Immigrants, Legal Status, and Illegal Trade

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First version: May 2020

This version: February 2024

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Abstract

Nearly $2 trillion of illegally trafficked goods flow across international borders every year, generating violence and other social costs along the way. Due to the absence of legal contracts and the challenge of finding trading partners in an illegal market, traffickers may rely on co-ethnic networks to facilitate trade. In this paper, I use novel microdata on the universe of large illegal drug confiscations in Spain to provide the first causal estimates of how immigrants and immigration policy affect the pattern and scale of illegal drug trafficking. I find that immigrants increase both illegal drugs imported from and exported to their origin country, with irregular immigrants raising illegal drug imports. Doubling the number of immigrants from an origin country raises the likelihood of illegal drug imports from that country by 8 percentage points. I find suggestive evidence that granting legal status to immigrants reduces illegal drug imports.

JEL Codes: F14, F22, K42, J15.

*brett.mccully@carloalberto.org. I am extremely grateful to Jonathan Vogel, Pablo Fajgelbaum, Felipe Goncalves, Randall Kuhn, Adriana Lleras-Muney, and Emily Weisburst for advice and encouragement. I thank Treb Allen, Ashna Arora, Wookun Kim, Brian Kovak, Giovanni Mastrobuoni, Melanie Morten, and Vidhya Soundararajan for helpful discussions, and participants at the Virtual Crime Economics Seminar (ViCE), European Winter Meetings of the Econometric Society, the NBER Summer Institute for International Trade and Investment, European Trade Study Group, Italian Society of Economics Annual Meeting, the Southern Economic Association Annual Meeting, DemSemX Virtual Conference, the Online Crime Economics Graduate Student Seminar, APPAM Fall Research Conference, Empirical Investigations in International Trade conference, Networks Conference at INET, the Population Association of America, Economic Graduate Student Conference, and seminar participants at Collegio Carlo Alberto, Bocconi University, UCLA, George Washington University, the OCC, CFPB, and USITC. I owe special thanks to Ariadna Jou for assistance with contacting the Spanish government, and to Remi Boivin for help with the UNODC data. I acknowledge financial support from CCPR's Population Research Infrastructure Grant P2C from NICHD: P2C-HD041022, CCPR's Population Research Training Grants T32 from NICHD: T32-HD007545, and the Institute on Global Conflict and Cooperation. All errors are my own.

1 Introduction

The War on Drugs has been characterized by intensive police interventions in the drug market, with $100 billion per year spent on drug law enforcement worldwide (Rolles et al., 2016). Despite this rigorous enforcement, the prices of illegal drugs have remained low while drug use remains widespread (Storti and Grauwe, 2009). Consequently, some jurisdictions have sought alternative approaches, such as diverting drug users to rehabilitation programs (Mueller-Smith and Schnepel, 2021). However, these policy changes focus only on the retail side of the drug market—dealers selling to users. They therefore ignore the source of the drugs: the wholesale side of the market, including international drug trafficking, which in the United States is responsible for bringing in 82 percent of domestically consumed drugs (Atkinson, 2020). Moreover, because the determinants of international drug trafficking are poorly understood, policymakers are left without sufficient information to craft alternative policies.[1]

In this paper, I provide the first estimates of the effect of immigrants on illegal drug trafficking, and the role of immigration policy as a tool to combat the illegal drug trade. A large literature suggests that immigrants are key facilitators of international trade, taking advantage of social connections to their origin country (Peri and Requena-Silvente, 2010; Combes et al., 2005; Head and Ries, 1998). These same social connections may also facilitate illegal trade, though no quantitative evidence exists. Moreover, the Becker-Ehrlich model of crime (Becker, 1968; Ehrlich, 1973) suggests that the diminished earnings prospects available to unauthorized immigrants will result in a higher propensity to participate in financially motivated illegal activities, such as trafficking illegal goods. Understanding how immigrants and immigrant legal status affect drug trafficking are critical for informing the current debate on immigration policies.[2]

I use novel data on the universe of large drug confiscations to estimate the causal effect of immigrants and immigrant legal status on illegal trafficking. I make two key findings. First, I estimate that immigrants raise illegal drug trafficking flows between their origin and host countries. I find suggestive evidence pointing towards two mechanisms which drive the immigrant-drug trafficking relationship: that immigrants reduce bilateral trade costs, and that immigrant exposure to drug trafficking in their country of origin raises the likelihood of

[1] To the extent that we know anything about the determinants of international drug trafficking, we know that policing which focuses on drug interdiction is ineffective. For example, Mejia and Restrepo (2016) estimate that the cost of intercepting the marginal gram of cocaine coming to the United States from Colombia is $175,000, about a thousand times higher than the retail price. Castillo et al. (2020) show that major drug confiscations can increase violence further along trafficking routes.

[2] The Trump administration, for example, claimed Mexican immigrants brought drugs to the U.S. while opposing any path to legal status for unauthorized immigrants.

participating in trafficking in the immigrants' host country. My second key finding is that granting immigrants legal status reduces illegal drug imports, consistent with the Becker-Ehrlich model of crime.

Credibly establishing a causal relationship between immigrants and drug trafficking is challenging for two reasons. First, the illegal nature of trafficking makes measurement difficult. Second, other factors (such as geography) may affect both the distribution of immigrant populations and illegal drug trafficking.

To make progress on measuring illegal drug trafficking, I leverage uniquely detailed data on drug confiscations. In particular, I use a database of individual drug confiscation events as a proxy for actual drug flows in the context of Spain, a country with high-quality reporting of data on drug confiscations. These data report where each drug confiscation occurred within Spain, from which country the drugs were trafficked, and to which country the drugs were intended to be trafficked, thus providing insight into the region-to-region flows of illegal drug trafficking. To validate that this indirect measure captures variation in actual flows of illegal goods, I compare confiscations to survey-based measures of drug use and availability. I find that more confiscations correspond to more drug use and availability. The context, data, and validation are discussed in Section 2.

To identify the causal impact of immigrants on drug trafficking, I estimate a gravity equation, the workhorse empirical model in the international trade literature used to explain the volume of trade flowing from one region to another (Tinbergen, 1962; Head and Mayer, 2014). I estimate a gravity equation of illegal drug trafficking, relating the likelihood or value of drug trafficking between a given foreign country and a given Spanish province with the number of immigrants from that country living in the province.

The rich set of fixed effects afforded by the gravity equation allows me to control for unobserved heterogeneity that may bias my estimates, including unobserved variation in policing enforcement. As in many studies of the economics of illegal behavior, I rely on official records based on enforcement actions carried out by police to proxy for true illegal activity (Pinotti, 2020). However, the rich variation across origins and destinations in the gravity model allows me to control for policing enforcement intensity at both the immigrants' nationality and province level, which has not been feasible in prior studies on immigrants and crime based on regional variation.

There may still be factors at the country-province pair level which drive both drug trafficking and immigration between the country and the province. For example, Moroccan immigrants and Moroccan drug traffickers may be drawn to the province of Barcelona for its familiar Mediterranean climate. To address this potential endogeneity, I adopt the instrumental variables approach developed by Burchardi et al. (2019) to generate exogenous variation in the number of immigrants from a given country living in a given Spanish province. The instrument relies on the intuition that immigrants from, for example, Morocco, are likely to settle in Barcelona if many Moroccan immigrants are arriving in Spanish provinces outside Barcelona at the same time that many non-African immigrants are settling in Barcelona. In particular, the instrument interacts the "pull" of Spanish province d to immigrants—measured as the share of immigrants in a given decade settling in d—with the "push" to emigrate from origin country o—measured as the number of immigrants from o entering Spain in a given decade.

I find that a higher immigrant population from a given origin country facilitates the import and export of illegal drugs from and to that origin country. For an average Spanish province, I find that a 10% increase relative to the mean in the number of immigrants from a given origin country raises the likelihood that illegal drugs trafficked from that origin country will be confiscated locally by 0.8 percentage points. Similarly, a 10% increase in the number of immigrants relative to the mean from a given origin country raises the likelihood that drugs intended for export to the immigrants' origin country will be confiscated locally by 0.3 percentage points. I discuss the baseline gravity estimation and results in Section 3.

I find suggestive evidence for two mechanisms which drive my results, discussed in Section 4. First, my results are consistent with immigrants reducing bilateral trade costs since immigrants raise illegal drug exports. These baseline results are consistent with the extant qualitative evidence that immigrants reduce information frictions and transaction costs for illegal imports and exports via their social connections. Second, I find that greater immigrant exposure to drug trafficking in their origin country corresponds to greater trafficking between their host and origin countries. Finally, I rule out that immigrants' preferences can explain my results.

Turning to immigration policy, I find that immigrant legal status crucially mediates the effect that immigrants have on illegal trafficking, as discussed in Section 5. To understand the role of legal status, I estimate the effect of immigrants on drug trafficking separately by immigrant legal status using the gravity specification. I find that my baseline estimates for imports are driven primarily by irregular immigrants, consistent with the Becker-Ehrlich model of crime. However, regular immigrants drive the estimated baseline effect for exports to the immigrants' origin country. The effect of regular immigrants on exports results from the fact that Spain's primary export destinations for illegal drugs are countries within the European Union, and all E.U. immigrants to Spain automatically have

regular status. Moreover, with no border checks within the E.U., I suggest that it is only profitable for drug trafficking organizations to export from Spain to destinations within the E.U. rather than having to cross a customs border again where police confiscations are likely.

These results suggest that countries face a tradeoff when legalizing immigrants between illegal imports and exports, but only when trade and migration costs are low between the country and immigrants' origin countries.

To better understand the effect of legal status on trafficking, I exploit a major immigrant regularization program implemented in 2005. This program resulted in roughly half a million immigrants receiving legal status (Monras et al., 2021). I find that the 2005 mass immigrant regularization reduced the likelihood of illegal drug importation significantly. I calculate that legalizing 10% of the irregular immigrant population from a given origin country would reduce the likelihood of illegal drug imports from that country by 1.4 percentage points. I find no effect of the regularization program on illegal drug exports.

Literature and Contribution. This paper makes a novel contribution to the literature on illegal drug trafficking, what causes it and what policies most effectively combat it. In particular, this paper provides the first causally identified estimates of the effect of immigrants and immigrant legal status on illegal trafficking. Economists have thus far been completely absent from the nascent literature studying this question, despite its policy importance. Instead, one can find a few papers by criminologists Berlusconi et al. (2017), Giommoni et al. (2017), and Aziani et al. (2021), who use cross-country data on drug confiscations to assess how immigrant populations correlate with drug confiscations. However, these papers use significantly more aggregated data than I do, do not address the endogeneity of migration and drug trafficking, and say little about underlying mechanisms.

Further, a number of studies look at the causes and consequences of the illegal drug trade. For example, Abadie et al. (2014) and Mejía et al. (2017) examine the effects of law enforcement crackdowns on regional drug cultivation while Castillo et al. (2020) and Dell (2015) estimate how crackdowns affect drug violence. Each of these studies uses regionally-aggregated measures of drug trafficking. By contrast, I use data measuring region-to-region drug trafficking, which allows me to study the bilateral determinants of trafficking (such as immigrants).

This article also contributes to the debates on the costs and benefits of immigration and on which immigration policies host countries should adopt. Much of the literature on the consequences of immigration focuses on labor market outcomes.[3] An emerging set of research papers studies how immigrants affect local crime. Prior research on immigration and crime tends to focus on the labor market opportunities available to immigrants (Bell et al., 2013; Spenkuch, 2014; Pinotti, 2017; Freedman et al., 2018). I demonstrate the first causal link

[3]See, for example, Monras (2020), Dustmann et al. (2013), Ottaviano and Peri (2012), Borjas (2003), and Card (2001). For a recent review of the literature, see Dustmann et al. (2016).

between international migration and international crime and explore the policy implications. I further show that exposure to crime in immigrants' origin country may affect immigrants likelihood to participate in crime in their host country, consistent with Couttenier et al. (2019).

This paper also contributes to our understanding of what factors shape trade costs. Trade costs are hard to rationalize based on observables, such as transportation costs and tariffs (Anderson and Van Wincoop, 2004). Information frictions in trade are one key factor that can account for a sizable portion of trade costs (Allen, 2014; Chaney, 2014; Startz, 2021). Such frictions in international illegal goods markets are likely to be particularly salient, given the challenges inherent in finding import or export partners for an illegal substance where above-ground search and matching is made impossible by intensive policing. Surprisingly, then, there is mixed evidence about immigrants raising trade even though their social networks (i.e., having friends, relatives, and professional contacts back in their origin country) should help to overcome information frictions. For example, Burchardi et al. (2019) find no effect of immigrants on trade flows while others such as Parsons and Vézina (2018), Peri and Requena-Silvente (2010), and Combes et al. (2005) find a positive effect. However, legal trade has many non-information frictions, such as tarrifs, customs inspections, international contracting and trade credit. Therefore, illegal trade provides a useful context to test the importance of information frictions, where such frictions are disproportionately large.

2 Background and Measurement of Drug Trafficking

2.1 Background

Illegal Drugs. Cocaine and cannabis are among the most commonly consumed illegal drugs in the world (p.7, UNODC, 2020c). Spain is a key entry point for much of the cocaine and cannabis consumed in the European illegal drug market.[4]

Illegal drugs typically pass through many countries between the production location and the final consumption location. Cocaine, for example, is grown almost exclusively in three countries in the world: Colombia, Peru, and Bolivia. Cocaine typically passes through intermediary regions—or entrepót—such as Mexico, Spain, or West Africa on the way to consumer markets in the United States and Europe (p. 30, UNODC, 2020b).

Cannabis, by contrast, "is produced in almost all countries worldwide," and is therefore less traded across regions (p. 67, UNODC, 2020b). Nevertheless, a substantial amount of cannabis is still trafficked across international borders (p. 71-73, UNODC, 2020b).

[4] See https://www.emcdda.europa.eu/countries/drug-reports/2019/spain/drug-markets_en.

In Spain, confiscations of domestic cannabis plants are quite small compared to the amount of cannabis confiscated arriving from abroad (Alvarez et al., 2016).

International drug trafficking is characterized by the prevalence of transnational criminal organizations, including in Spain. Still, there are many competing groups, with limited monopolization (OCP, 2015).

Immigration. Spain has experienced a tremendous amount of immigration in recent decades. Between 1991 and 2011, the share of immigrants in Spain's population rose from about 2 percent to over 13 percent, as shown in Appendix Figure D.2, representing "the highest rate of growth of the foreign-born population over a short period observed in any OECD country since the Second World War" (OECD, 2010).

