely wide.

This paper brings three contributions to the immigration and crime literature. First, I strengthen the credibility of the null crime-immigration elasticity by applying the latest developments of the shift-share literature. Secondly, I extend the available evidence to the Netherlands. Thirdly, I test the hypothesis of increased police activity. Finally, I extend the empirical literature on crime (mis)perceptions using survey data.

The structure of the paper is the following. After outlining the main findings from the literature, I explain the data source and provide some basic descriptives in the data section. Subsequently, the methodology section explains the shift-share intuition and the latest refinements to this method, followed by an exposition of the main results, a brief robustness section and the discussion.

2 Related literature

The present thesis is based on the seminal work of Becker (1968)[1], in which the decision to engage in crime is rationalized as a product of a cost-benefit analysis by the perpetrator. This intuition is particularly relevant when discussing immigration and crime: immigrants have a lower opportunity cost of offending as their legal earnings tend to be lower (because of lower education, labor market integration or skill downgrading, for instance). On the other hand, they might face a higher probability of incarceration or harsher penalties conditional on being caught, including being sent back to their origin country (in the case of illegal aliens). Thus, the effect of immigration on crime remains a positive question that must be brought to the data.

Thus far, the empirical literature on the impact of immigration on crime has produced three main results: i) the elasticity of crime rates with respect to the share of immigrants is non-distinguishable from 0; ii) access to the formal labor market reduces the propensity to engage in the crime of illegal aliens; iii) immigration increases the perceived presence of crime. In the next section, these three main findings will be reviewed in detail.

2.1 Immigration has no detectable effect on crime

Butcher and Piehl (1998)[2] employed metropolitan area statistics to investigate the relationship between immigration and crime (reported offences) in the USA. They report no detectable effect when instrumenting immigration with the initial shares by sending country in each municipality, following Altonji & Card's (1991)[10] version of the shift-share strategy. More recently, Spenkuch (2014)[6] conducted a similar analysis in the US, using county-level data from 1980 to 2000. He employs reported offences as the main outcome variable. He reports a non-significant elasticity of 0.11 for property crimes and a precise 0.01 for violent crimes.

Turning to European evidence, Bianchi et al. (2012)[4] estimated the immigration-crime relationship on a yearly panel of Italian provinces (1991-2003). They find no significant impact on reported crimes, except for robberies, when applying the shift-share strategy. Bell et al. (2013)[5] found no discernible effect of the "European wave" (immigrants coming from new European countries after the EU expansion in 2004) on notified offences in the UK, except for a negative elasticity of property crimes. Finally, Ajzenman et al. (2020)[20] found no discernible effect of Venezuelan immigration on crime employing victimization data from a yearly 2008-2017 survey, which are preferable to police data as they do not suffer from any under-reporting problem. They precisely identify an elasticity of 0, both on property and violent crimes.

2.2 Legal status has a significant crime-reducing impact

An exception to the consistent results outlined above is the section of Bell et al. (2013)[5] examining the effect on crime of asylum seekers. They identify a unity elasticity (1.09) for property crimes, which they explain by the significant formal labor market restrictions faced by asylum seekers. Quasi-experimental evidence supports this explanation. Namely, Mastrobuoni Pinotti (2015)[7] leveraged the combination of a mass pardon on 2006 and 2007 EU enlargement in a DiD setting to study the effect of legalization on recidivism in Italy. They found that legal status leads to a 50 per cent reduction in recidivism. Pinotti (2017)[9] exploited the Italian “click days”, whereby fixed quotas of residence permit are granted to sponsored immigrants applying after 8.00 AM, in a Regression Discontinuity Design, finding a 55% decrease in illegal immigrants’ crime rates. In addition, Fasani (2018)[12] found that the seven amnesties that occurred in Italy between 1986 and 2012 did not cause significant reductions in total crime rates.

In the context of the US, Freedman et al. (2018)[13] studied the impact of the IRCA legalization act (1986) and its expiration (1988) on crime rates in areas of San Antonio County more likely to be affected, namely with a higher share of illegal aliens. They found that the end of the amnesty caused a 14% increase in charges for econ-motivated crimes. In Colombia, the Permiso Especial de Permanencia (2018) - granting work permits to 500 thousand Venezuelan immigrants - did not cause a significant decrease in crimes rates, as found by Bahar et al. (2021)[25].

2.3 Immigration increases perceptions of crime

In an attempt to explain the incongruence between the null effect of immigration on crime and the high level of concern that natives have for the criminal immigrant (as shown by Bianchi et al., 2012[4]), Ajzenman et al. (2020)[20] have studied the effect of immigration on crime perceptions. Using a survey of 101 Chilean municipalities observed from 2008 to 2017, they uncover a significant relationship between immigration and self-reported measures of crime concerns. For instance, they find that a 1% increase in immigration causes a .14 percentage point rise in the aggregated measure of crime concerns. In addition, they also find an increase in crime-preventive behavior from the citizens. Hence, citizens’ concerns for criminal activities appear to strikingly rise in response to immigration.

