

# The Dilution Problem in Observational Studies on the Relationship Between Immigration and Crime

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July 2, 2025

## Abstract

This research note critically reevaluates one of the most common empirical strategies to study the effect of immigration on crime – namely, regressing changes in aggregate crime rates on exogenous shifts in local immigrant shares. I show that, because immigrants typically comprise only a small fraction of the population, even large crime-rate differences between immigrants and natives are mechanically diluted. As a result, null findings from such designs are predetermined and reveal little to no information about immigrant-native crime differentials. I derive a closed-form expression for the *minimum detectable gap* – the smallest immigrant–native crime difference these regressions can identify – and then use Monte Carlo simulations, calibrated to real-world immigration and crime data, to demonstrate that conventional designs only achieve adequate statistical power with implausibly large crime differentials and extreme immigration shocks.

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# 1 Introduction

The purported connection between immigration and crime has become one of the most contentious and salient topics in contemporary political discourse. Recent surveys indicate that a majority of Americans believe that immigration increases crime ([Pew Research Center, 2024](#)). Similarly, across Western Europe, substantial majorities in Germany (81%), Spain (80%), Sweden (73%), Italy (71%), and the United Kingdom (71%) consider immigration over the past decade to have been “too high,” with concerns about crime frequently cited among the primary reasons for their opposition ([YouGov, 2019, 2025](#)). Populist and radical-right parties on both sides of the Atlantic have exploited and amplified these perceptions, advocating for stricter border controls and more restrictive asylum policies ([Alizade, 2024](#)). The politicization of the immigration-crime nexus fuels anti-immigrant sentiment and political polarization across Western democracies ([Schneider-Strawczynski and Valette, 2025](#)).

Against this background, a growing body of research examines the link between immigration and crime empirically. One of the most common empirical strategies to study the relationship between immigration and crime is to aggregate offences to a geographic unit – counties, municipalities, or policing districts – and then regress changes in a given unit’s aggregate crime rates on (plausibly exogenous) changes in the same unit’s immigrant share. Variants of this design underpin many of the most highly cited contributions to this literature (see [Table 1](#)).

In this research note, I argue that such designs are fundamentally limited in their ability to inform the immigration-crime debate due to what I call the “dilution problem.” Because immigrant populations typically represent a small share of the total population, their specific crime rates (be they higher, lower, or equivalent to native rates) are arithmetically diluted within aggregate crime statistics. This issue is often amplified because many studies examine over-time changes in immigrant populations, and these incremental changes are generally even smaller than the already modest immigrant population stocks. Consequently, studies

Table 1: Overview of published research on the effects of immigration on crime

<b>Study</b>	<b>Journal</b>	<b>Finding</b>
Bianchi, Buonanno and Pinotti (2012)	<i>Journal of the European Economic Association</i>	Null
Bell, Fasani and Machin (2013)	<i>Review of Economics and Statistics</i>	Mixed
Chalfin (2014)	<i>American Law and Economics Review</i>	Null
Spenkuch (2013)	<i>American Law and Economics Review</i>	Null
Masterson and Yasenov (2021)	<i>American Political Science Review</i>	Null
Gehrsitz and Ungerer (2022)	<i>Economica</i>	Positive
Kayaoglu (2022)	<i>World Development</i>	Null
Ajzenman, Dominguez and Undurraga (2023)	<i>American Economic Journal: Applied Economics</i>	Null
Maghularia and Uebelmesser (2023)	<i>Journal of Economic Behavior &amp; Organization</i>	Null
Severino and Visconti (2024)	<i>Journal of Politics</i>	Null
Lange and Sommerfeld (2024)	<i>Labour Economics</i>	Mixed
Marie and Pinotti (2024)	<i>Journal of Economic Perspectives</i>	Null

*Notes:* The table lists studies examining the relationship between immigration inflows and aggregate crime rates. Effects on total crime reported by Lange and Sommerfeld (2024) are not statistically significant at conventional levels and are therefore classified as “mixed”.

adopting this empirical approach are systematically underpowered. Specifically, when the analytical goal is to estimate group-level differences – namely, the immigrant–native crime gap – such designs possess sufficient statistical power only if this gap reaches an implausibly large magnitude.<sup>1</sup>