Irregularity is a common feature of immigration in Spain. Irregular immigrants are defined as those living in the country without a residency permit, and they usually enter Spain through legal means (González-Enríquez, 2009). They include immigrants who overstay their tourist visas and remain in Spain beyond the terms of their temporary residence permits.[5] I discuss irregular immigration in Spain in greater detail in Section 5.1.

2.2 Drug Trafficking Data Description

Data limitations typically complicate the study of illegal activity. In this study, I use data on confiscations of illegal drugs by law enforcement to proxy for actual illegal drug flows. To validate that drug confiscations capture variation in actual flows of illegal drugs, I compare confiscations to survey-based measures of drug availability at the province level.

I use a database of nearly 10,000 individual drug confiscation events to proxy for actual drug flows in the context of Spain, a country with high-quality reporting of drug confiscations.[6] Using enforcement-based measures as a proxy for illegal and therefore hard-to-observe activity is typical in the study of crime (Dell, 2015; Dube et al., 2016). Prior economics studies, however, have relied on region-level measures of drug enforcement. In contrast, this study is the first in economics to leverage region-to-region measures of drug trafficking.

The database of confiscation events is compiled by the United Nations Office of Drugs and Crime (UNODC) and includes only large drug confiscations. An observation in these data is a single drug confiscation event, which provides details on the drug type, amount confiscated, the country from which the drugs were trafficked, the country to which the drugs

[5] Irregular immigrants who enter Spain via either crossing the Strait of Gibraltar by boat or by illegally entering the Spanish North African cities of Ceuta or Mellila are a small fraction of irregular immigrants, though they garner a disproportionate share of press coverage (González-Enríquez, 2009).

[6] In Appendix A I discuss how Spanish law enforcement constructs the data, my cleaning procedures for these data, and compare Spain's data to that of other countries.

were intended to be trafficked, and the location of the confiscation. By including both the locality of a confiscation and its country of departure or intended destination, I observe the bilateral flow for each confiscation event.

I focus on Spain due to the high quality of reported confiscations data. While other countries also report drug confiscations data to the UNODC, reporting tends to be less comprehensive and more irregular outside Spain (see Appendix Figure A.1). For example, Spain reports the country of origin for nearly 70 percent of confiscations, whereas the average across all other countries is just 3 percent of confiscations.

Country of origin and intended destination for each drug confiscation in the dataset are assigned based on subsequent police investigation, where country of origin refers to the most recent foreign country the drugs had been in (not necessarily the country in which they were produced). I detail how country of origin and intended destination and ascertained in the data in Appendix A.

To transform quantities confiscated into dollar amounts, I use illegal drug prices reported by the Spanish Ministry of the Interior.[7]

Four facts emerge when looking at the data on confiscations in Spain. First, nearly all drugs confiscated by Spanish authorities are cocaine or cannabis, with negligible amounts of amphetamines and heroin as shown in Appendix Figure D.1. For this reason, I restrict the subsequent analysis to cocaine and cannabis confiscations.[8]

Second, the distribution of drug confiscation amounts is right skewed as shown in Appendix Figure D.4, with many moderate-sized confiscations (the median confiscation value is \$48,283) and a few huge confiscations (the mean confiscation value is \$509,571). Third, Spain imports cannabis largely from Morocco and cocaine from Latin America,[9] as shown in Appendix Figure D.5, and Spain exports drugs primarily to the rest of Europe and the Mediterranean region, as seen in Appendix Figure D.6. Importantly, a variety of Latin American countries export cocaine to Spain, providing useful variation for the subsequent empirical estimation. Finally, there is substantial spatial variation across Spain in the intensity of drug trafficking overall, as shown in Appendix Figure D.7, as well as by drug type, as shwon in Appendix Figure D.8.

[7]Specifically, these are prices in dollars for 2012 for heroin, cocaine, amphetamines, and cannabis as reported by Spain's the Centre of Intelligence Against Organized Crime to the UNODC.

[8]My baseline results do not change qualitatively or in magnitude when including heroin and amphetamines, as shown in Appendix Table D.5.

[9]I show in Appendix Section C.6.2 that my baseline results are robust to dropping Morocco and Latin America.

2.3 Drugs Data Validation Exercise

In this section I provide evidence suggesting that the drug confiscations data are a valid proxy for actual illicit drug flows. In particular, I correlate confiscations of imported drugs per capita (net of confiscations destined for other countries) in a locality to the availability of drugs in that locality. This approach is valid if local production is small relative to the local market, an assumption likely to hold in Spain as discussed in Section 2.1.

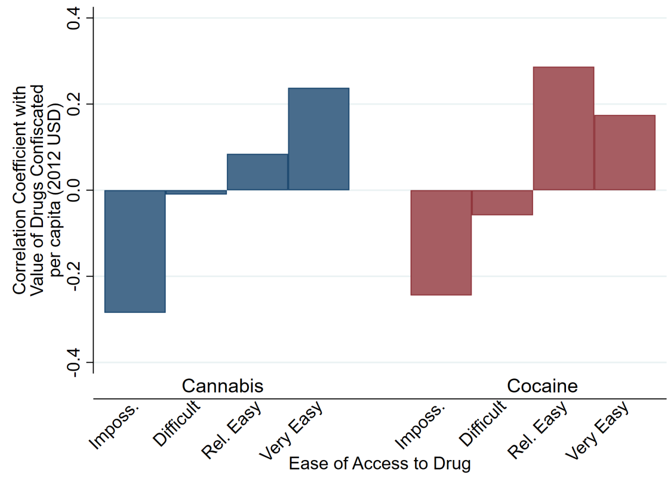
To measure local drug availability, I turn to the Survey on Alcohol and Drugs in Spain (EDADES). The EDADES is a nationally representative biennial survey on substance use in Spain, interviewing 20,000 to 30,000 residents per survey. Respondents are asked how easy it is for them to access various illegal drugs within 24 hours and how much of a problem illegal drugs are in their neighborhood. I aggregate responses across the 2011, 2013, and 2015 survey rounds to create a measure of province-level drug use and drug availability consistent with the baseline estimation period of 2011 to 2016 used in Section 3.

I find that confiscations of illegal drugs positively correlate with a wide range of measures of local drug availability. In Figure 1, I plot the correlation coefficient between the reported ease of obtaining a particular drug within 24 hours and the amount of that drug which was confiscated in the province per capita between 2011 and 2016. Consistent with confiscations corresponding to real flows of illicit drugs, I find that when a higher proportion of respondents say it is "impossible" to obtain a particular drug, the amount of that drug confiscated per capita in the province tends to be lower. Conversely, I find that the proportion of respondents saying it is "easy" or "very easy" to obtain a drug correlates positively with the amount of that drug confiscated in the province.

In Figure 2 I plot the correlation coefficients of additional measures of local drug availability and consumption with the value of confiscations per capita across all illicit drugs. In the first bar of the figure, the local drug availability measure is the fraction of respondents answering "very" to the question, "Thinking about where you live, how important of a problem do you think illegal drugs are?" For the remainder of the bars, the drug availability measures are the fraction of respondents who report seeing various drug behaviors exhibited by others in their neighborhood.[10] Across survey questions, confiscations vary positively with local drug availability.

[10] Respondents are asked how often in their neighborhood they see people (i) drugged and on the ground, (ii) inhaling drugs in paper or aluminium, (iii) injecting drugs, (iv) selling drugs, (v) smoking joints, (vi) snorting drugs by nose, and (vii) leaving syringes lying on the ground.

Figure 1: Correlation of Drug Confiscations to Drug Availability by Drug



Notes: This figure shows Pearson correlation coefficients between the amount of confiscations per capita of a particular drug (cannabis on the left and cocaine to the right) and the fraction of respondents in a province who report finding it impossible/difficult/relatively easy/very easy to obtain that drug within 24 hours. Correlations are estimated on a cross-section of 52 Spanish provinces, averaged across 2011 to 2016 for drug confiscations and across the 2011, 2013, and 2015 waves of the Survey on Alcohol and Drugs in Spain (EDADES).

2.4 Immigration Data

To measure bilateral immigrant populations and inflows, I use the decennial Spanish census. I measure the number of immigrants from an origin country by counting the number of individuals with citizenship from that origin country.[11] In the case of dual citizens, the non-Spanish country of citizenship is reported.

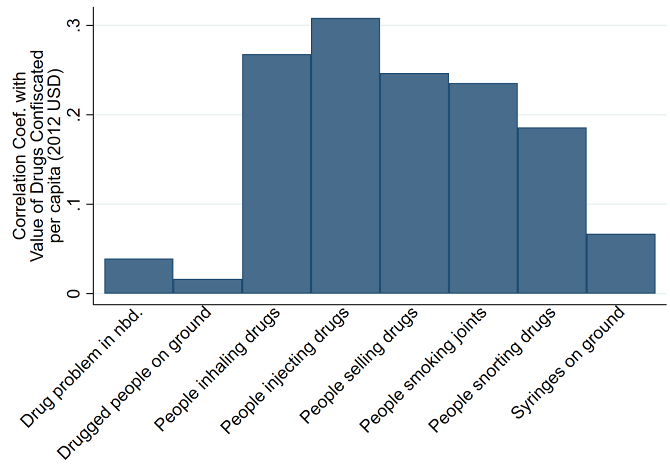
To construct the instrumental variable, described in Section 3.2, I also use data from the Spanish Census on immigrant inflows. To measure bilateral inflows for each decade, I count the number of immigrants who arrived from the origin country and reside in the Spanish province within the last 10 years.

Because the set of immigrant origin countries reported varies across census waves, I aggregate countries into groups consistent across both the 2001 and 2011 Spanish Censuses. In both waves I observe 102 individual origin countries, and group remaining countries by continent into country groups (e.g., "Other countries, Africa"). In total, I exploit variation across 107 origin regions.[12]

[11]In unreported regressions I find that changing the definition of immigrant to be based on country of birth has virtually no qualitative or quantitative effect on my results.

[12]In unreported regressions, my results are robust to dropping the 5 residual country groups from the estimation.

Figure 2: Correlation of Drug Confiscations to Measures of Local Drug Availability



Notes: This figure plots Pearson correlation coefficients between illegal drug confiscations (measured in dollars) per capita across all drugs and the fraction of respondents in the province who reported observing the listed drug-related behaviors either "frequently" or "very frequently" or, for the first bar on the left, "very." The behaviors listed are, from left to right: (i) "Thinking about where you live, how important of a problem do you think illegal drugs are?"; (ii) "How often in your neighborhood are there drugged people on the ground?"; (iii) "How often in your neighborhood are there people inhaling drugs in paper/aluminium?"; (iv) "How often in your neighborhood are there people injecting drugs?"; (v) "How often in your neighborhood are there people selling drugs?"; (vi) "How often in your neighborhood are there people smoking joints?"; (vii) "How often in your neighborhood are there people snorting drugs by nose?"; (viii) "How often in your neighborhood are there syringes lying on the ground?" I drop cannabis from the drug confiscation variable in the correlations for the questions on people snorting or injecting drugs or syringes being on the ground, since cannabis is generally not snorted or injected. Correlations are estimated on a cross-section of 52 Spanish provinces, averaged across 2011 to 2016 for drug confiscations and across the 2011, 2013, and 2015 waves of the EDADES (Survey on Alcohol and Drugs in Spain).

3 Bilateral Empirical Analysis

I first seek to understand whether immigrants facilitate drug trafficking between their origin and home region. To do so, I relate drugs coming from a given origin country and confiscated locally to the number of immigrants from that origin country and living locally. Exploiting this country-province-pair level variation, I can flexibly control for observed and unobserved characteristics of the country and the province. Because migration and drug trafficking may be jointly determined by other factors, such as geographic or climatic similarity between country and province, I generate exogenous variation in the immigrant population using an instrumental variable strategy.

3.1 Gravity Regression

The two-dimensional nature of my data allows me to flexibly control for origin- and destination-specific characteristics which may shape trafficking and migration. This estimation strategy also allows me to deal with concerns about enforcement intensity variation at the regional- or country-level driving observed drug confiscations, an improvement on the existing literature relating immigration to crime which typically relies on cross-region variation.

I estimate a baseline gravity equation of the form

$$Y_{o,d}^{2011-2016} = \alpha_o + \alpha_d + \beta M_{o,d}^{2011} + \delta \ln(Dist_{o,d}) + \varepsilon_{o,d} \tag{1}$$

where α_o and α_d are country and province fixed effects, respectively; $Y_{o,d}$ is either a dummy for whether any drug imports from o into d were confiscated between 2011 and 2016 or a dummy for whether any intended export from province d to country o were confiscated between 2011 and 2016; and $Dist_{o,d}$ is the distance in kilometers between o and d taken from Peri and Requena-Silvente (2010). $M_{o,d}^{2011}$ is a measure of the number of immigrants from o living in d, defined as the log of one plus the number of immigrants in d from o, measured in thousands (I provide justification for my functional form choice as well as robustness to alternatives in Appendix Section C.10). The error term $\varepsilon_{o,d}$ includes all omitted bilateral forces which may shape drug trafficking. I measure the immigrant population $M_{o,d}^{2011}$ using the 2011 Spanish Census distributed by the Minnesota Population Center (2019) as described in Section 2.4.

The origin country and destination province fixed effects are key to my identification strategy. The origin fixed effect α_o controls for, among other factors, the economic development, institutions, and crime in the origin country as well as national-level policies of Spain vis-a-vis origin country o. These country-pair level policies can include visa regimes, customs regulations, and national law enforcement cooperation. Similarly, the province fixed effect α_d controls for province d factors common across origins, such as province d's police force strength and the economic conditions in d. For example, if the labor market in Barcelona attracts immigrants and raises the demand for cocaine, α_d will absorb such variation to the extent that it is constant across origin countries. Thus β is identified from variation in drug confiscations and immigrant populations across country-province pairs.

I cluster standard errors at the origin country-level in my baseline specification, though my results are robust to alternative standard error choices (see Appendix Table C.12).

I estimate equation 1 separately for import confiscations and export confiscations. To measure intended exports, I consider drugs confiscated in d but which were intended to go to country o.[13] I show the summary statistics for the relevant variables in Table D.1.

3.2 Instrumental Variables Approach

While country and province fixed effects absorb many potential confounders in my baseline specification, there may still be unobserved factors at the country-province-pair level, such as the geographic or climatic similarity between a foreign country and a Spanish province. Consider, for example, that Morocco shares a similar Mediterranean climate with Barcelona. Suppose this similar climate is preferred by Moroccan immigrants, who are then more likely to settle in Barcelona. If Moroccan narcotraffickers are more skilled at piloting boats in the Mediterranean climate, then that similar climate may also drive Moroccan drug traffickers to Barcelona. Hence similar climate is a country-province-specific confounder which may drive both immigration and drug trafficking.