3 Data

All the data employed in this project are retrieved from Statline², the open database operated by CBS, the Central Bureau of Statistics of the Netherlands. All data are available at the municipality level, which is the finest level of aggregation open to the public. Over time, the Netherlands has significantly reduced the number of municipalities by merging smaller areas: municipalities went from 537 in 2000 to 352 in 2021. Given that crime data was only accessible until 2018, I employed the municipality definition of that year. Therefore, I here run analysis on a panel of 355 municipalities.

I employ two measures of crime: registered (and cleared) offences and reported suspects. Registered offences – crimes recorded by the police in an official report - are available yearly from 2010 to 2019. Cleared offences are all the registered offences for which at least one suspect is apprehended. Suspects data are available from 1999 to 2015. A person is registered as a suspect when there is a reasonable suspicion of guilt for a crime. The underlying source for both datasets is the National Police Services Agency (KLPD). For the explanatory variable (share of immigrants) and the construction of the instrument, I employ municipality-level data about the stock of foreign-born by area of birth from 1996 to 2019. CBS distinguishes between western and non-western immigrants. The distinction is an economic one: the western countries of origin include Europe (Turkey excluded), North America, and Oceania, with the addition of Indonesia and Japan. The latter two are included on the basis of their socio-economic and socio-cultural position, as clearly stated in the Metadata.

For the mechanism outcomes, I rely on a municipality-level survey on feelings of safety and encounters with the police. The Safety Monitor (VM) is a biennial survey on safety, quality of life, and victimization; it is made available only for municipalities above 70.000 inhabitants – hence only 50 out of the total sample. CBS itself carries out the survey over the internet or paper on a representative sample of 65.000 citizens aged 16 or more. They ask questions about feelings of insecurity, the chance of becoming a victim and an assessment of the levels and trends of crime in the areas. The second set of questions is concerned with the frequency and cause of encounters with the police. Crucially, the citizens in the sample are asked if the contact was initiated by the police or by the citizens themselves. Finally, respondents are asked to rate the quality of their interaction with the police officers. All survey results are standardized to have mean zero and unity variance.

Bianchi, Buonanno and Pinotti (2012)[4] explain how measurement issues can be particularly severe when studying immigration and crime. Reported crimes underestimate the true – unobserv-

²<https://opendata.cbs.nl//CBS/nl/>

able – number of crimes: this can bias my econometric estimates if the extent of under-reporting is correlated with area or time-specific factors. Moreover, the official number of immigrants might underestimate the real number if there are illegal aliens. The data in the Netherlands are known to be of high quality and reliability. Moreover, the Netherlands is rarely the first European soil touched by illegal immigrants. Hence, these issues are likely to be severe than in other cases. I follow the literature, which literature has dealt with the underreporting problem by taking the natural logarithm of the crime rate and including area and year fixed effects. This approach corrects measurement issues that are constant within municipalities or across years.

Table 1: Descriptive statistics

	count	mean	sd	min	max
Population	7100	46660.62	66575.69	919	862965
% recipients of bystander income	7100	2.236758	2.362769	0	14.752
% Aged 16-25	7100	11.45358	1.586366	5.708245	22.88991
% of immigrants	7100	6.558104	4.524898	.9839292	38.20848
% of non-western immigrants	7100	3.353451	2.967855	.2480036	20.87893
Suspects per 1000	7100	12.96317	9.422257	0	54.14436
Offences per 1000	3550	45.45834	18.23388	14.25009	132.5647
% of which violent	3550	9.616654	2.62673	0	25
% of which property	3550	57.21269	6.290142	30.4878	87.5
% of which drug-related	3550	1.446242	1.262716	0	14.60317
% of cleared offences	3550	25.03762	5.658371	0	48
% had contact with police in past year	303	25.67129	4.303079	13.6	37.3
% of contacts initiated by police	297	16.92357	5.079834	4.8	31.4
% of contacts initiated by citizen	299	83.14816	5.13833	68.6	95.2
Self reported safety grade	303	7.077558	.2779263	6.4	7.9
% agreeing that crime has increased	303	13.97525	4.054523	5.2	32.1
Observations	7100				

4 Methodology

4.1 OLS regression and endogeneity concerns

My baseline specification is the following:

$$\Delta \frac{\text{Crime}_{j,t}}{\text{Pop}_{j,t}} = \alpha + \beta \frac{\text{Imm}_{j,t}}{\text{Pop}_{j,t}} + \gamma(\Delta X) + \theta_t + \epsilon_{j,t} \quad (4.1)$$

Where the LHS is the log-change in crime rate (security perceptions / police encounters / clear-up rate) every 1,000 individuals. The main explanatory variable is the log-change in the share of (non-western) immigrants in municipality j at time t . Given the log-log specification, β can naturally be interpreted as the elasticity of the crime rate with respect to the share of (non-western) immigrants. The advantages of estimating an elasticity are twofold: i) the β term can be directly compared to the findings of the literature; ii) it is a unitless variable, independent of the quantities being varied. This is important in the context of the present paper because the immigrants share varies widely across municipalities. In the vector of controls X , I include three covariates that the crime literature has identified as relevant. Firstly, population, since immigration is correlated with the population that is expected to lower the cost of committing a crime. Secondly, immigrants tend to be younger than natives, and a long strain of literature dating back to Quetelet (1831) has identified youth as a major determinant of criminal activity. Finally, I include GDP, which can be correlated with migration and have an ambiguous effect on criminal activity: either negative by improving the outside option or positive by increasing the reward to property crimes. The θ_t term captures any unobservable common shocks, such as a tightening of immigration rules or national changes in the composition of migrants. The first difference specification takes care of any municipality unobservable that is constant over time.