I develop the argument in two steps. First, I formalize the dilution problem and derive a closed-form expression for the *minimum detectable gap* in immigrant vs. native per-capita crime rates that any design following the structure outlined above can uncover at conventional levels of significance. Second, I calibrate a Monte-Carlo simulation to real-world immigration shocks and crime data across German counties between 2014 and 2016, i.e. before and after the large-scale refugee influx in 2015. The simulation demonstrates that conventional research designs lose statistical power extremely quickly: only under conditions of exceptionally large immigration shocks (around 1 percentage point, comparable to Germany’s post-2015 refugee inflow), coupled with immigrant crime rates at least 15 times higher than natives, can these designs reliably detect differences and reject the null hypothesis. Under more typical immigration scenarios (e.g., increases of approximately 0.25 percentage points), statistical power collapses to near-zero, making it practically impossible to identify even extreme immigrant-native crime differentials.

Crucially, this paper takes no position on whether immigrants are more, less, or equally prone to crime than natives. Immigrants are a highly heterogeneous population, and any group-level differences in offending will hinge on the demographic mix of a given inflow, especially age, gender, and prior exposure to violence (Falk et al., 2014; Couttenier et al., 2019). The argument is purely epistemic: I scrutinize whether the designs commonly used in this literature are capable of shedding light on immigrant–native crime gaps, and show that – because of mechanical dilution – they generally are not.

The dilution problem identified in this paper is broadly related to, but distinct from,

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<sup>1</sup>Designs using aggregate-level crime statistics may be useful when testing a targeted spillover mechanism – the effect of immigration on *native* offending – since they capture the overall response to variation in immigrant share. I revisit this point in the Discussion section.

the ecological inference literature. Whereas most EI research concentrates on *bias* – and methods for correcting it – when drawing group-level inferences from aggregate data (King, Tanner and Rosen, 2004), my approach sets bias aside and instead highlights a different, arithmetic constraint: the dilution of group effects within large populations. In this respect, the analysis is most closely aligned with the “bounding” tradition in ecological inference (Duncan and Davis, 1953), which derives the feasible range of group-specific averages from observed aggregates. When the key parameter of interest relates to a small fraction of the population, these bounds become so wide as to be practically uninformative. While this limitation is well understood in theory, it remains a persistent and largely overlooked feature in studies of immigration and crime, affecting the results of much of the published research in this field (Table 1).

## 2 The Dilution Problem

I start by deriving a general expression for the minimum detectable gap – the smallest per-capita crime differential between immigrants and natives that any consistent estimator of the slope in an aggregate-crime-on-immigrant-share regression could identify (see equation 2). I then employ a Monte Carlo simulation, calibrated to actual refugee inflows and county-level crime data, to illustrate the lack of statistical power of this empirical approach.

## 2.1 Set-up

Let  $i = 1, \dots, n$  index geographic units and  $t = 0, \dots, T$  time. For each  $(i, t)$  we observe

$N_{it}$  total population,

$I_{it}$  number of immigrants,

$S_{it} = \frac{I_{it}}{N_{it}}$  immigrant share,

$C_{it}$  total reported crimes,

$y_{it} = \frac{C_{it}}{N_{it}}$  crimes per capita.

Let  $c_N$  and  $c_I$  denote per-capita crime propensities for natives and immigrants, and define the immigrant–native crime gap

$$r \equiv c_I - c_N.$$

Aggregate crime is given by:

$$y_{it} = c_N + r S_{it} + \varepsilon_{it}, \tag{1}$$

where  $\varepsilon_{it}$  captures shocks orthogonal to immigration.

## 2.2 The minimum detectable gap

Consider any consistent estimator  $\hat{\beta}$  of the slope in a regression of  $y_{it}$  on  $S_{it}$  – whether cross-sectional OLS, including unit and time fixed effects, difference-in-differences, instrumental variables, or other variants.<sup>2</sup> Assuming that causal identification assumptions<sup>3</sup> hold in a

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<sup>2</sup>Alternative specifications, such as regressing total crime counts on the immigrant population while controlling for population size, do not solve the fundamental issue. Because this counts-on-counts model is simply a rescaling of the rate-on-share specification, it inherits the same signal-to-noise problem: the variance of total crimes is dominated by offences committed by the much larger native population.