To obtain variation in country-to-province-specific immigration that is exogenous to such concerns, I follow Burchardi et al. (2019) and develop a set of leave-out push-pull instruments for the number of immigrants arriving in a given region and coming from a given origin country.[14] These instruments produce plausibly exogenous variation in bilateral immigrant inflows. I use two decades of inflows between 1991 and 2011 to predict the 2011 population size of immigrants from a given origin country living in a Spanish province.

The intuition of the instrument is that a social connection, in this case an immigration decision, between an origin and a destination is likely to occur when the origin is sending many immigrants at the same time the destination is pulling in many immigrants. For example, suppose we want to predict the number of Moroccans settling in the province of Barcelona. To do so, I look at the number of Moroccans flowing into Spain and the number of immigrants from all origin countries settling in Barcelona for the same decade. In particular, the instrument will predict Moroccans to settle in Barcelona if large numbers of immigrants from other countries are also settling there. Similarly, if many immigrants from other origins are settling in Barcelona, then an immigrant arriving from Morocco will be predicted to

[13] In the data, I observe substantially more confiscations on the import margin than I do on the export margin. In particular, the value of import confiscations is nearly 6 times larger than that of export confiscations between 2011 and 2016. This is likely due to the greater difficulty in law enforcement's ability to pin down the intended destination relative to the country of trafficking origin. In addition, country's law enforcement priority is typically on preventing entry of illegal drugs (and their local consumption) rather than their exit.

[14] This approach also bears resemblance to Sequeira et al. (2020), who generate exogenous variation in local immigrant populations by interacting the inflows of immigrants to the U.S. (push) and the locations of new railroads in the U.S. (pull). Note that because the push-pull instrument does not rely on variation in the lagged size of ethnic enclaves, it uses different variation than the popular shift-share instrument of Card (2001). Card's (2001) ethnic enclave instrument is typically applied to regional regressions, in contrast to this study's region-by-region-level estimation.

settle in Barcelona.

Concretely, the migration leave-out push-pull instrument interacts the arrival into Spain of immigrants from origin country o (push) with the attractiveness of different destinations to immigrants (pull) measured by the fraction of all immigrants to Spain who choose to settle in province d. A simple version of the instrument is defined as

$$\tilde{IV}^D_{o,d} = I^D_o \times \frac{I^D_d}{I^D},$$

where I^D_o is the number of immigrants from origin o coming to Spain in decade D, and I^D_d/I^D is the fraction of immigrants to Spain who choose to settle in province d in decade D.

Still, there may be threats to the exogeneity of the instrument as defined thus far. One potential exclusion restriction violation occurs when endogenous bilateral immigration is a large share of the instrument's components. For example, if all Moroccan immigrants coming to Spain choose to settle in Barcelona due to its similar climate, then the instrument will include climate similarity in its prediction of bilateral immigration. A simple solution then is to leave out bilateral immigration ($I^D_{o,d}$) when computing the instrument.

However, there might also be spatial correlation in confounding variables. For example, both Moroccan and Algerian immigrants and drug traffickers may go to Barcelona for the same reason: a similar climate. Then, even leaving out Morocco-to-Barcelona immigration flows when computing the instrument is not sufficient, because now the Algerian immigration flows to Barcelona (used to predict Morocco-to-Barcelona flows) are contaminated with the confounding climate preference.

To avoid such endogeneity, I again follow Burchardi et al. (2019) and leave out both the continent of origin country o and the autonomous community (the highest-level administrative unit in Spain) of province d to construct the instrumental variable that I use in my baseline estimation:

$$IV^D_{o,d} = I^D_{o,-a(d)} \times \frac{I^D_{-c(o),d}}{I^D_{-c(o)}} \tag{2}$$

where $a(d)$ is the set of provinces in the autonomous community of d, and $c(o)$ is the set of countries on o's continent. Therefore, $I^D_{o,-a(d)}$ is the number of immigrants from o settling in Spain outside the autonomous community of province d in decade D, and $I^D_{-c(o),d}/I^D_{-c(o)}$ is the fraction of immigrants to Spain from outside of the continent of o who choose to settle in province d. In our running Morocco-Barcelona example, the instrument interacts the number of Moroccan immigrants settling outside Catalonia ($I^D_{o,-a(d)}$) with the fraction of non-African immigrants arriving in Spain who choose to settle in Barcelona ($I^D_{-c(o),d}/I^D_{-c(o)}$).

One advantage of the leave-out structure of the instrumental variables is that it neatly deals with concerns over reverse causality. For example, drug trafficking organizations may send workers from an origin country to migrate to the Spanish provinces into which they hope to traffick drugs. However, these bilateral flows, as well as any historical bilateral flows, are not used for the prediction of the bilateral immigrant population.

The identification assumption is that any confounding factors which make a given province more attractive for both immigration and drug trafficking with a given country do not simultaneously affect the interaction of (i) the settlement of immigrants from other continents with (ii) the total number of immigrants arriving from the same country but settling in a different autonomous community. A violation may occur if, say, immigrants skilled at drug trafficking from Morocco tend to settle in the province of Barcelona and immigrants skilled in drug trafficking from Lebanon settle in Alicante (Barcelona and Alicante are in different autonomous communities) in the same decade and for the same reason: a preference for the familiar Mediterranean climate. This violation is only quantitatively meaningful if Moroccans are a large fraction of immigrants settling in Barcelona, and if Lebanese immigrants are a large fraction of the immigrants settling in Alicante. I empirically test the plausibility for such a violation to drive my baseline results in Appendix Section C.1. To do so, I leave out countries or provinces with correlated immigrant populations from the construction of the instrument, and conclude that such correlation between migration location choices across provinces in different autonomous communities or across countries from different continents is unlikely to significantly affect my results.[15]

In order to account for spillovers in immigration flows between decades and potential nonlinearities, I include second-order interaction and squared terms for the instruments, which allow me to better predict the nonlinear immigrant population measure that I use.[16] Nevertheless, my baseline results are robust to more parsimonious sets of instruments as shown in Appendix Section C.1.

In Appendix Section C.2 I document the variation that underlies the push-pull instrument. In particular, I show both time-series variation and cross-sectional variation in immigrant location decisions in Appendix Figure C.1. I do so for all inflows for each decade in the top two maps. To get a sense of the underlying cross-sectional variation, I plot the location decisions of immigrants as of 2011 for a selection of countries in the bottom four maps of Appendix Figure C.1. Finally, in Appendix Section C.1.3, I demonstrate robustness using additional decades of immigrant inflows to construct the instrument, although this comes at

[15] I provide additional discussion of the identification provided by the instrument conditional on the set of country and province fixed effects in Appendix Section B.

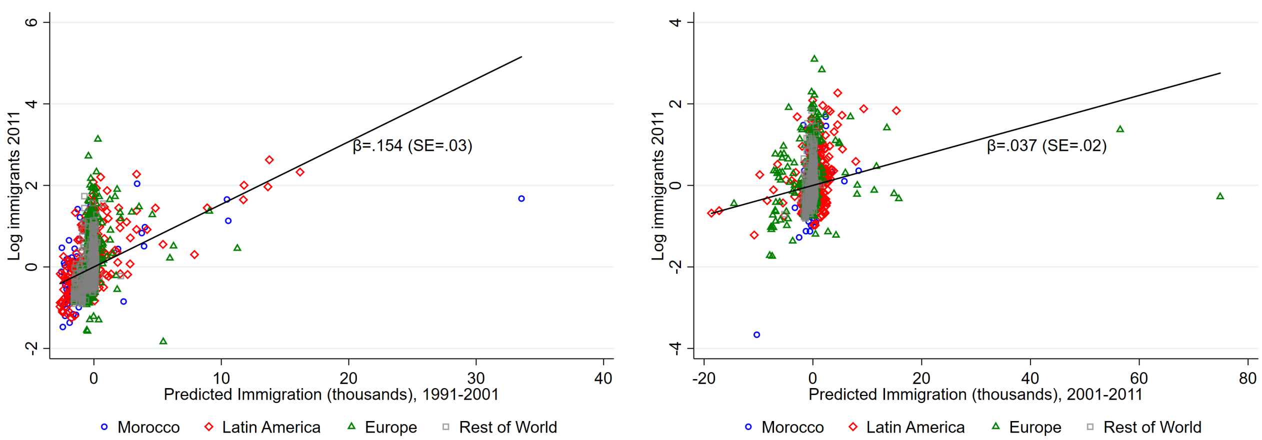
[16] Including higher-order interaction terms is standard practice in the small literature using these push-pull instruments; see Burchardi et al. (2019) and Choi et al. (2023).

the cost of observing fewer origins.

3.3 First-Stage

In Figure 3, I plot the residualized first-stage fit of the instruments for the two decades of predicted inflows. All variables are residualized on the set of country and province fixed effects as well as log distance. The instruments vary positively with the log number of immigrants, as expected. Moreover, the first-stage strength is driven by variation in the important drug sending and receiving regions: Morocco (the top exporter of cannabis to Spain), Latin America (the top exporter of cocaine to Spain), and Europe (the top recipient of exported Spanish drugs). In Appendix Table D.2 I show first-stage regressions across different sets of instruments, with column 3 corresponding to the regressions depicted in Figure 3. Instruments from both decades have a positive and statistically significant coefficient across specifications. The preferred set of instruments that I use in subsequent estimation is the set of instruments and second-order interactions, shown in column 4 of Appendix Table D.2.[17]

Figure 3: First-Stage Fit



Notes: The figures show the conditional scatter plots of *Log immigrants* 2011 with the instruments for immigrant inflows for decades 1991 to 2001 (on the left) and 2001 to 2011 (on the right). Both *Log immigrants* 2011 and the predicted inflows are residualized on origin and destination fixed effects, log distance, and on the instrument from the left-out decade. Each point represents an immigrant origin country-by-destination Spanish province pair, with immigrant origin regions color coded. For example, immigration from Morocco to the 52 Spanish provinces is plotted with blue circles, while immigration from Latin America is plotted with red diamonds. The regressions depicted correspond to column 3 of Table D.2.

[17]Results are robust to more parsimonious sets of instruments, as shown in Appendix Section C.1.

3.4 Results

I now turn to my baseline results on the effect of immigrants on confiscations of illegal drug imports and exports.

Panel A of Table 1 shows the two-stage least squares estimation results of equation 1. Column 1 refers to imports, and column 2 to exports. The coefficient estimate of the effect of immigrants on the likelihood of a confiscation of imported illegal drugs for a country-province pair is 0.172 (SE = 0.046). This estimate implies that at the mean immigrant population at the province-country-pair level, 942, a 10% increase in the number of immigrants raises the likelihood that drugs trafficked from the immigrants' origin country will be confiscated locally by about 0.8 percentage points.[18] For comparison, 8.4% of country-province pairs exhibited some amount of illegal drug confiscations.

Table 1: Effect of Immigrants on Drug Trafficking

	(1) Imports	(2) Exports
PANEL A: 2SLS		
Log immigrants 2011	0.172	0.0625
	(0.0461)	(0.0347)
Observations	5564	5564
	Imports	Exports
PANEL B: OLS		
Log immigrants 2011	0.141	0.0691
	(0.0224)	(0.0215)
Observations	5564	5564
Country FEs	Y	Y
Province FEs	Y	Y
Log distance	Y	Y

Notes: The table presents coefficient estimates from regressions of equation 1 at the country-province level. In Panel A, I instrument for *Log immigrants 2011* with $\{IV^D_{o,d} = I^D_{o,-a(d)} \times I^D_{-c(o),d}/I^D_{-c(o)}\}_{1991-2001,2001-2011}$, their interaction across decades, and squared terms. The dependent variable is a dummy for whether any drugs trafficked between country o and province d were confiscated between 2011 and 2016 (imports into Spain in column 1 and exports out of Spain in column 2). All regressions control for province and country fixed effects as well as log distance. Standard errors are clustered at the country level.

In column 2 of Panel A, I find that immigrants also increase exports of illegal drugs. The coefficient estimate is 0.063 (SE=0.035). This estimate implies that a 10% increase in the

[18] Using $\hat{\beta} = 0.172$ from column 1 in Table 1, can compute: $\mathbb{1}\left[C^{2011-2016}_{o,d,Imports} > 0 | M^{2011}_{o,d} = 942\right] = 0.172\left(\ln\left(1 + \frac{942\times1.1}{1000}\right) - \ln\left(1 + \frac{942}{1000}\right)\right) \approx 0.0081$.

number of immigrants relative to the mean raises the likelihood that drugs will be exported to the immigrants' origin country and confiscated locally by about 0.3 percentage points.[19] The point estimate for exports may be smaller than for imports because exports are likely more difficult to measure than imports, as police prioritize preventing drugs from entering Spain rather than from leaving the country. Nevertheless, the 95% confidence intervals overlap, so I cannot rule out equality between the two coefficients.

For comparison, I show OLS estimates in Panel B. There are two biases relative to the OLS to consider. First, there may be confounding variables at the country-province-pair level which drive both immigration and drug trafficking between locations. These confounders will tend to bias the OLS estimates upwards. Second, the number of immigrants from a given country living in a Spanish province may be mismeasured, biasing the OLS estimates downwards. My two-stage least squares estimates are statistically indistinguishable from the OLS estimates, suggesting that after controlling for a rich set of fixed effects, bilateral confounders do not substantially bias the OLS estimates.

3.5 Value of Drugs Confiscated

To see whether immigrants increase drug trafficking on the intensive margin, I next estimate the effect of immigrants on the value of drugs confiscated. In order to measure the value of the dependent variable in logs without dropping zero values, I use pseudo-Poisson maximum likelihood (PPML) estimation (Silva and Tenreyro, 2006). Due to the non-linearity of PPML, I take a control function approach to generating exogenous variation in the immigrant population (Petrin and Train, 2010; Morten and Oliveira, 2023).[20]

In particular, I estimate the first-stage as in column 4 of Table D.2 and add the residuals to the PPML estimating equation. The PPML first-order condition is then

$$\sum_{o,d} \left(Value\ confiscated_{o,d}^{2011\text{–}2016} - \exp(\delta_o + \delta_d + \beta M_{o,d}^{2011} + \zeta \hat{\epsilon}_{o,d}^{M} + \gamma \ln(Dist_{o,d}) \right) X_{o,d} = 0 \tag{3}$$

where $Value\ confiscated_{o,d}^{2011\text{–}2016}$ is the value in dollars of illegal drugs confiscated between country o and province d; $\hat{\epsilon}_{o,d}^{M}$ is the first-stage residual; and $X_{o,d}$ is the vector of variables included in the exponential function (i.e., dummies for countries and provinces, $M_{o,d}^{2011}$, $\hat{\epsilon}_{o,d}^{M}$,

[19] Using $\hat{\beta} = 0.0625$ from column 2 in Table 1, can compute: $\mathbb{1}\left[C_{d,o}^{2011\text{–}2016} > 0 | M_{o,d}^{2011} = 942\right] = 0.0625 \left(\ln\left(1 + \frac{942 \times 1.1}{1000}\right) - \ln\left(1 + \frac{942}{1000}\right)\right) \approx 0.003$.