The specification proposed in (1) cannot be interpreted as causal, mainly for two reasons. Firstly, there is a problem of reverse causality, as migrants might choose to settle in areas where the crime rates are higher, as this might lower rents through a decrease in amenity values. Moreover, there is a problem of omitted variable bias since there is an unknown number of factors that influence the decision of the migrant to settle in a municipality rather than another. In short: as endemic to the literature on the effects of migration, we are dealing with the endogeneity of immigration. As Bartik (1991) first proposed, I will leverage the persistence of migrants' setting decisions as a source of exogeneity. The shift-share strategy is described in detail in the next section.

4.2 Shift-share solution and the exogeneity assumption

A large body of literature has employed the shift-share – or Bartik – as an instrument to estimate the causal impact of immigration. Intuitively, it relies on the fact that migrants tend to settle in areas where there is already a network of immigrants from their country of origin. Thus, any subsequent inflow from a sending country should follow a similar pattern to the distribution of migrants from that country in a reference year. The shift-share approach combines the country-level shift, the yearly inflow of migrants from each country of origin, with the initial shares of immigrants from each country of origin that settled in a specific area in t_0 . In my specific case, I instrument the log-change of the share of immigrants in each municipality with the log-change of the predicted share of immigrants, $\frac{\widehat{\text{Imm}}_{j,t}}{\widehat{\text{Pop}}_{j,t}}$, where:

$$\begin{cases} \widehat{\text{Imm}}_{j,t} = \text{Imm}_{j,t} & \text{when } t = -1 \\ \widehat{\text{Imm}}_{j,t} = \widehat{\text{Imm}}_{j,t-1} + \sum_o^0 \frac{F_{o,j,t_0}}{F_{o,t_j}} \cdot \Delta F_t & \text{when } t > -1 \end{cases} \quad (4.2)$$

Although the instrument and the instrumented variable are equal when $t = -1$ (i.e. 1996), the fact that we are estimating everything in first differences means that we are only leveraging the predicted inflow component. The initial shares, $\frac{F_{o,j,t_0}}{F_{o,t_j}}$, are calculated in 1996 – the first available year – for 52 sending countries. Not all sending countries are available and aggregations are often performed (i.e. former Yugoslavian countries are pooled together).

In order for the instrument to be valid, it has to have a strong first stage and to satisfy the exclusion restriction. The first condition is testable and verified in my case: I display the F-statistic in my 2SLS tables. The exclusion restriction – the instrument has to affect y only through x – is fundamentally untestable, and has been put under the spotlight by a pair of recent econometrics paper, namely Goldsmith-Pinkham et al. (2020)[21] and Borusyak et al. (2021)[11]³. The question that both these papers try to answer is: should you place the exclusion restriction on the *shift* or on the *shares* component of the instrument? GPSS places it on the initial shares, and thus proposes some tests to check the exogeneity of shares; BHJ contends that the *shift* should be exogenous, and thus propose to perform *shift*-level inference. Ultimately, the answer depends on the setting of the study, but the researcher should be explicit in addressing the exogeneity restriction⁴. I follow GPSS and argue that the initial shares are orthogonal to the crime rates in a given year, thus allowing the shift to be endogenous. Following Tabellini (2020)[24], I provide support for this assumption by augmenting my baseline specification with initial shares interacted with year dummies, thus testing that my results are not driven by specific groups of immigrants that settled in each municipality before 1996 and are responsible for a large share of migration in that area.

³I will refer to Goldsmith-Pinkham et al. (2020) as GPSS, and Borusyak et al. (2021) as BHJ.

⁴Per declaration of one of the authors of BHJ

There are two other developments of the shift-share literature that are worth mentioning. First, Adao et al. (2019)[15] point to an inference problem of the literature, since simulations show that the null of no effect is rejected in 55% of the randomly generated samples (it should be 5% with a 95% confidence interval). This over rejection problem arises because residuals are correlated across regions that are similar in terms of initial shares. A second issue has been highlighted in a working paper of Jaeger et al. (2018)[14]: since regions adjust slowly to immigration shocks, the β coefficient of our instrumented regression conflates short- and long-term effects of immigration. Depending on the sign of the two, β can under or overestimate the true coefficient. I adopt their double-instrumentation approach in the robustness section to disentangle short and long run effects of immigration on crime.

5 Results

5.1 Main results

The results for estimating (1) crime rates are shown in Table 1. As shown by the large F-statistic of the first stage, the relevance condition of my instrument seems to hold. The coefficients are close to 1, meaning that the migration pattern in the Netherlands follows quite strongly the initial distribution of migrants in 1996. In Table 1, I consider two measures of crime: namely, registered offences and reported suspects; the difference between the two is that the former is less likely to suffer from police bias, thus closer to the “true” measure of crime. All specifications include area-level controls and year fixed effects. All specifications are weighted by population, and standard errors are clustered at the municipality level.