<sup>3</sup>Throughout, it is assumed that immigration does not affect native crime propensities (see footnote 1 and Discussion section). It is also assumed that all identification assumptions in a given setting hold, so that  $\hat{\beta}$  is unbiased. Crucially, to the extent that the identification assumptions do not hold, such designs can yield spuriously significant estimates despite having virtually no power – a likely explanation for the subset of such studies that report statistically significant effects (see Table 1).

given setting,  $\hat{\beta}$  identifies the immigrant–native crime gap:

$$\mathbb{E}[\hat{\beta}] = r.$$

If immigrants and natives offend at the same rate ( $r = 0$ ), the expected slope is zero, so immigration-driven shifts in population composition, on average, leave the area-level crime rate unchanged. By contrast, a positive (negative)  $r$  implies that, on average, each additional percentage-point of immigrant share raises (lowers) the crime rate by exactly  $r$  crimes per capita.

For a two-sided test of  $H_0: r = 0$  at significance level  $\alpha$ , the null is rejected when

$$|\hat{\beta}| \geq z_{1-\alpha/2} \text{se}(\hat{\beta}),$$

The smallest immigrant-native crime gap that could trigger rejection is thus:

$$\boxed{r_{\text{MD}} = z_{1-\alpha/2} \text{se}(\hat{\beta})}. \tag{2}$$

Equation (2) is general: any design following the structure outlined above is informative about immigrant–native crime differences only if its standard error is small relative to plausible values of  $r$ .

I demonstrate in the SI (section A) that this dilution problem affects published research. Specifically, I apply the analytical framework developed above to a study by [Masterson and Yasenov \(2021\)](#) published in the *APSR*. The authors examine the effects of a reduction in refugee resettlement to the United States on county-level crime rates, leveraging a difference-in-differences design. They find no impact on either property or violent crime. However, applying equation (2) shows that the empirical design employed by the authors could only have rejected the null hypothesis if refugees committed crime at roughly **16**× (property)

or  $44\times$  (violent) the native rate.<sup>4</sup> The study’s null findings, therefore, most likely reflect a mechanical consequence of dilution rather than evidence that refugees and natives offend at comparable rates.

### 3 Monte-Carlo simulation

Figure 1 illustrates the practical implications of the dilution problem through a Monte-Carlo simulation. Here, I assume a first-differences design (pre/post an immigration shock). The model parameters are calibrated using German county-level data on refugee inflows and average crime rates in 2014 and 2016. The heatmap visualizes statistical power across a wide range of hypothetical immigration shocks (x-axis) and varying immigrant–native crime ratios (y-axis). The results underscore that conventional designs are severely underpowered except at the extremes: statistical power only surpasses the standard 80% threshold if the immigration shock is both extremely large – on the order of Germany’s post-2015 refugee inflow ( $\approx 1\%$  on average) – and accompanied by immigrant crime propensities that are extraordinarily higher (at least 15 times) than the native rate. For smaller, more typical shocks (e.g., a 0.25 percentage-point increase), power remains effectively zero irrespective of immigrant crime rates. Thus, under realistic conditions, the approach is virtually incapable of detecting even large immigrant–native crime differentials, and is likely to yield null results regardless of true group-level differences in offending behavior.