[20] Atalay et al. (2019) use Monte Carlo simulations to show that the control function approach in PPML produces consistent estimates similar to the GMM estimator developed by Wooldridge (1997) and Windmeijer (2000).

and $\ln(Dist_{o,d})$). I estimate equation 3 separately for imports and exports as in the baseline estimation.

I show the results of the PPML estimation in Table 2.[21] In columns 1 and 3, I estimate the effect of immigrants on import and export confiscation values, respectively, without including the first-stage residuals. In columns 2 and 4 I add the first-stage residuals.

Table 2: Effect of Immigrants on Drug Trafficking (Intensive Margin)

	Value of drug confiscations			
	(1) Imports	(2) Imports	(3) Exports	(4) Exports
Log immigrants 2011	0.746	0.463	0.0341	0.649
	(0.220)	(0.285)	(0.276)	(0.354)
First-stage residuals		0.437		-0.722
		(0.285)		(0.390)
Observations	3120	3120	2640	2640
Country FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y
Ln dist.	Y	Y	Y	Y
1st-stage F-statistic		112.5		92.0

Notes: The table presents coefficient estimates for the intensive margin from pseudo-Poisson maximum likelihood estimation at the country-province level. I instrument for *Log immigrants 2011* with $\{IV^D_{o,d} = I^D_{o,-a(d)} \times I^D_{-c(o),d}/I^D_{-c(o)}\}_{1991-2001,2001-2011}$, their interaction across decades, and squared terms. The dependent variable is the value of illegal drug confiscations between country o and province d between 2011 and 2016. I implement a control function approach using Poisson pseudo-maximum likelihood estimation whereby I estimate residuals from a first-stage regression of all the instruments on *Log immigrants 2011*, and then include that residual as a control in the second-stage regression as in columns 2 and 4. All regressions control for province and country fixed effects as well as log distance. Standard errors are clustered at the country level.

Consistent with my baseline results, I find that immigrants increase the value of drugs imported and exported. In particular, the coefficient estimate of the effect of immigrants on the value of imported illegal drugs for a country-province pair is 0.46 (SE = 0.29). This estimate implies that a 10% increase in the number of immigrants relative to the mean raises the value of drugs trafficked from the immigrants' origin country and confiscated locally by 2.3%.[22]

[21] Note that my sample size drops in the PPML relative to my baseline. This is because PPML estimates will not exist for countries or provinces that never experience drug confiscations given my inclusion of country and province fixed effects (Silva and Tenreyro, 2010). Correia et al. (2019) argue that it is best to drop such "separated" observations from the estimation since they do not contribute to the estimation of β. For all PPML estimates, I use the methods developed by Correia et al. (2020).

[22] Using $\hat{\beta} = 0.463$ from column 2 in Table 2 and a mean bilateral immigrant population of 942, we have: $\frac{C^{2011-2016}_{o,d}[M^{2011}_{o,d}=1.1\times 942]}{C^{2011-2016}_{o,d}[M^{2011}_{o,d}=942]} - 1 = \exp\left(0.463\left(\ln\left(1+\frac{1.1\times 942}{1000}\right) - \ln\left(1+\frac{942}{1000}\right)\right)\right) - 1 = 0.022.$

Turning to the effect of immigrants on the value of drug exports, the estimated coefficient is 0.649 (SE=0.354). This estimate implies that a 10% increase in the number of immigrants relative to the mean raises the value of drugs trafficked to the immigrants' origin country and confiscated locally by 3.1%.[23] As in my estimates of equation 1, the effects of immigrants on imports and exports are statistically indistinguishable. In sum, I find that immigrants increase the extensive and intensive margin of drug trafficking with their origin countries.

My point estimate import and export elasticities are similar to those found elsewhere in the immigration-trade literature. In a meta-analysis of 48 studies, Genc et al. (2011) find an interquartile range of imports-to-immigration elasticities of 0.07 to 0.26, and for exports 0.06 to 0.28. My estimates of a 0.23 import elasticity and 0.31 export elasticity fall at the upper end of this range, but lower than Parsons and Vézina (2018), who estimate an export elasticity of between 0.4 and 0.6.

3.6 Robustness of Baseline Estimates

I establish the robustness of my baseline findings through a variety of exercises. I briefly summarize some of these robustness checks below, but for additional details and additional robustness checks (e.g., exploring variation in standard error choices, functional form choices, sampling choices, and alternative instrumental variables) see Appendix C.

Panel estimation. I estimate a panel version of equation 1 in Appendix Section C.3. Two drawbacks of this approach is that the panel dataset exhibits greater variance year-to-year, and the specification with province-country fixed effects changes the interpretation of the coefficient on the bilateral immigrant population. Nevertheless, the results from the panel estimation are largely consistent with my baseline cross-sectional gravity estimates from Section 3.4.

Shift-share instrumental variable diagnostics. A recent set of papers have clarified a range of issues regarding the exogeneity assumptions of instrumental variables which leverage different sources of variation according to a known formula (Goldsmith-Pinkham et al., 2020; Borusyak and Hull, 2021). I implement two tests inspired by this literature. See Appendix C.4 for additional details and explanation.

Measurement error in dependent variable. One common challenge in studies of crime is measurement of criminal activity (Pinotti, 2020). I conduct an exercise in Appendix Section

[23] Using $\hat{\beta} = 0.649$ from column 4 in Table 2 and a mean bilateral immigrant population of 942, we have: $\frac{C_{d,o}^{2011-2016}[M_{o,d}^{2011}=1.1\times942]}{C_{d,o}^{2011-2016}[M_{o,d}^{2011}=942]} - 1 = \exp\left(0.649\left(\ln\left(1+\frac{1.1\times942}{1000}\right) - \ln\left(1+\frac{942}{1000}\right)\right)\right) - 1 = 0.031$.

C.5 to address concerns about relying on an enforcement proxy to measure trafficking. In particular, in my baseline I implicitly assume that enforcement intensity does not vary with immigration. By contrast, I can relax this assumption by focusing on the set of country-province pairs which I predict to be on the margin of drug trafficking. In doing so I quantify how much enforcement intensity drives my results using different assumptions than in my baseline analysis. I find that enforcement intensity cannot fully explain my results, with the effect of immigrants on both imports and exports with their origin country remaining positive.

Dropping the major drug origins. Morocco and Latin America represent a disproportionate share of both immigrants and drug trafficking origins. To see whether my results are primarily driven by these origins, I re-estimate my baseline specification without these origins in Appendix Section C.6.2. I find that even without these origins, the results are consistent with my baseline estimation.

Heterogeneity across drugs. In my baseline estimation I combine two types of drugs: cannabis and cocaine. Because cannabis is considered a "soft" drug and cocaine a "hard" one, each drug may have different social costs. In Appendix Section C.6, I estimate my baseline specification separately for cannabis and cocaine, with results shown in Appendix Figure C.3. The coefficients are consistently positive, and all except cocaine exports are statistically significantly different from 0. Moreover, the effect of immigrants on cocaine imports from their origin country is the largest by magnitude.

Taking account of general equilibrium adjustment. As with any gravity equation, my estimates using equation 1 alone can only be used to make relative statements. That is, how much immigrants from a given origin living in a given province increase drug trafficking *relative to* another origin-province pair. While I do not formally estimate the aggregate effect of immigrants on drug trafficking, in Appendix Section C.7 I conduct two suggestive exercises which provide evidence consistent with immigrants increasing overall trafficking. First, I estimate a province-level panel relating local drug confiscations with the population share of immigrants. Second, I plot the time series of drug confiscations for two groups of provinces: those in the lowest tercile immigrant population share as of 2000, and those in the top tercile in Appendix Figure C.6. Both exercises suggest that immigrants increase the aggregate inflow of illegal drugs, though of course one cannot definitively say so with just these exercises alone.

Cross-Country Estimation. I also explore the relationship between immigrants and trade around the globe. I do so using the same UNODC dataset on individual drug seizures and the same empirical strategy and instrumental variable approach. I detail the exercise, its limitations, and the results in Appendix Section C.8. Overall the cross-country findings are consistent with my main results from Spain.

4 Mechanisms

Several potential mechanisms may drive the baseline effects estimated in Section 3. These include, (i) immigrants having a preference for drugs imported from their home country, (ii) immigrants reducing bilateral trade costs, and (iii) immigrants who were more exposed to drug trafficking in their home country developing drug trafficking skills, such as the ability to evade police. While it is difficult to definitively prove the importance of once channel over another, I provide some suggestive evidence in the direction of certain mechanisms over others below.

4.1 Immigrant Preferences

Atkin (2016) and Bronnenberg et al. (2012) find that immigrants and consumers in the immigrants' home country may exhibit similar preferences for consumption goods. If these similar tastes also apply to illicit drugs, more drugs may be trafficked from the immigrants' origin country. However, such a story would require retail drug consumers to have an implausible combination of tastes and information. Consider an immigrant from Venezuela who consumes cocaine. This immigrant would need to be able to distinguish street cocaine based on which country it was trafficked from (rather than produced in) by, for example, asking drug dealers if their cocaine comes from Venezuela or some other country. However, since the modifications to cocaine generally occur close to the point of production, and in any case do not differ much based on production location, it is unlikely that the immigrant's utility from consuming the cocaine would differ much based on which country the cocaine was trafficked through.[24]

Moreover, the overall demand for drugs by immigrants is disproportionately lower than for natives. I compare drug use between immigrants and native-born Spaniards and find that immigrants consume drugs at a substantially lower rate. Using the EDADES data introduced

[24]It is plausible that strains of cannabis and hashish may be somewhat differentiated in the retail market by country of origin. However, the fact that I estimate an even stronger effect of immigrants on cocaine imports than on cannabis imports (see Appendix Figure C.3) suggests that this origin-specific product differentiation is not particularly salient.

in Section 2.3 for the years 2005 through 2015, I find that 22% of respondents born outside of Spain have ever consumed cannabis, cocaine, heroin, or amphetamines compared to nearly 35% of native-born Spaniards.

4.2 Trade Costs

The joint increase along both the import and export margins of international trafficking in response to more immigrants is the key evidence for the mechanism that immigrants reduce bilateral trade costs. While a rise in imports but not exports would be consistent with the immigrant preference mechanism, the fact that I observe a rise in exports to the immigrants' origin country suggests instead that immigrants' reduction of bilateral trade costs is more salient for explaining my results.

How might immigrants drive down bilateral trade costs? I consider three candidate explanations: (i) reducing bilateral passenger transit costs through economies of scale as demand for travel increases with more bilateral immigration; (ii) immigrants from a given origin reducing the cost of hiring labor for the transit of goods from that origin; and (iii) immigrants' social connections to their origin country—e.g., family, friends, or professional contacts—reducing the search costs of arranging import and export transactions, especially in an illegal context.

Airlines may face declining average costs as quantity supplied rises, leading more immigration to reduce the costs of flights between a country and Spanish province.[25] I quantitatively rule out this channel, that of economies of scale in passenger traffic. For example, To test whether this mechanism drives my results, I first restrict my sample to the set of country-province pairs more than 1000 km away from each other. Second, I only consider confiscations which do not occur on airplanes (see Appendix Figure D.3 for the distribution of confiscations by transit mode), which leaves me primarily with container ships. I show the results in Appendix Table D.3. The coefficient on imports is very close in magnitude to my baseline results in Table 1, and for exports the magnitude increases by almost 50 percent. Both coefficients remain statistically significantly different from 0. This suggests that the trade cost reductions of immigrants do not operate through increases in passenger traffic.

I offer two alternative channels by which immigrants drive down trade costs. First, I suggest that immigrants' social connections may be driving my baseline results. Immigrants may

[25]Note that if immigrants increase bilateral trade, they may also push cargo shipping costs down. If flight volumes are more sensitive to immigration than cargo shipping, then this exercise remains valid. However, I am not aware of any studies estimating the elasticity of immigration to flight volumes, and therefore cannot evaluate quantitatively which transportation method immigrants affect more. I believe it is quite plausible that immigrants seeing friends and relatives in their home country (and vice versa) leads to a greater elasticity of immigration on flight volumes than on cargo shipping.

increase illegal trade in much the same way they can raise legal trade. Immigrant networks may reduce information and search frictions for trade between two locations, since trust may be greater within nationality and information travels more smoothly within nationality group (Gould, 1994; Felbermayr et al., 2015). Allen (2014), Chaney (2014), and Startz (2021) find that information frictions are a key driver of trade costs. These information frictions, such as the difficulty of a drug trader trying to source illegal drugs, are particularly challenging to overcome in the context of an illegal market. Immigrant networks may also raise the cost of opportunistic or cheating behavior by firms within the nationality network, who can be punished for bad behavior by being shunned from business within the network (Rauch and Trindade, 2002).

A substantial qualitative literature in criminology provides additional support for the effect of migrant social networks on trafficking. In a set of interviews in the United Kingdom conducted by Matrix Knowledge Group (2007), jailed traffickers shared the importance of social ties. Employees in the drug trafficking sector are typically recruited through employers' existing social networks,[26] and traffickers also noted examples in which a shared nationality raised trust between individuals seeking to conduct illegal trade transactions.[27] Proximity to immigrants from a variety of drug source countries was seen as advantageous as it reduced search costs.[28] In a study of Canadian drug traffickers, Desroches (2005) reports that among 34 jailed traffickers, nearly all chose to work exclusively within their own ethnic group.