Table 2: The effect of immigration on crime

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Offences						Suspects					
	All crimes		Violent		Property		All crimes		Violent		Property	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
% immigrants	-0.00269 (-0.08)	-0.0698 (-0.40)	-0.00796 (-0.11)	0.0759 (0.39)	0.0527 (0.94)	0.0625 (0.28)	0.134** (2.72)	0.164 (0.77)	0.0573 (0.68)	0.328 (1.04)	0.153 (1.8)	0.325 (1.06)
Coeff. 1st stage		0.982		0.982		0.982		1.05		1.03		1.04
F-stat 1st stage		53.30		53.30		53.30		67.75		66.26		66.84
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3195	3195	3182	3182	3195	3195	5300	5300	5234	5234	5205	5205

t statistics are in parentheses, SE are clustered at the municipality level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The results of the first six columns confirm the first result of the literature: immigration has no discernible effect on reported offences, even on property crimes, which are more likely to be affected. According to my identification strategy, even columns report the true causal effects and odd columns the biased coefficients. The combination of two-way fixed effects and controls significantly reduces the bias of a naïve regression, as the two columns are similar. The null elasticity is precisely identified and is not simply the result of the standard errors increase due to the 2SLS estimation procedure. The interpretation of the coefficient in columns (6) is that a 1% increase in the share of first-generation immigrants causes a negligible 0.063% increase in property crimes rate.

In the six rightmost columns, I measure the effect of immigration on crime operationalized as the number of suspects. Results show that there is a positive elasticity for all crimes, which means

that a 1% increase in the share of immigrants is associated with a 0.13% increase in the suspect rate. Despite the coefficients of the 2SLS estimations show a positive trend, being considerably larger than the OLS, they are not statistically significant.

Overall, the results of Table 2 point to a clear null effect of immigration on crime rate measured as offences and to a positive but non-significant effect on suspects, a measure which partly reflects police response to criminal activity.

Table 3: The effect of immigration clear up rates

	(1)		(2)		(3)		(4)		(5)		(6)	
	All crimes				Violent				Property			
	OLS	2SLS			OLS		2SLS		OLS		2SLS	
% Immigrants	0.0893	-0.219			-0.206*		-3.668**		0.660***		5.337***	
	(1.85)	(0.86)			(2.34)		(2.91)		(3.88)		(5.84)	
Coeff 1st stage		0.982					0.982				0.981	
F-stat 1st stage		53.31					53.28				53.29	
Controls	Yes	Yes			Yes		Yes		Yes		Yes	
Municipality Fes	Yes	Yes			Yes		Yes		Yes		Yes	
Observations	3193	3193			3182		3182		3185		3185	

t statistics in parentheses, SE are clustered at the municipality level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3 shows the effect of immigration on the share of cleared offences, arguably a better measure of police activity than suspects. The OLS coefficient suggests that immigration decreases the share of cleared violent crimes, while increasing the share of property crimes solved. When applying our shift-share strategy, the magnitude of the two coefficients increases by a considerable extent. The interpretation of the coefficient in (6) is that a 1% increase in the share of migrants leads to about a 5% increase in the clear-up rate for property crimes. With a baseline of 13.2%, this is an increase of 13.9% , implying a yearly increase of 31 thousand solved offences nationwide . The decrease of solved violent offence would amount to 953, taking 2018 as a reference year.

Combined with the lack of increase in registered offences, it is striking to register such a large increase (decrease) in clear-up rates for property (violent) crimes. Clearance rates are a measure of police productivity, and their increase fares well with the observation from Table 2 that the number of suspects increased – though not significantly.

5.2 Survey results

In this section, I analyze the effect of immigration from non-western countries on survey answers regarding perceptions of safety and contact with the police. The survey results are available for only the 50 largest municipalities, which leads to sampling selection issues and weak statistical power. I focus on non-western migrants because they are more likely to shift perceptions than European or North American immigrants; moreover, the instrument is not relevant for all migrants in this restricted sample of municipalities, with an F-stat often below 5.

The 2SLS results for perceptions about safety are presented in Table 4. Despite the wide confidence intervals, results are in line with Ajzenman et al. (2020)[20] regarding the sign of the effects, which is positive. Specifically, the coefficient in Column 5 can be interpreted as follows: a 1% increase in the share of non-western migrants leads to an increase in the share of people who fear being a victim of pickpocketing of 7.5 standard deviations. Two main issues could bias these result on perception: i) sample selection bias due to only large municipalities being sampled; and ii) internal displacement of concerned citizens. The former could, in principle, have a positive or negative sign, as smaller municipalities tend to be less diverse, and non-western migrants could cause a more considerable increase in feelings of insecurity (-), but smaller absolute levels of migrants might make the perceived threat smaller (+). With the data available, it is impossible to assess the size and direction of this bias. If the latter is present, citizens concerned about the crime and immigration might emigrate from municipalities with more migrants to “safer” areas, leaving unconcerned citizens to answer the survey. I find some support for this hypothesis. As shown in the Appendix, a 1% increase in the share on non-western migrants causes a .2% increase in the number of inhabitants that emigrate to another municipality in the Netherlands.