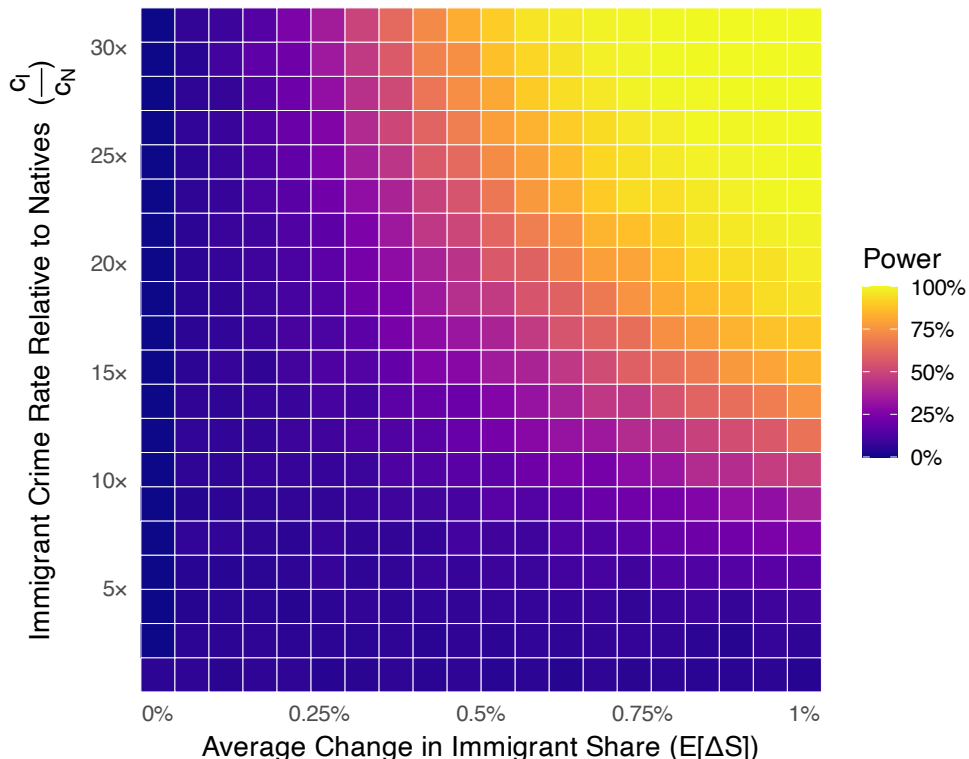
### 4 Discussion

The dilution problem documented in this paper implies that designs using aggregate crime statistics and small changes in immigrant population shares are almost certain to produce “null” results, regardless of whether immigrants are more, less, or equally prone to crime. To

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<sup>4</sup>It is important to note that this assumes that all identification assumptions (parallel trends in particular) hold.

Figure 1: Statistical Power Heatmap



*Notes.*— The heatmap shows the statistical power for a two-sided test (significance level  $\alpha = 0.05$ ) of the hypothesis  $H_0 : \beta = 0$  in the first-difference model  $\Delta y_i = \alpha + \beta \Delta S_i + u_i$ . The horizontal axis represents the average county-level change in the refugee share,  $E[\Delta S_i]$ , varying from 0 to 1 percentage point. The vertical axis represents the immigrant–native crime rate ratio ( $c_I/c_N$ ). Power is estimated at each grid point using 1,000 Monte Carlo simulations calibrated to German county data (2014–2016). Statistical power is the share of simulations in which  $|\hat{\beta}|/\text{se}(\hat{\beta}) > 1.96$ , i.e. the null  $H_0 : \beta = 0$  is rejected.

the extent that the causal identification assumptions hold in a given study, the results are largely mechanically predetermined. Consequently, researchers aiming to study the effect of immigration on crime should leverage more granular data. This includes, for example, country-of-origin-specific crime rates (e.g. [Couttenier et al., 2019](#); [Dehos, 2021](#)), or individual-level data (e.g. [Butcher and Piehl, 1998](#); [Sampson, Morenoff and Raudenbush, 2005](#); [Pinotti, 2017](#); [Freedman, Owens and Bohn, 2018](#); [Abramitzky et al., 2024](#)).

Aggregate crime statistics may still hold value for addressing specific, narrowly defined questions, provided researchers are transparent about their analytical goals and which specific mechanisms they aim to test. For instance, if the primary goal is to investigate potential

spillover effects – that is, how immigration might influence offending patterns among the native population (see e.g. [Borjas, Grogger and Hanson, 2010](#)), then changes in aggregate crime rates can be a relevant and useful outcome. In such cases, researchers should explicitly articulate that they are testing this spillover mechanism, but that the design reveals little to no information about the immigrant-native crime differential itself. A second, more direct application where aggregate data can be informative is when the outcome of interest is crime directed against immigrants themselves, e.g., xenophobic hate crimes ([Müller and Schwarz, 2021](#)). In such cases, the local immigrant share serves as a direct and plausible explanatory variable for offences committed by natives, for which the design can provide sufficient statistical power. Beyond these specific applications where the research question is precisely tailored to what aggregate data might reveal, regressions of overall crime rates on immigrant shares teach us little about the relationship between immigration and crime.