Reducing search costs is particularly important for international traffickers, as they typically cannot search online for better prices or to investigate the quality of the products remotely, and must also be careful when communicating with existing contacts over the phone or internet for fear of police wiretaps.[29]

Second, I provide suggestive evidence that immigrants reduce transportation costs for more labor intensive transit modes. For example, cocaine may be sent from Latin America to Spain either by air or by sea. Sending cocaine on a commercial flight is typically labor intensive, as the drugs must be smuggled by a so-called 'drug mule.' In contrast, loading cocaine onto a container ship requires no such in-person chaperoning, and hence is likely to

[26] "A number of interviewees indicated that the importance of trust meant that they only recruited employees [for their smuggling organization] largely through their existing social networks." (Marsh et al., 2012)

[27] For example, "[one convicted drug trafficker] was from Ghana. In 2000 he was approached by a Ghanian friend to manage his drug business in the United Kingdom. He was trusted by the dealers he had to manage because they knew his family in Ghana." (Marsh et al., 2012)

[28] For example, one convicted trafficker said that to import cocaine into the United Kingdom, "You need to know someone in the West Indies but this is not difficult to do. London is multicultural, you can meet a contact." (Matrix Knowledge Group, 2007)

[29] While the so-called Darknet of online illegal purchases may eventually make cross-border purchasing more frictionless, it remains a tiny fraction of the drug market. The UNODC (2020a) estimates that the Darknet accounts for at most 0.2 percent of the global retail market for illegal drugs in the U.S. and Europe.

be less labor-intensive. By comparing these two modes of transit, we may be able to glean some suggestive evidence of the labor cost channel's relevance.[30]

I show estimates of the effect of immigrants on cocaine trafficking in Appendix Table D.4. I find that, consistent with the labor cost channel, trafficking by air (columns 1 and 3) is more sensitive to changes in the bilateral immigrant population relative to trafficking by sea (columns 2 and 4). While the differences are not statistically significant, they are suggestive of the salience of the labor cost channel.

4.3 Past Exposure to Crime in Origin Country

Immigrants may be more likely to participate in crimes which were more prevalent in their origin country. For example, Couttenier et al. (2019) find that past exposure to conflict makes migrants more likely to commit violent crimes. To understand the degree to which origin country exposure to drug trafficking drives trafficking participation in the host country, I look at whether drugs being confiscated are more likely to be coming from countries that are hubs of drug trafficking. I measure a country's drug hubness as the fraction of global drug confiscations emanating from the country (but not to Spain) between 2011 and 2016, and then take the ordinal rank. Data on global bilateral drug confiscations are taken from the UNODC dataset. One drawback of these data for countries other than Spain is that reporting drug confiscations to the UNODC occurs less frequently and is of lower quality (as discussed in Section C.8). Nevertheless, no alternative data source on country-pair drug trafficking exists, so I pursue this analysis using these imperfect data.

I re-estimate equation 1, interacting the country-province immigrant log population with the drug-hub rank of the immigrants' origin country. I further interact the instruments with the drug hubness rank.

In Table 3, I show the estimated coefficients. I find support for the hypothesis that immigrants' propensity to be involved in trafficking is related to the prevalence of trafficking in their origin country. Origin countries which ship lots of illicit drugs to countries beyond Spain are more likely to have immigrants facilitate the export of drugs to Spain. Column 1 shows that as a country ranks higher on global illegal drug exports, the more likely are its migrants to facilitate drug imports to Spain. I also find that immigrants from drug hub countries facilitate the export of illegal drugs to their home country, as shown in column 2.

[30]Such a comparison would not be informative for cannabis because much of the cannabis trafficked by sea is done on small manned boats in the Gibraltar straight, and hence both modes of transit likely exhibit similar labor intensity. This is one of several differences with the earlier estimates shown in Appendix Table D.3: here I exclude cannabis, include all confiscations no matter how far away the origin country, and restrict the modes of transit to those occurring either by sea or in airports.

Table 3: Effect of Immigrants on Drug Trafficking by Drug Hubness of Origin

	(1) Imports	(2) Exports
Log immigrants 2011	0.59	0.24
	(0.19)	(0.084)
Log immigrants 2011 × Drug hubness rank	-0.0011	-0.00038
	(0.00029)	(0.00016)
Observations	5564	5564
Country FE	Y	Y
Province FE	Y	Y
Ln dist.	Y	Y
1st-stage F-statistic	37.5	37.5

Notes: The table presents instrumental variable regression estimates at the country-province level. The dependent variable is a dummy for whether any illegal drugs trafficked with country o were confiscated in province d between 2011 and 2016, separately for imports and exports. I modify equation 1 to include a term interacting *Log immigrants 2011* with the ordinal rank of the fraction of confiscated drugs worldwide originating in the country. I instrument for *Log immigrants 2011* using the IV defined in equation 2, the IVs interacted across decades and squared, and the full set of IVs interacted with the drug hub rank. All regressions control for country and province fixed effects as well as log distance. Standard errors are clustered at the country-level in parentheses.

While only suggestive, these results indicate that the characteristics of immigrants' origin country, in particular their likelihood of exposure to drug trafficking, plays a role in facilitating of drug trafficking in their destination country. These results are consistent with an immigrant's skill in drug trafficking scaling with his likelihood of having observed trafficking in his home country.

5 Immigrant Legal Status and Trafficking

Thus far, I have shown evidence that immigrants increase drug trafficking from their origin country, and that this trafficking is driven by immigrants reducing bilateral trade costs as well as their exposure to trafficking in their origin country. To better understand how immigration policy shapes illegal drug trafficking, I next explore how immigrants' legal status affects the pattern and scale of drug trafficking.

Immigrants' integration into labor markets and civil society may be hampered when they do not have legal status. A lack of access to the formal labor sector lowers the opportunity cost of crime for immigrants without legal status. The lower opportunity cost may increase criminal activity among immigrants without legal status, as found empirically by Mastrobuoni and Pinotti (2015), Pinotti (2017), and Freedman et al. (2018), especially in financially motivated crime such as drug trafficking. To assess whether this intuition holds for drug trafficking, I estimate a gravity equation to test the effect of irregular immigrants (those without legal status) and regular immigrants on drug confiscations.

5.1 Measuring the Irregular Immigrant Population

Counting the number of immigrants without legal status can be challenging, as these immigrants are typically missed in standard administrative datasets (Warren and Passel, 1987; Borjas, 2017). Spain, however, offers unique institutional features which facilitate a more accurate tabulation of irregular immigrants. In particular, Spain has offered immigrants access to the public healthcare system regardless of one's legal status since the passage of the 2000 Aliens Law (González-Enríquez, 2009).

I take advantage of this institutional feature to impute the number of irregular immigrants in Spain at the country-province-pair level. To do so, I take the difference between the number of persons appearing in the population registry of province d from origin country o and the number of persons with residency permits in province d from country o. Specifically, I compute

$$Irregular\ \ Migrants_{od} = Population\ \ Registry\ \ Count_{od} - Residency\ \ Permits_{od} \tag{4}$$

and then divide by the total bilateral immigrant population to obtain the fraction of immigrants who have irregular status.[31]

The population registry is an imperfect measure for several reasons. First, municipalities differ in their documentation requirements for registration and the degree to which they notify immigrants that they must re-register every two years. In addition, according to González-Enríquez (2009), sex workers and immigrants from China are less likely to register due to deportation fears. This measurement error in immigrant legal status may correlate to drug trafficking. However, if the measurement error occurs for a specific nationality across Spain (say, for Chinese immigrants) or for all immigrants in a particular Spanish province (e.g., due to local registration practices), the country and province fixed effects will absorb variation in trafficking induced by such measurement error. Any residual measurement error at the country-province-pair level will bias the estimated coefficients toward zero so long as it is classical.

I impute the irregular immigrant population for set of the origin countries for which I observe bilateral population registry figures and bilateral residency permits in 2011 and the 52 Spanish provinces. I estimate that 20% of immigrants living in Spain are irregular, in

[31] For countries within the E.U., I set the number of irregular immigrants to 0.

line with the estimate from González-Enríquez (2009). My estimate also roughly aligns with Gálvez Iniesta (2020), who finds that 11 to 13 percent of non-EU immigrants in Spain lack legal status as of 2019.

5.2 Gravity Estimation by Legal Status

To explore whether immigrant legal status can explain the connection I find between immigrants and drug trafficking, I modify my baseline specification to include two separate terms for the bilateral immigrant population in 2011 by irregular ($M^{irreg}_{o,d}$) and regular ($M^{reg}_{o,d}$) status:

$$Y^{2011-2016}_{o,d} = \alpha_o + \alpha_d + \beta_{irreg} M^{irreg}_{o,d} + \beta_{reg} M^{reg}_{o,d} + \zeta \ln(Dist_{o,d}) + \varepsilon_{o,d} \tag{5}$$

where, as in equation 1, $Y^{2011-2016}_{o,d}$ is a dummy for any drugs trafficked between o and d were confiscated by Spanish authorities between 2011 and 2016, esdtimated separately for import and export confiscations. Thus β_{irreg} is the effect of irregular immigrants on trafficking and β_{reg} is the effect of regular immigrants on trafficking.

Separating immigrants by legal status introduces another endogeneity issue—differential selection of immigrants into legal status and trafficking—which the baseline leave-out push-pull instrument defined in equation 2 may not sufficiently address. For example, some immigrants with a higher taste for risk may be more likely to lack legal status and participate in illegal drug trafficking. To the extent that this selection is common across provinces for a given nationality, the country fixed effect α_o will absorb such selection. Similarly, if the characteristic is common across immigrants of all nationalities in a given province, the province fixed effect α_d will absorb this common preference for risk-taking.

To address province-country-specific selection into irregularity and drug trafficking, I modify the leave-out push-pull instrument predicting total immigrant inflows to predict immigrant inflows by legal status. In particular, I define the legal status-specific leave-out push-pull instrument as:

$$IV^{D,L}_{o,d} = m^L_{o,d} \times IV^D_{o,d} \tag{6}$$

for $L \in \{regular, irregular\}$ and decade D, where $IV^D_{o,d}$ is the baseline leave-out push-pull from equation 2 and $m^L_{o,d} = \frac{immigrants^{2001,L}_{o,-a(d)}}{immigrants^{2001}_{o,-a(d)}}$ is the fraction of immigrants with legal status L from country o who live outside the autonomous community of province d back in 2001. I use 2001 as the base year as it was the first year in which irregular immigrants were incentivized to enroll in their local population registry in order to qualify for public health care due to the passage of the 2000 Aliens Law (González-Enríquez, 2009). The instrument interacts variation across three dimensions: (i) immigration from various origin countries, (ii) immigration to various Spanish provinces, and (iii) the propensity of immigrants to have legal status L at the country-province level.

The identification assumption is that there are no confounders at the country-province level which are persistent from 2001 to 2011 and present in both province d and another province outside d's autonomous community and which drive selection of immigrants into both irregular status and drug trafficking. For example, suppose we want to predict the fraction of irregular Moroccan immigrants living in Barcelona in 2011. $m^L_{o,d}$ uses information on the legal status of Moroccan immigrants outside Catalonia (the autonomous community of Barcelona) back in 2001 to predict the 2011 legal status of Moroccans in Barcelona. The exclusion restriction is violated if, say, Moroccans in Madrid in 2001 were driven into irregularity and drug trafficking by the same confounder (e.g., a preference for risk-taking) that drove Moroccans in Barcelona in 2011 into irregularity and trafficking. This endogeneity will meaningfully affect the estimation if a non-trivial share of Moroccans outside Catalonia live in Madrid in 2001 and the confounder acts disproportionately on Moroccans in Madrid moreso than on Moroccans elsewhere (i.e., it is not absorbed by the Moroccan fixed effect).

I show the results of estimating equation 5 in Table 4. The coefficient on irregular immigrants is 0.26 (SE=0.06), which is substantially larger than the coefficient of 0.07 (SE=0.06) for regular immigrants. Moreover, the effect of regular immigrants on illegal trafficking is statistically insignificant. The effect of irregular immigrants implies that a 10% increase in the bilateral irregular immigrant population raises the likelihood of an illegal drug import confiscation by 0.4 percentage points.[32] I also find that regular immigrants increase illegal drug exports, while irregular immigrants reduce them, as shown in column 2 of Table 4.[33]

The import results are therefore consistent with the Becker-Ehrlich model of crime: immigrants with worse labor market opportunities due to their legal status are more likely to facilitate illegal drug trafficking. However, the results for exports are puzzling when interpreted through the lens of the Becker-Ehrlich model of crime.[34]

Instead, I argue that two factors drive the result for irregular immigrants on exports.

[32] Using $\hat{\beta}^{Irreg} = 0.271$ from column 1 and mean value of bilateral irregular immigrant population of 204, I find that $\mathbb{1}\left[C^{2011-2016}_{o,d} > 0 | M^{2011}_{o,d} = 204\right] = 0.26\left(\ln\left(1 + \frac{204\times 1.1}{1000}\right) - \ln\left(1 + \frac{204}{1000}\right)\right) \approx 0.0044$. The regular and irregular coefficients are statistically different at the 10% level.

[33] The coefficients on exports for regular and irregular migrants are statistically different at the 1% level.

[34] The negative coefficient on irregular immigrants in column 2 is likely a result of the fact that there are few irregular immigrants from the top export destinations (e.g., France) while those countries with many irregular immigrants in Spain rarely receive any exports (e.g., Latin America and Morocco). Indeed, when I drop Latin America and Morocco from the analysis, the coefficient on irregular migrants becomes statistically insignificant. Full results are available upon request.

Table 4: Effect of Immigrants by Legal Status on Drug Confiscations

	Dummy for Any Drug Confiscations	
	Imports	Exports
Log regular immigrants 2011	0.0679	0.148
	(0.0588)	(0.0336)
Log irregular immigrants 2011	0.260	-0.164
	(0.0608)	(0.0430)
Observations	5252	5252
SW 1st-stg. F-stat. (regular immigrants)	34.4	34.4
SW 1st-stg. F-stat. (irregular immigrants)	86.1	86.1

Notes: The table presents estimates of IV regressions by legal status at the country-province level. The dependent variable is a dummy for whether any confiscation occurred, separately for imports (column 1) and exports (column 2). I instrument for the immigrant population by legal status using equation 6 as well as the interaction across decades and squared terms. SW F-statistics refer to those described by Sanderson and Windmeijer (2016). Standard errors are clustered by country.

First, all immigrants from Schengen countries are automatically regular and can freely move throughout the bloc. This means that irregular immigrants cannot facilitate exports to Spain's primary illegal drug export markets, which are in Europe (see Figure D.6).

Second, export trade costs out of Spain are much lower when the final destination is within the E.U. than otherwise.[35] This makes Spain useful to traffickers insofar as it helps them reach lucrative E.U. markets.

To assess the importance of the trade and migration cost margin, I re-estimate the baseline gravity specification and split the sample into E.U./Schengen countries and all other countries. I show the results in Table D.7, with estimates on exports in Panel A and on imports in Panel B. The results are split by the sample of E.U./Schengen countries in columns 1 and 2 and for all other countries in columns 3 and 4.