Table 4: The effect of immigration on perceptions of safety

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Safety grade	Lot of crime around	Fear victim	Sometimes unsafe	Victim Pickpocket	Harrassed streets	Harassed transport
% Nonwestern	-1.89 (1.42)	3.06 (2.21)	5.21 (3.99)	0.84 (2.29)	7.49** (4.06)	-2.74 (2.06)	2.38 (2.29)
Coeff 1st	1.37	1.37	1.00	1.37	1.37	1.37	1.37
F-stat 1st	14.51	14.51	13.31	14.51	13.65	14.51	14.51
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	200	200	181	200	188	200	200

Standard errors, clustered at the municipality level, in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In Table 6, I display the 2SLS results of survey questions regarding contact with the police. Information on which of the two parties - citizen or police – initiated the contact could help

disentangle the increase in police effort from the rise in citizens' participation in law enforcement (through witnessing, reporting etc). Contrary to expectations, none of the coefficients of interest is significant

Table 5: The effect of immigration on contact with police

	(1)	(2)	(3)	(4)	(5)
	Contact with police	% initiated by police	% initiated by citizen	Trust in police	Overall police grade
% Nonwestern	-2.32 (2.63)	5.40 (5.74)	-4.01 (5.62)	2.67 (2.44)	1.32 (2.22)
Coeff. 1st stage	1.39	1.22	1.23	1.39	1.39
F-stat 1st stage	14.30	15.77	16.60	14.30	14.30
Controls	Yes	Yes	Yes	Yes	Yes
Year Fes	Yes	Yes	Yes	Yes	Yes
Observations	200	196	198	200	200

Standard errors, clustered at the municipality level, in parentheses

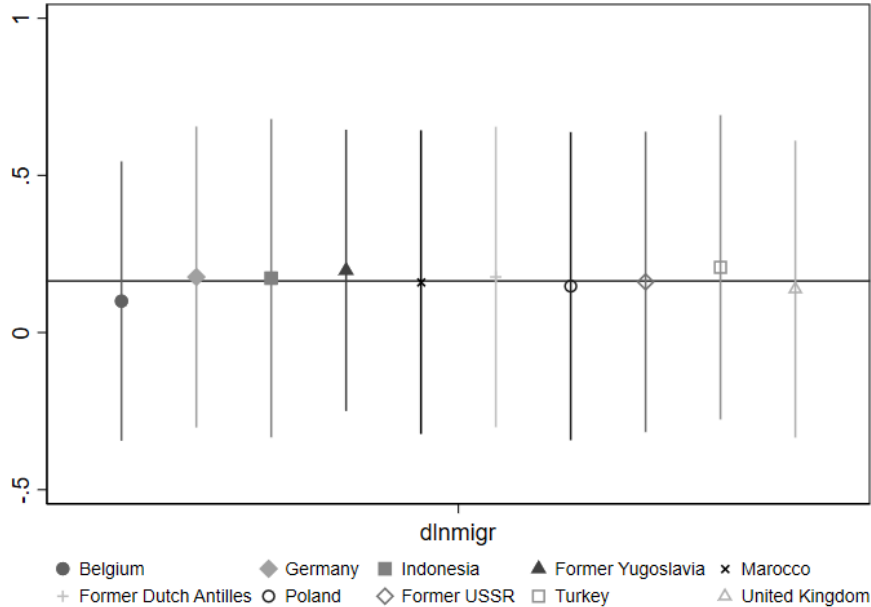
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

6 Robustness

As explained in the Methodology section, the shift-share strategy requires the validity of the exogeneity assumption, which I place on the share component of the instrument. This means that my outcome of interest, crime rates, should not be correlated with the initial shares. This could happen if certain types of migrants settled in areas with different criminal activity trends. Tabellini (2020)[24] proposes to test this assumption by augmenting the baseline specification with the initial shares (for the main sending countries) interacted with year dummies. If the estimated elasticities are not different from that estimated in the “short” regressions, we can be confident that exogeneity holds. I run the following specification:

$$\Delta \frac{\text{Crime}_{j,t}}{\text{Pop}_{j,t}} = \alpha + \beta \frac{\text{Imm}_{j,t}}{\text{Pop}_{j,t}} + \gamma(\Delta X) + \zeta_1 \frac{F_{o,j,t_0}}{F_{o,t_j}} \cdot \text{year}_1 + \zeta_2 \frac{F_{o,j,t_0}}{F_{o,t_j}} \cdot \text{year}_2 + \dots + \zeta_n \frac{F_{o,j,t_0}}{F_{o,t_j}} \cdot \text{year}_n + \theta_t + \epsilon_{j,t} + \quad (6.1)$$

Figure 2 displays the 2SLS coefficients (and 95% confidence intervals) of specification (2) for the main sending countries, where the outcome variable is crime measured as suspects, and the horizontal line shows the coefficient of Column 8 in Table 2. Reassuringly, none of the coefficients is different from the baseline, which supports the exogeneity assumption placed on initial shares.