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## AI Use Disclosure

Artificial intelligence was used for code review and language editing.

# The Dilution Problem in Observational Studies on the Relationship Between Immigration and Crime

Supporting Information

## Contents

[A Application to Masterson & Yasenov \(2021\)](#)

1

## A Application to Masterson & Yasenov (2021)

In this section, I illustrate the dilution problem by applying the analytical framework presented in the main paper to a study by [Masterson and Yasenov \(2021\)](#), published in the *American Political Science Review*. The authors study the effects of a reduction in U.S. refugee resettlement on county-level crime rates between 2010–2018 using a difference-in-differences approach. They find no statistically significant impact on either property or violent crime rates.

To demonstrate how dilution undercuts the statistical power of the design, I compute the *minimum detectable immigrant–native crime gap* ( $r_{\text{MD}}$ ), as derived in equation (2). Specifically:

- **Standard errors** ( $\text{se}(\hat{\beta})$ ): These values are taken directly from the first-difference estimates in Masterson and Yasenov (2021, Table 1, columns 1 and 3):
  - Property crime:  $\text{se}(\hat{\beta}) = 185.996$
  - Violent crime:  $\text{se}(\hat{\beta}) = 81.784$
- **Scaling to per-refugee crime rates**: Each regression coefficient ( $\hat{\beta}$ ) is expressed in *crimes per 100 000 residents* per one additional *refugee per 100 residents*. To obtain the minimum detectable gap in *crimes per refugee*, rescale by  $100/100\,000 = 1/1,000$ . Thus:
  - Property crime:  $r_{\text{MD}}^{\text{prop}} = 1.96 \times 185.996/1,000 \approx 0.365$
  - Violent crime:  $r_{\text{MD}}^{\text{viol}} = 1.96 \times 81.784/1,000 \approx 0.160$
- **Baseline crime propensities** ( $c_N$ ): Non-refugee crime rates<sup>5</sup> prior to the policy shock are taken from the [FBI \(2016\)](#) crime reports for the pre-treatment year 2015:

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<sup>5</sup>Refugees accounted for only about 267,222/320.6 million  $\approx 0.08\%$  of the U.S. population in 2015, so their inclusion in these aggregate crime rates is practically negligible.

- Property crime:  $c_N^{\text{prop}} = 2,487$  per 100,000 people (= 0.0249 per person)
- Violent crime:  $c_N^{\text{viol}} = 372.6$  per 100,000 people (= 0.00373 per person)

Table A.2 summarizes these results, showing how large the immigrant–native crime differential would need to be for the design of Masterson and Yasenov (2021) to reject the null hypothesis.

Table A.2: Minimum Detectable Crime Differentials in Masterson and Yasenov (2021)

<b>Outcome</b>	$\text{SE}(\hat{\beta})^{\text{a}}$	$r_{\text{MD}}^{\text{b}}$	Baseline $c_N^{\text{c}}$	Min. Ratio $c_I/c_N^{\text{d}}$
Property	185.996	0.365	0.0249	15.7
Violent	81.784	0.160	0.00373	43.9

<sup>a</sup> Standard errors for first-difference estimates from Masterson and Yasenov (2021, Table 1, cols. 1 and 3).

<sup>b</sup>  $r_{\text{MD}}$  (crimes per refugee / year) is computed as  $1.96 \times \text{SE}(\hat{\beta})/1000$ .

<sup>c</sup> Baseline crime rates ( $c_N$ ) from FBI (2016)

<sup>d</sup> “Min. Ratio” is the smallest *total* immigrant-to-native crime rate ratio  $c_I/c_N = 1 + r_{\text{MD}}/c_N$  that the design could detect at the 5 % significance level.