Whether the export destination is inside the E.U. is crucial for explaining my results. I find that Spain serves as an entrepôt for drugs coming from outside the E.U. and being shipped to destinations within the E.U., but not for drugs being shipped to non-E.U. countries. This is shown by the estimates in columns 1 and 3 of Panel A. Column 1 shows that immigrants (regardless of their legal status) play an important role in promoting inter-E.U. illegal drug re-exports. Column 3 shows that immigrants (again, regardless of their legal

[35] Fukumi (2008) notes that "the introduction of the Schengen Agreement in 1985, and the full implementation of the Schengen Treaty in 1995 opened a window of opportunity to cocaine traffickers because it enabled free movement within a major part of Western Europe." (p. 50) She further argues that drug traffickers often launder money by buying import and export companies, commodity trading businesses, and cargo businesses, which are all useful in transporting illegal drugs (p. 54). I argue that such companies are much more likely to be operated by someone with legal status, either a native or a regular immigrant.

status) play a negligible role in facilitating re-exports to non-E.U. countries. By contrast, there is no such heterogeneity in effect size for imports, as shown in columns 1 and 3 of Panel B.

I then look at this effect on exports by legal status in columns 2 and 4 of Panel A. Among E.U./Schengen countries (column 2), I find that regular immigrants facilitate illegal exports but (mechanically) irregular immigrants cannot, since there are no irregular migrants from Schengen countries. Among non-E.U./Schengen destinations (column 4), I find a modest positive effect for regular immigrants and modest negative effect for irregular immigrants, but both coefficients are statistically insigificantly different from 0. From these regression results I conclude that the E.U./non-E.U. sample split is more important than the regular/non-irregular immigrant split in driving my export results.[36]

I suggest, then, that the above results can be rationalized with the following intuition. Crossing a customs barrier is exceedingly costly for drug traffickers as these customs barriers are the most likely location for the illegal drugs to be confiscated by authorities. The trade-cost reducing effect of immigrants may not be enough to offset the hazard of multiple custom barrier crossings. Hence, transiting drugs through Spain is likely less profitable for drug traffickers than sending the drugs directly to the destination when that destination is outside the E.U., regardless of the irregular immigrant population size. By contrast, for destinations within the E.U., Spain is a useful entrepôt—that is, a place where traded goods pass through—where transshipments out of Spain may be facilitated by regular migrants from those E.U. countries.

5.3 Event Study

In 2005, Spain conducted the largest mass regularization of immigrants in its history, with over half a million immigrants obtaining legal status. Immigrants who were registered with their local council in the population registry as of August 8, 2004, were offered a work contract of at least six months (three months if in agriculture), and had no criminal record in their home country or in Spain, were eligible to apply for regular status, usually through their prospective employer (González-Enríquez, 2009). Moreover, the reform was a surprise

[36]Note that we cannot draw the same conclusion for import results, as shown in Panel B of Table D.7: regardless of whether one considers the E.U. or non-E.U. sample, the effect of immigrants on illegal imports is statistically significantly positive and of equal magnitude (columns 1 and 3 of Panel B). Since there are no irregular immigrants from the E.U., the estimates effect in column 1 of Panel B necessarily must come from regular immigrants, as shown in column 2 of Panel B. For non-E.U. countries, the result is driven by irregular immigrants (column 4 of Panel B). Consistent with the Becker-Erhlich model of crime, regular immigrants are less likely to facilitate illegal drug imports. The discrepancy between the regular immigrant coefficients between columns 2 and 4 may result from the fact that a regular immigrant from a non-E.U. country has more to lose, since they may be deported to their origin country if caught.

as a result of the 2004 Madrid bombings shortly before the 2004 elections resulted in an upset victory for the pro-immigrant regularization party (Monras et al., 2021). The 2005 regularization led to a sharp increase in the number of work authorizations granted to immigrants in Spain, as shown in Figure D.10, without an increase in new immigration (Monras et al., 2021).

To better understand the effects of the regularization, I estimate a simple event study at the country-by-quarter level. The event study unit of aggregation differs from the estimates from my baseline in that I use higher frequency quarterly variation in drug confiscations and aggregate from the province-by-country level to the country level. I do so for three reasons. First, I use nationality-level aggregation because the policy differentially affected immigrants depending on their country of origin. For example, immigrants from the E.U. were not impacted by the policy since the Schengen Agreement precludes irregularity. Second, at the bilateral level, confiscations can occur highly irregularly, with no confiscations for several quarters followed by a quarter with one massive confiscation. Such volatility is likely more a result of variation in enforcement luck rather than changes in the actual flow of illicit drugs, and therefore reflects measurement error. To smooth out this variation and thereby obtain more precise estimates, I aggregate to the country-quarter level. Third, to better establish a causal relationship between the legalization policy and any change in drug trafficking, I exploit the rich detail in the timing of drug confiscations and look at confiscations at a quarterly frequency.

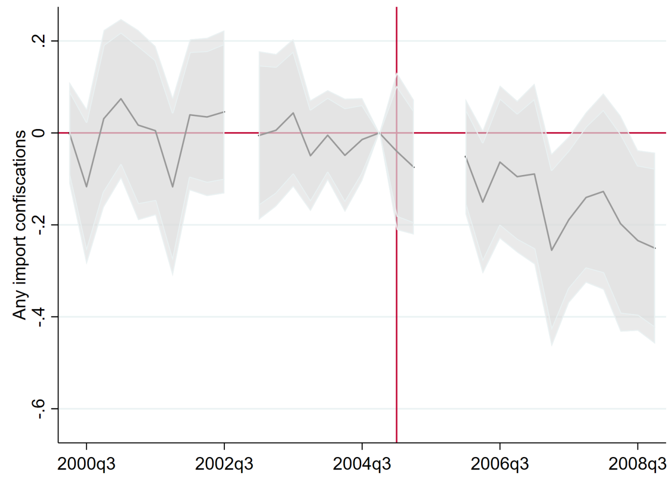
I estimate the event study using the equation

$$Y_o^t = \sum_{t \neq 2004q4} \theta^t \times Frac.\ irregular_o^{2003} + \delta_o + \delta^t + \epsilon_o^t \tag{7}$$

where $Frac.\ irregular_o^{2003}$ is the fraction of immigrants in 2003 without legal status, as imputed using equation (4) from the paper. I estimate the event study for the years 2000 through 2008, choosing the end-year cutoff of 2008 to avoid conflating any effects with the Great Recession.

I plot the θ^t coefficients in Figure 4 with the dependent variable a dummy for whether any import confiscation occurred. I show the 2005 regularization reduced the likelihood of any import drug confiscation and remained lower thereafter. Moreover, this decline came primarily from reductions in cocaine confiscations, as shown in Figure 5, with no change observed in cannabis confiscations. I find no discernable pattern in export confiscations, as shown in Figure D.11, where exports were statistically significantly greater than 0 even prior to the policy change.

Figure 4: Effect of 2005 Immigrant Regularization on Drug Imports



Notes: The figure shows an event study plot of the effect of the 2005 immigrant regularization on whether any drug imports were confiscated locally. Plot is estimated using equation 7. The dark grey area shows the 90% confidence interval while the light grey area shows the 95% confidence interval.

Figure 5: Effect of 2005 Immigrant Regularization on Confiscations by Drug Type

Notes: The figure shows event study plots of the effect of the 2005 immigrant regularization on import confiscations of cannabis (on the left) and cocaine (on the right). The dependent variable is whether any drugs were confiscated coming from the origin country in that quarter. Plots are estimated using equation 7. The dark grey area shows the 90% confidence interval while the light grey area shows the 95% confidence interval.

Immigrants' improved labor market opportunities as a result of the legalization program may partially explain my results. Monras et al. (2021) find that the 2005 legalization substantially improved immigrants' labor market outcomes by facilitating their entry into sectors with low levels of informal employment.

To explain the differential effect of the policy on cocaine—coming primarily from Latin America—versus cannabis (which comes primarily from Morocco) I note the various ways in which Spanish institutions treat Latin American immigrants more favorably than Moroccan immigrants. For example, for the 2005 mass legalization, Latin Americans made up about half of successful applicants for legalization whereas Moroccan applicants made up about 12%. Moreover, the likelihood of having a sucessful application varies by immigrant origin, with Moroccans experiencing a 9 percentage point lower likelihood of successfully applying for legalization as compared to Latin American immigrants.[37] Moroccan immigrants also face more hurdles to obtain Spanish citizenship, being required to be in the country legally for ten consecutive years. In contrast, Latin American immigrants are only required to be present for two consecutive years before becoming eligible for citizenship. Such policies have led to a divergence in citizenship acquisition by origin region, as shown in Figure D.12. Finally, Latin American immigrants are more likely to natively speak Spanish and thus face an easier time culturally and economically assimilating into Spanish society.

Overall, the event study results suggest that granting legal status to immigrants plays an important role in reducing illegal drug imports, especially for those immigrants facing the easiest time assimilating into Spanish society. Taking the average of the coefficients from 2005 to 2008 for the event study estimated on the extensive margin of illegal imports suggests that granting 10% of immigrants legal status reduces the likelihood of import trafficking of any drug by 1.4 percentage points and of cocaine specifically by 2 percentage points.

5.4 Discussion

Taken together, these results suggest immigrant legal status is an important factor shaping immigrants' role in drug trafficking. Across both exercises, the gravity estimation of Section 5.2 and the event study of Section 5.3, I find that irregular immigrants raise illegal drug imports and that legalizing those immigrants can reduce illegal imports. This is a key policy implication from my research.

For exports, I find that the institutional context of the European Union—within which both trade and migration is largely unrestricted—is key to understanding my seemingly divergent results. In particular, I find that the export-creating effects of immigrants only manifest for E.U. immigrants, who automatically have legal status in Spain. Since the 2005 legalization applied only to non-E.U. migrants, it had no effect on exports. Hence I conclude that legal immigrants can raise illegal exports when trade and migration costs are sufficiently low.

[37] Calculations based on Table VI of González-Enríquez (2009).

6 Conclusion

The effect of immigration on crime has long been a controversial political issue. In this paper, I contribute to this debate by causally estimating the effect of immigrants and immigrant legal status on international illegal drug trafficking. I find that an increase in the number of international immigrants increases international drug trafficking, both on the import and export margin. Four mechanisms likely drive these effects: immigrants' social connections with their origin country, immigrants labor supplied to drug transportation, immigrants' exposure in their origin country to drug trafficking, and immigrants' legal labor market opportunities, as proxied for by their legal status. I find that granting immigrants legal status can reduce illegal drug imports.

The results presented here have significant relevance to ongoing debates on immigration policy in the United States and around the world. In particular, as many European countries and the United States discuss providing some form of amnesty and a path to citizenship to their large populations of unauthorized immigrants, this paper offers an additional potential benefit to society from such amnesties. Providing amnesty may be cheaper to administer than attempting to keep irregular immigrants from entering the country, such as by building a wall. For example, Allen et al. (2018) estimate that the 2007–2010 expansion of the border wall on the U.S.-Mexico border cost approximately $57,500 per deterred immigrant.

An important caveat is that immigrants generate a range of effects on their host countries, from native-born wages to innovation. Hence, generalizing welfare effects of immigration from just one outcome, as is the subject of the present study, may lead to suboptimal policy choices. Instead, policymakers must weigh the varied impacts of migration when crafting immigration policy.

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Appendix to "Immigrants, Legal Status, and Illegal Trade"
by Brett McCully

A Data Appendix

As discussed in Section 2.2, I draw the data on drug confiscations from the United Nations Office of Drugs and Crime (UNODC). For my baseline estimation, I use data on confiscations from Spain, which reports high-quality drugs data. However, in some exercises (Section 4.3 and Appendix Section C.8) I utilize data from all countries in the dataset. I next describe in greater detail the UNODC data and the data cleaning procedures I apply.

Drug Groupings. The UNODC data on drug confiscations are reported in a variety of unit amounts and drug types. In the empirical analysis, I focus on the two primary drug groups trafficking in Spain: cannabis and cocaine. In the worldwide analysis of Section C.8, I also include heroin and amphetamines. Because drug confiscations are reported for a variety of drug types, I condense these types into aggregated groupings according to Table A.1.

Comparability of Confiscation Amounts. Comparing confiscated amounts even within drug groupings is challenging due to their imperfect substitution. For example, opium and heroin are two distinct drugs with different street prices. To make confiscations comparable across drug types and the reported unit of seizures, I proceed in three steps. First, I translate all units into kilograms. Second, I apply a deflation term to the imputed kilograms depending on what stage in the production chain the drug type is (e.g., cocaine base is an input into consumable cocaine). Third, I convert the kilogram measure into a dollar value using a Spanish survey of drug prices reported by the UNODC.

The first step, translating units into kilograms, is straightforward when a mass or weight unit is provided. When a mass value is not provided (e.g., I see the number of capsules or liters of a confiscation), I turn to United Nations and scientific papers on the estimated average conversion rates between different units of drugs and their consumption-grade equivalent in kilograms. Following the UNODC (p. 39, 2017), I convert 1 liter into 1 kilogram for all drugs, and a cannabis plant into 100 grams. For amphetamines and methamphetamines, I apply conversion rates summarized in Table A.2.

Additionally, I convert drugs higher up the supply chain—i.e., inputs into final consumable drugs—into an equivalent amount of the drug lower down the supply chain. For example, heroin is derived from opium poppies, with about 9.6 kg of opium producing 1 kg of heroin

Table A.1: Author's Drug Groupings

Drug Group (Analysis Data)	**Drug Type (Raw Data)**
Cannabis	Cannabis Cannabis herb (Marijuana) Cannabis leaves Cannabis oil Cannabis plants Cannabis pollen Cannabis sativa Cannabis resin Cannabis seeds
Cocaine	Cocaine Coca plant Coca seeds Coca leaf Coca paste Cocaine base Cocaine HCL
Heroin	Heroin Heroin base Extract from Opium poppy Morphine Opium Acetylated Opium
Amphetamines	Amphetamine Liquid methamphetamine Methamphetamine

Notes: The table describes how I group drug types within the UNODC data.

(UNODC and Afghan Ministry of Counter Narcotics, 2015). I therefore convert opium poppies into their heroin equivalent. I also convert morphine base into heroin, following Figure II of Zerell et al. (2005), which states that 7.8 kg of morphine base can be convered into 3.9 kg of white heroin.

Cleaning the Geography of Confiscations. The UNODC data provides data on the "Place" of the confiscation, which may refer to the province or municipio. Within Spain, I match municipios to their province using the IPUMS crosswalk between regions. I drop municipios that I cannot match to a Spanish province, either because of some typos in the municipio name or because the place name does not correspond to a province.