The effect of immigration on crime fits well in the slow-adjusting labor market framework

proposed by Jaeger et al. (2018)[14]. They propose a sluggish labor market model, in which natives' wages adjust slowly to labor shocks (such as an inflow of immigrants): hence, the *share* part instrument can be correlated with demand shocks preceding t_0 . Moreover, they propose a double instrumentation procedure, in which the lagged share of migrants is added as a regressor, and the predicted stock based on lagged inflows serves as an instrument. I have two endogenous variables with two instruments:

$$\Delta \frac{\text{Imm}_{j,t}}{\text{Pop}_{j,t}} = \alpha + \Delta \frac{\widehat{\text{Imm}}_{j,t}}{\text{Pop}_{j,t}} + \Delta \frac{\widehat{\text{Imm}}_{j,t-5}}{\text{Pop}_{j,t}} + u \quad (6.2)$$

$$\Delta \frac{\text{Imm}_{j,t-5}}{\text{Pop}_{j,t}} = \alpha + \Delta \frac{\widehat{\text{Imm}}_{j,t}}{\text{Pop}_{j,t}} + \Delta \frac{\widehat{\text{Imm}}_{j,t-5}}{\text{Pop}_{j,t}} + u \quad (6.3)$$

This methodology also allows disentangling long-term (5 years) from short-term (1 year) effects of immigration on crime. We would expect the two to have the opposite sign: in the short run, immigration might increase the crime rate as migrants struggle to enter the legal labor market, but in the long run, they improve the local economic situation, thus increasing the opportunity cost of crime. Table 7 shows the results of performing this empirical exercise.

Table 6: Jaeger et al. (2018) double instrumentation

	(1)	(2)	(3)	(4)	(5)	(6)
	Registered offences			Suspects		
	All	Property	Violent	All	Property	Violent
2SLS						
% Immigrants	0.0191 (0.344)	0.199 (0.414)	0.262 (0.426)	-0.116 (0.307)	0.117 (0.548)	-0.471 (0.503)
L5.% Immigrants	-0.287 (0.565)	-0.429 (0.710)	-0.602 (0.811)	-0.176 (0.582)	-0.573 -1,046	0.450 (0.904)
First Stage						
Kleibergen-Paap LM statistic	6.65	6.65	6.57	6.55	6.47	6.59
<i>A. Current Stock</i>						
Predicted stock	0.46 (0.12)	0.46 (0.12)	0.46 (0.13)	0.47 (0.13)	0.45 (0.13)	0.46 (0.13)
Lagged predicted stock	1.09 (0.21)	1.09 (0.21)	1.09 (0.21)	1.09 (0.21)	1.09 (0.21)	1.09 (0.21)
F	32.95	32.95	32.90	32.92	32.72	32.76
<i>B. Lagged stock</i>						
Predicted stock	-0.15 (0.12)	-0.15 (0.12)	-0.15 (0.13)	-0.15 (0.13)	-0.15 (0.13)	-0.15 (0.13)
Lagged predicted stock	1.02 (0.22)	1.02 (0.22)	1.02 (0.22)	1.02 (0.23)	1.02 (0.23)	1.02 (0.23)
F	14.83	14.83	14.85	14.85	14.76	14.72
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3195	3195	3182	3184	3146	3149

Standard errors, clustered at the municipality level, in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The coefficients of interest are never significant, suggesting that the lack of effect found in Table 2 is not due to a conflation between positive (short term) and negative (long term) effects of immigration on crime. As Jaeger et al.(2018)[14] prescribed, I report the Kleibergen-Paap statistic, which tests the under-identification of the joint instruments. The null hypothesis (of under-identification) is rejected in all cases, and the instruments check the first stage requirements as shown by the F statistics.

7 Conclusions

In the first part of my analysis, I employed yearly data on registered offences in 355 Dutch municipalities to estimate the causal impact of immigration on crime. Based on the existing literature, I employed a shift-share instrumental variable strategy, leveraging the persistence of migrants' settling decision. My main result, even when accounting for the latest developments of the econometric literature, is that the elasticity of immigration with respect to crime is not different from 0.

From the above finding, a question emerges, namely: how can we conciliate the null effect of immigration on crime with the large overrepresentation of immigrants in the prison population of the Netherlands? I find that the clear-up rate of property crimes increases considerably, which means that police productivity increases considerably for economically motivated crimes. This could partly explain why migrants, more likely to be involved in property crimes, are prevalent to such a great extent in the prison population.

Following up, the question remains of why property crimes clear-up rates show an increase. I suggest that the increased (mis)perceptions about crime dynamics might explain this phenomenon, as I found that fear of being a victim of pickpocketing significantly increases because of immigration. In response to citizens' beliefs, local police agencies might shift investigating violent crimes to property crimes. Alternatively, citizens might increase their interaction with the police to apprehend immigrant offenders. Survey data on the interaction with the police do not allow any conclusion on which of the two options above is in place, as no significant effects are found for police or citizens-initiated contact.

The biggest limitation of this study is that it rests on the assumption that immigrants are proportionally more involved in economically motivated crimes. This assumption could be rigorously tested by analyzing individual data on the nationality of the offender. Moreover, more research is needed to establish if my finding is robust to alternative types of police productivity, such as time taken to conclude investigations or emergency response time. The sample selection issues strongly limit the survey results. Hence, it would be of interest to extend the survey to the whole country to better understand the impact of immigration on safety perceptions and contact with the police.