As a result, I drop confiscations for which I cannot specify a Spanish province of seizure. In addition, I drop confiscations which lack any information on the departure or intended

Table A.2: Drug Conversion Rates for Amphetamines and Methamphetamines (doses to milligrams)

Region	**Dose**	**Milligrams**	
		Amphetamine	**Methamphetamine**
Africa	1	250	250
Asia (excluding Middle East/Southwest Asia)	1	250	90
Europe	1	253	225
Central and South America and Caribbean	1	250	250
Middle East/Southwest Asia	1	170	250
North America	1	250	250
Oceania	1	250	250

Notes: The table displays conversion rates of individual tablets, pills, capsules, or doses into milligrams following the table from page 38 of UNODC (2017). For example, confiscations of a single dose of amphetamines or methamphetamines in Africa are converted into 250mg.

destination country. The likelihood of observing all necessary geographic information varies with the mode of transit of the drugs, as shown in Table A.3. Column 2 reports the share of confiscation events which make it into the final sample, while column 3 reports the same but weighted by the value of drugs seized. Columns 4 and 5 report the value of confiscations in the raw and final dataset, respectively. I find that in all cases, the majority of confiscation events as well as the majority of the value of confiscated drugs make it into the final sample. This is true even for confiscations made in international waters. I conclude that to maintain good sample coverage across modes of transport.

How Spain Fills in the UNODC Data. Spain's Centro de Inteligencia contra el Terrorismo y el Crimen Organizado (CITCO) sends information on individual drug confiscations regularly to the UNODC. These data are assembled by the Sistema Estadístico de Análisis y Evaluación sobre Crimen Organizado y Drogas (SENDA), using information furnished by the Policía Nacional, Guardia Civil, and the Departamento de Aduanas e Impuestos Especiales. However, drug confiscations conducted by local or municipio police are also included so long as they are reported to the National Police or Guardia Civil as drug trafficking crimes.

Attribution of drugs to their origin and destination is done via investigation following confiscations, covering a range of evidence seized (e.g., persons detained and any relevant documentation). For example, for drugs confiscated from cargo ship containers, a range of documents are checked for country of origin and intended destination, including the bill of lading, the commercial invoice, the certificate of origin, customs clearance forms, and the relevant letter of credit. While investigations are conducted after every confiscation event, boats with hashish resin intercepted in the Strait of Gibraltar or on the Andalusian coast

Table A.3: Confiscations Sample Selection by Mode of Transit

(1)	(2)	(3)	(4)	(5)
	Share Retained		Value Confiscated (billion 2012 USD)	
Mode	# Confiscation	Value Confiscated	Raw data	Cleaned data
Airport	0.92	0.96	725	698
International waters	0.91	0.64	965	617
By sea (but not in int'l waters)	0.62	0.81	3,522	2,856
Other	0.77	0.72	1,665	1,202
Not specified	0.84	0.88	615	545

Notes: Column 1 lists the mode of transit in which the drugs were confiscated; column 2 lists the share of total confiscation events in the raw data which appear in the cleaned data; column 3 shows the same as column 2 but weighted by the value of each confiscation; column 4 lists the total value of confiscations by mode in the raw data; column 5 lists the total value of confiscations by mode in the cleaned data. The cleaning procedure drops confiscations which either have no listed departure or destination country, or have no information on Spanish municipio or province. All computations refer to the UNODC Individual Drug Seizures data for the 2011 to 2016 period. The "By sea (but not in int'l waters)" category refers to confiscations in which the mode listed is seaport, beach, port facilities, territorial waters, or underwater site. The "Other" category includes modes listed as customs facilities, highway, post office, road, street, warehouse, bar, court, factory, flat/apartment, hospital, military facilities, other, prison, or store.

are assumed to come from Morocco unless proven otherwise.

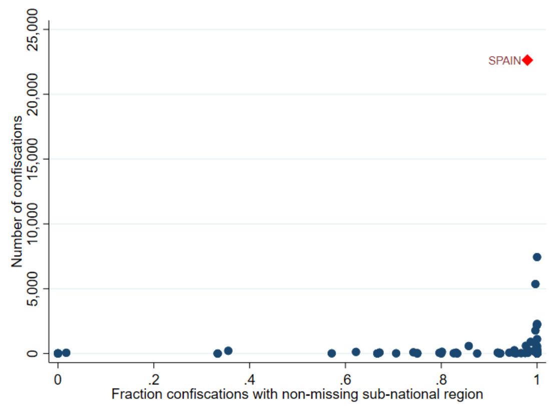
For drugs confiscated from airline passengers upon arrival at an airport, the most common mode of transport of confiscated drugs as shown in Appendix Figure D.3, the origin country is the passenger's departure country and the destination country is the passenger's ultimate destination on their travel itinerary. For less straightforward cases, such as the case of drug gangs transporting cocaine intercepted in the Atlantic Ocean off the Galician coast, the country of origin and destination is determined based on additional information such as suspect and witness interviews and communicating with law enforcement agencies in the suspected origin and destination countries. If a person is arrested within Spain for drug trafficking but is outside an airport or port, the country of origin of the drugs will be determined on the basis of the police investigation carried out, including any statements made by the arrested person.[38]

Comparison of UNODC Data Across Countries. In my baseline analysis I focus on the country of Spain due to its higher quality reporting of illegal drug confiscations to the UNODC. I graphically depict Spain's superior data reporting in Figure A.1. The figure plots the number of reported confiscations against the fraction of those confiscations in which I

[38]The preceding description is based on discussions with representatives from the Spanish Ministry of the Interior.

can identify the sub-national geography in which the confiscation took place, conditional on the confiscation reporting either an origin or intended destination country. It shows that Spain reports an unusually high number of confiscations in which the country of trafficking origin is reported (vertical axis). Moreover, Spain almost always reports the location within Spain in which the confiscation occurred (horizontal axis). Combined, these two dimensions of data quality make Spain an outlier within the UNODC data on individual drug seizures.

Figure A.1: Data Quality Across Countries



Notes: The chart plots the relationship between the number of confiscations reported to the United Nations Office of Drugs and Crime across all years with information on the sending country and the fraction of those confiscations with information on the sub-national region of the confiscation across countries. Sample is restricted to confiscations with information on either the departure or intended destination country.

B Instrumental Variable: Additional Discussion

In my baseline estimation, I instrument for the immigrant population using two decades of predicted immigrant inflows generated by a leave-out push-pull IV, defined in equation 2. Because the instrument leverages variation at the country and province level, and I have both country and province fixed effects, one may be concerned about what residual variation the IV captures. To better understand the variation generated by the IV conditional on o and d fixed effects, I first explore its residual variation after controlling for the set of fixed effects using the simple, non-leave-out version of the instrument (where I drop the decade superscript for notational clarity):

$$\widetilde{IV}_{o,d} = I_o \times \frac{I_d}{I} \tag{B.1}$$

Controlling for o and d fixed effects, the residual variation of B.1 become

$$\hat{\widetilde{IV}}_{od} = I_o \times \frac{I_d}{I} - \frac{1}{n_o}\sum_o I_o \times \frac{I_d}{I} - \frac{1}{n_d}\sum_d I_o \times \frac{I_d}{I}$$
$$= I_o^D \times \frac{I_d}{I} - \frac{I_d}{n_o} - \frac{I_o}{n_d}$$

where n_d is the number of Spanish provinces and n_o is the number of countries.

Therefore the push-pull IV predicts the bilateral immigrant flows by interacting the number of immigrants pushed out of o with the fraction of immigrants pulled into d, net of the average number of immigrants from o per Spanish province (I_d/n_o) and the average number of immigrants in d per origin country (I_o/n_d).

For example, if we want to predict the number of Moroccans going to Barcelona, I would interact the number of Moroccans coming to Spain ($I_{Morocco}$) with the fraction of immigrants across all origin countries coming to Spain who choose to live in Barcelona ($I_{Barcelona}/I$) net of the average number of immigrants in Barcelona per origin country ($I_{Barcelona}/107$) and the average number of Moroccans per Spanish province ($I_{Morocco}/52$). Note that as the number of geographic units (countries or provinces) grows, the fraction of the residual variation of the instrument made up by the push-pull interaction term grows. This holds so long as total immigration to Spain grows at a smaller rate than the number of provinces and countries do. Therefore, the instrument net of the fixed effects asymptotically approaches the baseline instrument of equation B.1: $\lim_{n_o\to\infty, n_d\to\infty} \hat{\widetilde{IV}}_{o,d} = \widetilde{IV}_{o,d}$.

Next, I consider how the above logic applies to the case of my baseline *leave-out* push-pull instrument. Netting out the provincial mean and country mean from equation 2, I obtain:

$$\begin{aligned}
\widehat{IV}_{od} &= I_{o,-a(d)} \times \frac{I_{-c(o),d}}{I_{-c(o)}} - \frac{1}{n_o}\sum_o I_{o,-a(d)} \times \frac{I_{-c(o),d}}{I_{-c(o)}} - \frac{1}{n_d}\sum_d I_{o,-a(d)} \times \frac{I_{-c(o),d}}{I_{-c(o)}} \\
&= I_{o,-a(d)} \times \frac{I_{-c(o),d}}{I_{-c(o)}} - \frac{1}{n_o}\sum_o (I_o - I_{o,a(d)}) \times \frac{I_d - I_{c(o),d}}{I - I_{c(o)}} - \frac{1}{n_d}\sum_d (I_o - I_{o,a(d)}) \times \frac{I_d - I_{c(o),d}}{I - I_{c(o)}} \\
&= I_{o,-a(d)} \times \frac{I_{-c(o),d}}{I_{-c(o)}} - \frac{I_d}{n_o}\sum_o \frac{I_o - I_{o,a(d)}}{I - I_{c(o)}} + \frac{1}{n_o}\sum_o (I_o - I_{o,a(d)}) \times \frac{I_{c(o),d}}{I - I_{c(o)}} - \frac{I_o}{n_d}\frac{\sum_d (I_d - I_{c(o),d})}{I - I_{c(o)}} + \frac{1}{n_d}\sum_d I_{o,a(d)} \times \frac{I_d - I_{c(o),d}}{I - I_{c(o)}} \\
&= \underbrace{I_{o,-a(d)} \times \frac{I_{-c(o),d}}{I_{-c(o)}} - \frac{I_d}{n_o}\sum_o \frac{I_o - I_{o,a(d)}}{I - I_{c(o)}} + \frac{1}{n_o}\sum_o (I_o - I_{o,a(d)}) \times \frac{I_{c(o),d}}{I - I_{c(o)}}}_{\text{decreasing in } n_o} - \underbrace{\frac{I_o}{n_d} + \frac{1}{n_d}\sum_d I_{o,a(d)} \times \frac{I_d - I_{c(o),d}}{I - I_{c(o)}}}_{\text{decreasing in } n_d}
\end{aligned}$$

In my baseline estimation I use all 52 Spanish provinces and 102 countries, with an additional country group per continent. Therefore, any increase in the number of immigrant destinations d or origins o will not increase total immigration to Spain, I^D. Therefore the same aymptotic logic applies whereby a larger number of origin and destination regions lead to a larger proportion of the residual variation in the instrument to come from the push-pull interaction term, i.e., $\lim_{n_o\to\infty, n_d\to\infty} \widehat{IV}_{o,d} = IV_{o,d}$.

C Empirical Appendix

C.1 Alternative Instrumental Variables

I next test the robustness of my baseline results to different choices of instrumental variables.

C.1.1 Dropping push-pull instruments

To assess whether a particular decade of immigration drives my results, I re-estimate the baseline with different subsets of the push-pull instrument in Panel A of Table C.1. I do so using only the instrument for immigrant inflows in the 1990s (the first row) and the 2000s (the second row), where each cell refers to a separate regression. In addition, I estimate my baseline without the higher-order interaction terms between the instruments in the third row of Panel A. Across all these regressions, I find that the immigrant population induces an increase in both imports and exports of illegal drugs. Differences across specifications in coefficient magnitudes are statistically insignificant.

Table C.1: Varying the Instrumental Variable

PANEL A: Using subsets of instruments	Import confiscations (Dummy)	Export confiscations (Dummy)
Only immigration 1991–2001	0.158**	0.095*
	(0.069)	(0.055)
Only immigration 2001–2011	0.099	0.079**
	(0.065)	(0.037)
Immigration 1991–2001, without squared and interaction terms	0.123*	0.085**
	(0.063)	(0.042)
PANEL B: variations of leave-out categories	**Import confiscations (Dummy)**	**Export confiscations (Dummy)**
Excluding origins with correlated 2011 immigrant pop.: $I^t_{o,-r(d)} \times (I^t_{-corr(o),d}/I^t_{-corr(o)})$	0.204***	0.095**
	(0.071)	(0.044)
Excluding provinces with correlated 2011 immigrant pop.: $I^t_{o,-corr(d)} \times (I^t_{-c(o),d}/I^t_{-c(o)})$	0.099***	0.095*
	(0.064)	(0.052)

Notes: The table presents coefficient estimates from instrumental variable regressions of equation 1, where each cell presents the coefficient on *Log immigrants* 2011 from seperate regressions. Panel A shows subsets of instruments relative to my baseline instrument set. Panel B shows alternative formulations of the baseline instrumental variable. In particular, I exclude from the pull factor countries with correlated immigrant populations across all Spanish provinces (in the first row of Panel B); and from the push factor, provinces with correlated immigrant populations across all origins (second row). For a region to be excluded due to correlation, the correlation coefficient must be greater than 0.5 with a p-value lower than 0.05. Standard errors are clustered at the country level.

C.1.2 Correlated leave-out groups

A remaining concern in the identification strategy is that some factor may still directly affect drug trafficking in two different provinces across different autonomous communities, while simultaneously driving disproportionate numbers of immigrants from two countries from different continents to the same Spanish provinces across different autonomous communities (e.g., the Lebanon–Morocco and Alicante–Barcelona example from Section 3.2). If such a confounder was affecting the baseline results, one would expect that excluding countries or provinces with correlated migration flows would significantly change the results.[39] In Panel B of Table C.1, I show that they do not.

In particular, I classify two origin countries as having correlated immigrant populations if, across Spain's 52 provinces, they have a correlation coefficient greater than 0.5 with a p-value less than 0.05. I then drop any correlated origins from the pull factor $\frac{I^t_{-corr(o),d}}{I^t_{-corr(o)}}$ of the push-pull instrument. This procedure drops sixty-three origin countries on average, with an average of seventeen from the same continent (the baseline leave-out group). In the first row of Panel B, I show the results for the baseline gravity specification (equation 1) when using this alternative instrument. The coefficients are statistically indistinguishable from those estimated in the baseline.