The scientific implication of my work is that a researcher should take into account the reactions of the police force when attempting to estimate the effect of immigration on crime rates. From a policy perspective, there is no such thing as an increase in crime reducing the net welfare gain from immigration.

References

- [1] Gary S Becker. “Crime and punishment: An economic approach”. In: *The economic dimensions of crime*. Springer, 1968, pp. 13–68.
- [2] Kristin F Butcher and Anne Morrison Piehl. “Cross-city evidence on the relationship between immigration and crime”. In: *Journal of Policy Analysis and Management: The Journal of the Association for Public Policy Analysis and Management* 17.3 (1998), pp. 457–493.
- [3] George J Borjas. “The labor demand curve is downward sloping: Reexamining the impact of immigration on the labor market”. In: *The quarterly journal of economics* 118.4 (2003), pp. 1335–1374.
- [4] Milo Bianchi, Paolo Buonanno, and Paolo Pinotti. “Do immigrants cause crime?” In: *Journal of the European Economic Association* 10.6 (2012), pp. 1318–1347.
- [5] Brian Bell, Francesco Fasani, and Stephen Machin. “Crime and immigration: Evidence from large immigrant waves”. In: *Review of Economics and statistics* 21.3 (2013), pp. 1278–1290.
- [6] Jörg L Spenkuch. “Understanding the impact of immigration on crime”. In: *American law and economics review* 16.1 (2014), pp. 177–219.
- [7] Giovanni Mastrobuoni and Paolo Pinotti. “Legal status and the criminal activity of immigrants”. In: *American Economic Journal: Applied Economics* 7.2 (2015), pp. 175–206.
- [8] Martin Halla, Alexander F Wagner, and Josef Zweimüller. “Immigration and voting for the far right”. In: *Journal of the European Economic Association* 15.6 (2017), pp. 1341–1385.
- [9] Paolo Pinotti. “Clicking on heaven’s door: The effect of immigrant legalization on crime”. In: *American Economic Review* 107.1 (2017), pp. 138–68.
- [10] Joseph G Altonji and David Card. *The effects of immigration on the labor market outcomes of less-skilled natives*. Routledge, 2018.
- [11] Kirill Borusyak, Peter Hull, and Xavier Jaravel. *Quasi-experimental shift-share research designs*. Tech. rep. National Bureau of Economic Research, 2018.
- [12] Francesco Fasani. “Immigrant crime and legal status: Evidence from repeated amnesty programs”. In: *Journal of economic geography* 18.4 (2018), pp. 887–914.
- [13] Matthew Freedman, Emily Owens, and Sarah Bohn. “Immigration, employment opportunities, and criminal behavior”. In: *American Economic Journal: Economic Policy* 10.2 (2018), pp. 117–51.
- [14] David A Jaeger, Joakim Ruist, and Jan Stuhler. *Shift-share instruments and the impact of immigration*. Tech. rep. National Bureau of Economic Research, 2018.

- [15] Rodrigo Adao, Michal Kolesár, and Eduardo Morales. “Shift-share designs: Theory and inference”. In: *The Quarterly Journal of Economics* 134.4 (2019), pp. 1949–2010.
- [16] Mathieu Couttenier et al. “The logic of fear-populism and media coverage of immigrant crimes”. In: (2019).
- [17] Christian Dustmann, Kristine Vasiljeva, and Anna Piil Damm. “Refugee migration and electoral outcomes”. In: *The Review of Economic Studies* 86.5 (2019), pp. 2035–2091.
- [18] Anthony Edo et al. “Immigration and electoral support for the far-left and the far-right”. In: *European Economic Review* 115 (2019), pp. 99–143.
- [19] Cody Tuttle. “Racial disparities in federal sentencing: Evidence from drug mandatory minimums”. In: *Available at SSRN 3080463* (2019).
- [20] Nicolás Ajzenman, Patricio Dominguez-Rivera, and Raimundo Undurraga. “Immigration, Crime, and Crime (Mis) Perceptions”. In: *Available at SSRN* (2020).
- [21] Paul Goldsmith-Pinkham, Isaac Sorkin, and Henry Swift. “Bartik instruments: What, when, why, and how”. In: *American Economic Review* 110.8 (2020), pp. 2586–2624.
- [22] Dean Knox and Jonathan Mummolo. “Making inferences about racial disparities in police violence”. In: *Proceedings of the National Academy of Sciences* 117.3 (2020), pp. 1261–1262.
- [23] Stelios Roupakias and Michael Chletsos. “Immigration and far-right voting: evidence from Greece”. In: *The Annals of Regional Science* 65.3 (2020), pp. 591–617.
- [24] Marco Tabellini. “Gifts of the immigrants, woes of the natives: Lessons from the age of mass migration”. In: *The Review of Economic Studies* 87.1 (2020), pp. 454–486.
- [25] Dany Bahar, Ana Maria Ibáñez, and Sandra V Rozo. “Give me your tired and your poor: Impact of a large-scale amnesty program for undocumented refugees”. In: *Journal of Development Economics* 151 (2021), p. 102652.

Appendix

The effect of non-western immigration

The richness of Dutch data allows to analyze the effect of the group of non-western immigrants on crime. Non-western immigrants are arguably less integrated in the labor market, hence we would expect them to have a larger effect on crime rate.

Table 7: The effect of non-western immigration on crime

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Offences						Suspects					
	All crimes		Violent		Property		All crimes		Violent		Property	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
% immigrants	0.003	-0.038	-0.001	0.041	0.035	0.034	0.091**	0.157	0.004	0.313	0.073	0.312
	(0.0214)	(0.092)	(0.044)	(0.189)	(0.029)	(0.127)	(0.032)	(0.278)	(0.055)	(0.477)	(0.056)	(0.483)
Coeff. 1st stage		1.802		1.804		1.802		1.094		1.085		1.085
F-stat 1st stage		181.95		181.65		181.95		71.56		70.83		70.69
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3195	3195	3182	3182	3195	3195	5300	5300	5234	5234	5205	5205

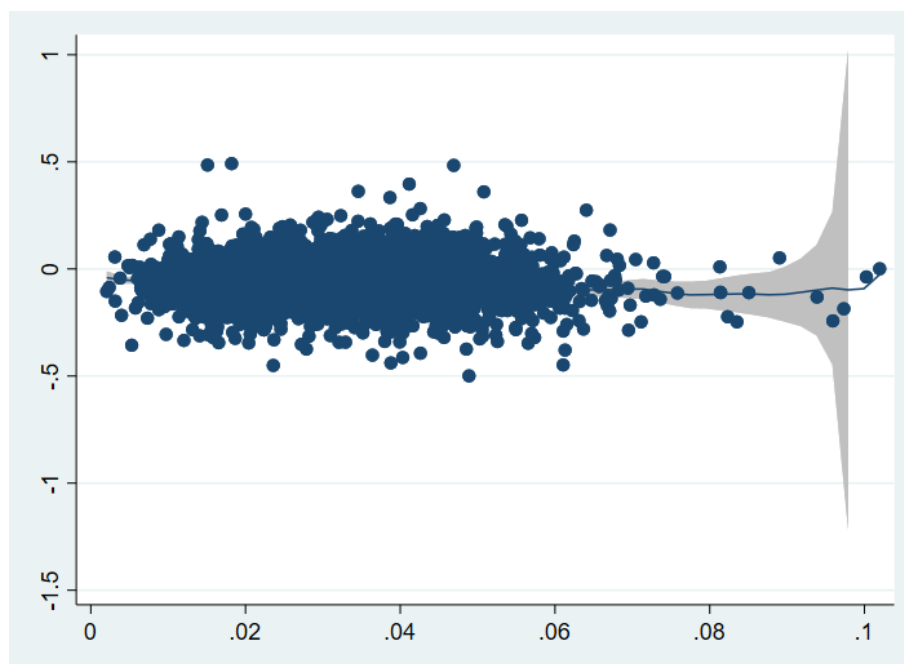
t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The effects are quantitatively and qualitatively not different from those of all migrants. The fact that there is very little illegal immigration, and the Dutch labor market is highly formal, might play a role in explaining this conclusion.

Non-linearities

The third hypothesis advanced in the Introduction as to why immigration might not increase crime rates is that migrants are "too few to matter". I test if there is a non-linear relationship between immigrants and crime with the SEMIPAR command of Stata 15, based on Robinson's (1988) estimator. Figure x displays the reduced-form between the log-change in the rate of registered offences and the instrument, accounting for the usual controls. A formal test rejects any higher order fit, confirming the visual intuition.



Displacement

The survey results might suffer from a downward bias due to displacement. If concerned citizens react to immigration "with their feet" by moving to another municipality, we might be underestimating the true effect of immigration on perceptions and contact with the police. Using data on movements to and from municipalities *within* the Netherlands, I find that there is indeed evidence of displacement. The coefficient in column 4 implies that a 1% increase in the share of non-western immigrants lead to a .2% increase in the number of citizens moving to another municipality.

Crime composition

The change in clear-up rates that we observed in the Results section could be partly explained by a change in the composition of crimes. There is, reassuringly, no significant change in the composition of offences over the observed period.

Table 8: The effect of non-western immigration on internal migration

	(1)	(2)	(3)	(4)
	Immigration		Emigration	
	OLS	2SLS	OLS	2SLS
% Non-western-immigrants	-0.008 (0.019)	0.077 (0.103)	0.386*** (0.028)	0.195** (0.074)
Coeff 1st stage		1.532		1.532
F-stat 1st stage		96.55		96.55
Controls	Yes	Yes	Yes	Yes
Municipality Fes	Yes	Yes	Yes	Yes
Observations	5325	5325	5325	5325

SE in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9: The effect of non-western immigration on crime composition

	(1)	(2)	(3)	(4)
	% Violent		%Property	
	OLS	2SLS	OLS	2SLS
% Non-western-immigrants	0.032 (0.019)	0.072 (0.046)	-0.004 (0.043)	0.079 (0.104)
Coeff 1st stage		1.802		1.804
F-stat 1st stage		86.14		86.30
Controls	Yes	Yes	Yes	Yes
Municipality Fes	Yes	Yes	Yes	Yes
Observations	3195	3195	3195	3195

SE in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$