Similarly, in the second row of Panel B, I exclude from the push component provinces with immigrant populations that correlate with province d across all origin countries. This drops on average 36 provinces, with an average of 5 within the same autonomous community (the baseline leave-out geography). Again, the coefficients are statistically indistinguishable from my baseline results.[40] I conclude from these two exercises that the baseline leave-out categories already well exclude confounding variation from the instrument.

C.1.3 Predicting more years of immigration

The original version of the push-pull instrumental variable was developed for the United States, and drew on over a century of immigration (Burchardi et al., 2019). This long time span was necessary to capture the centuries of immigration the U.S. has experienced. Spain, by contrast has only recently become an immigrant receiving country. The two decades used to construct my push-pull instruments, 1991-2011, cover the period during which the vast

[39] Note that many countries/provinces may have correlated migration flows for reasons completely benign for my identification strategy. For example, changes in Spanish immigration policies or in transportation infrastructure may affect the push factors for several countries on different continents without affecting illegal drug trafficking. To the extent that most or all of this intercountry immigration correlation is driven by such innocuous forces, there is no a priori reason to exclude countries or provinces with correlated immigration flows from the construction of the instrument.

[40] These exercises follow Table 5 of Burchardi et al. (2019).

majority of immigrants arrived to Spain, when the foreign-born population share rose from 2 percent in 1990 to 13.5 percent in 2010. Nevertheless, to make the analysis more comparable to Burchardi et al. (2019), one might want to see more decades of immigration inflows used to predict variation in bilateral immigrant populations.

In this subsection, I supplement the 2011 and 2001 Spanish censuses with the 1981 census to construct a push-pull instrument predicting the immigration population as of 1981 by country-province. This approach collapses some of the country-level variation present in the baseline, as detailed below, and hence is not used in my main specification.

The 1981 push-pull IV construction differs slightly from equation 2 only in that I replace immigrant inflows with immigrant population:

$$IV_{o,d}^{1981} = M_{o,-a(d)}^{1981} \times M_{-c(o),d}^{1981} / M_{-c(o)}^{1981}$$

where $M_{o,-a(d)}^{1981}$ represents the immigrant population from country o living outside of autonomous community $a(d)$ as of 1981; $M_{-c(o),d}^{1981}$ measures the number of immigrants living in province d as of 1981 who came from outside continent $c(o)$; and $M_{-c(o)}^{1981}$ is the total number of immigrants living in Spain as of 1981 who don't come from continent $c(o)$.

The downside of adding more census years is that I must aggregate origins to be consistent across census waves. Since each census has a differing set of origin countries about which immigrants are asked, more years means less variation in origin countries. In particular, the number of origins that I can identify drops from 107 to 66 due to data limitations in the 1981 census.[41] It is for this reason of maximizing variability that I prefer my baseline set of instruments predicting immigration between 1991 and 2011, as well as the fact that the 1991 to 2011 period covers the vast majority of immigration into Spain.

I show the results from including the 1981 push-pull predictor in Table C.2. I find that the estimated coefficients are quite close in magnitude to those which I estimated in the baseline (found in Table 1). The coefficient on imports remains statistically significant, while the coefficient on exports narrowly misses being statistically significant at the 10 percent level.

C.2 Variation underlying the push-pull instrument

To get a sense for the variation captured by the "pull" component of the leave-out push-pull instrument, I show both time-series variation and cross-sectional variation in immigrant location decisions in Figure C.1. The top two maps of Figure C.1 show the time series variation that the leave-out push-pull instrument leverages, namely the relative attractiveness (pull) of each Spanish province by decade. To do so, I project immigrant inflows (from all

[41] If I also added in the 1991 census, I would be left with 14 origins, which is too little variation to leverage.

Table C.2: Baseline Gravity with IV Predicting 1981 Immigrant Population

	Dummy for any confiscation	
	(1)	(2)
	Imports	Imports
Log immigrants 2011	0.177	0.0638
	(0.0519)	(0.0423)
Observations	3432	3432
Country FE	Y	Y
Province FE	Y	Y
Ln dist.	Y	Y
1st-stage F-statistic	40.2	40.2

Notes: The table presents coefficient estimates from regressions of equation 1 at the country-province level. I instrument for *Log immigrants 2011* with predictions for two decades of immigrant inflows $\{IV^D_{o,d} = I^D_{o,-a(d)} \times I^D_{-c(o),d}/I^D_{-c(o)}\}_{1991-2001,2001-2011}$ as well as a prediction for the immigrant population in 1981 $IV^{1981}_{o,d} = M^{1981}_{o,-a(d)} \times M^{1981}_{-c(o),d}/M^{1981}_{-c(o)}$, two- and three-way interactions across decades, and squared terms. The dependent variable is a dummy for whether any drugs trafficked between country o and province d were confiscated between 2011 and 2016 (imports into Spain in column 1 and intended exports out of Spain in column 2). All regressions control for province and country fixed effects as well as log distance. Standard errors are clustered at the country level.

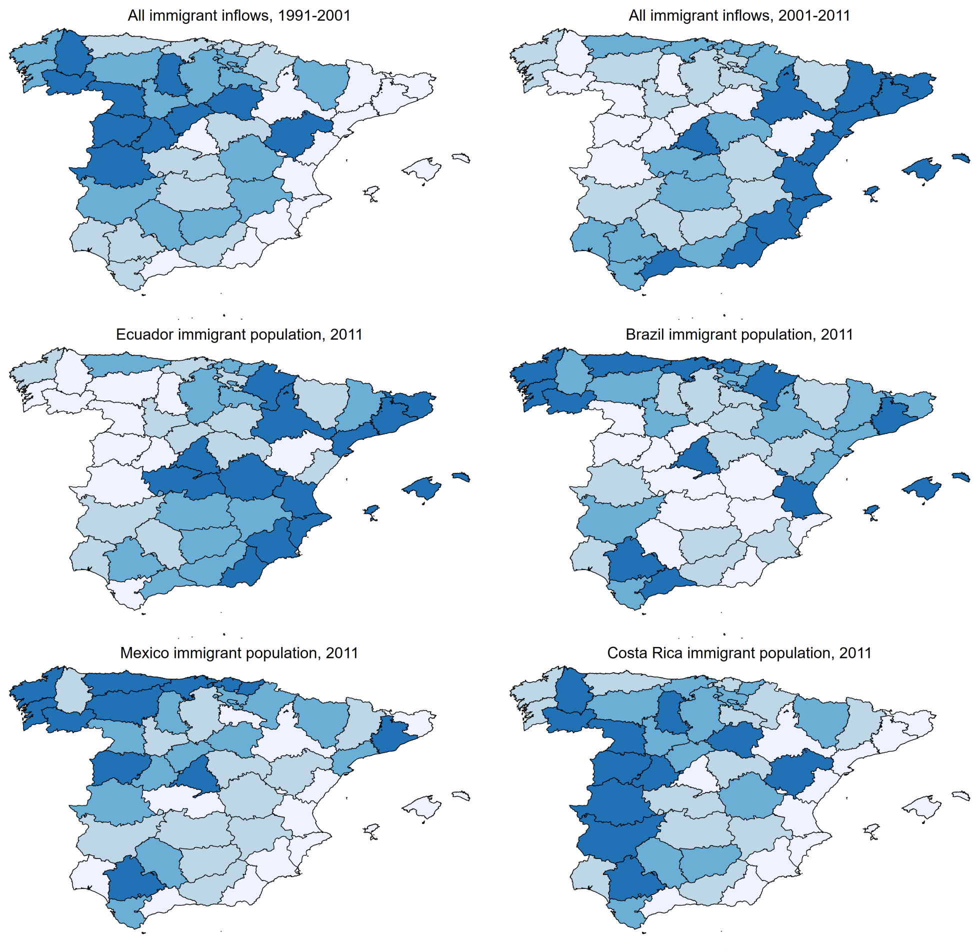
origins) on decade fixed effects and plot the residuals. The figure shows that immigrants favored inland destinations in Spain's northwest and center during the 1990s, but preferred to settle in coastal regions in the country's east during the 2000s.

There is also substantial cross-sectional variation in immigrant location decisions depending on the country of origin. I consider several countries from Latin America, a major source of both illegal drugs and immigrants for Spain. I plot the location decisions of immigrants as of 2011 for a selection of countries in the bottom four maps of Figure C.1.[42] These maps show that even for immigrants from the same continent, location decisions varied widely. Ecuadorian immigrants chose to settle in Spain's center and east, while Brazilians and Mexicans favored parts of the north and south of Spain. Costa Rican immigrants preferred to settle in the west, and unlike immigrants from the other origins, sent disproportionately fewer migrants to Madrid or Barcelona.

Moreover, immigration to Spain from Latin America peaked in different decades for different countries, as shown in Figure C.2. The figure plots the share of immigrants arriving from each Latin American country across the two decades included in the baseline analysis. The red line is the 45 degree line, indicating that Spain received disproportionately more

[42] The four countries were chosen because they all export large quantities of illegal drugs to Spain, have a substantial immigrant presence in Spain, and their immigrants have chosen to settle in different parts of the country.

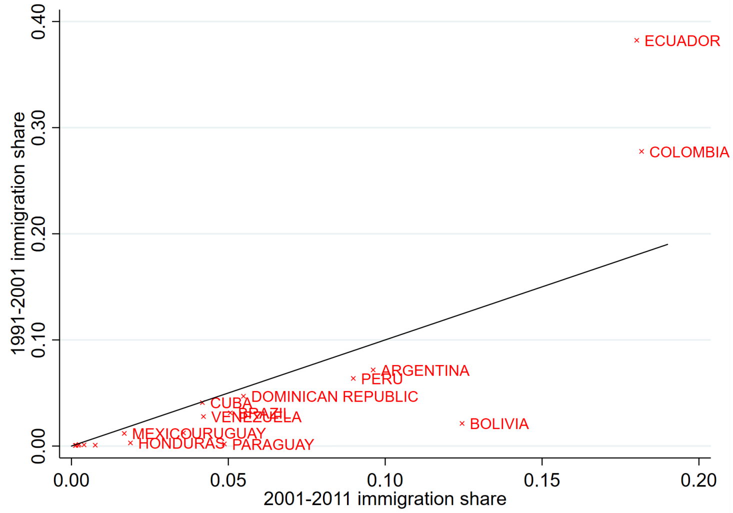
Figure C.1: Instrumental Variable Time-Series and Cross-Section Variation



Notes: The figures show immigrant location choices across Spain. The top left figure shows immigrant inflows between 1991 and 2001, while the top right figure shows immigrant inflows for the 2001-2011 period. For the top two figures, I regress the total number of immigrants (across all countries) inflowing into each Spanish province on a set of province and decade fixed effects and plot the residuals. For the bottom four figures, I regress the number of immigrants by country of origin living in the province as of 2011 on province fixed effects and a dummy for the country specified in the map title. I plot the residuals from the regression for each country in the maps. Colors correspond to the quartile of residuals for each regression, with darker colors indicating a higher quartile.

immigrants from countries below the red line in the 2000s relative to the 1990s. The graph shows that immigration into Spain from Colombia and Ecuador peaked in a different decade than immigration from the rest of Latin America. This shows that the push-pull instrumental variable is leveraging real variation within one of the major drug-sending regions.

Figure C.2: Latin American Immigration by Country and Decade



Notes: The figure plots the share of all immigrants arriving to Spain for each Latin American country. The horizontal axis refers to the share of immigrants arriving between 2001 and 2011, while the vertical axis measures the immigration share for the 1991 to 2001 decade. The black line is the 45 degree line. Hence, a country appearing on the black line send an equal propotion of immigrants to Spain in both decades relative to total immigration inflows in each decade. Countries above the black line therefore send disproportionately more immigrants in the 1990s relative to the 2000s.

C.3 Panel Estimation

I interpret my baseline cross-sectional estimates as representing the long-run effect of immigrants on drug trafficking. However, I can also estimate a panel specification to take advantage of year-to-year variation in immigration and drug trafficking. A drawback of this approach is that both immigrant population and drug trafficking may be less well measured from year to year. I must measure the bilateral immigrant population using local population registries instead of the population census. Because population registry entries may be updated with some lag and may capture some nationalities poorly (González-Enríquez, 2009), mismeasurement of the local immigrant population is a greater concern. Drug confiscations may vary wildly from year to year, as police come across a huge, multi-million dollar seizure in one year but not the next. Such variation may not reflect actual changes in drug smuggling routes. Therefore, I prefer the cross-section as my baseline with immigrant populations measured using the decennial census and drug confiscations pooled across several years.

I estimate the panel with the same specification as in the cross-sectional baseline, but adding time-superscripts:

$$Y_{o,d}^t = \alpha_o^t + \alpha_d^t + \gamma M_{o,d}^t + \zeta \ln(Dist_{o,d}) + \varepsilon_{o,d}^t \tag{C.1}$$

where $Y_{o,d}^t$ is a dummy for whether any illegal drugs were confiscated by Spanish authorities, either imports or exports. $M_{o,d}^t$ is defined as before and measured using annual tabulations taken from Spain's local population registries at the country-province-year level. The α_o^t and α_d^t fixed effects absorb time-varying factors at the country or province level which drive immigration and drug trafficking.[43]

I also estimate the panel including country-province fixed effects:

$$Y_{o,d}^t = \tilde{\alpha}_o^t + \tilde{\alpha}_d^t + \tilde{\gamma} M_{o,d}^t + \alpha_{o,d} + \tilde{\varepsilon}_{o,d}^t \tag{C.2}$$

These fixed effects absorb time-invariant bilateral characteristics, such as climatic or geographic similarity. However, the bilateral fixed effects $\alpha_{o,d}$ also absorb average bilateral immigrant population size, and thus change the interpretation of the coefficient on $M_{o,d}^t$. In particular, $\tilde{\gamma}$ represents the change in illegal drug trafficking resulting from year-to-year net changes in the immigrant population. Therefore, equation C.2 sheds light on the effect of recent immigrants on illegal trafficking, but it does not test whether migrant networks shape illegal trafficking, the central question of this paper.

To achieve causal identification, I use the instrumental variable defined in equation 2 for the decade 1991–2001 when estimating equation C.1:

$$IV_{o,d}^{1991-2001} = I_{o,-a(d)}^{1991-2001} \times \frac{I_{-c(o),d}^{1991-2001}}{I_{-c(o)}^{1991-2001}} \tag{C.3}$$

In addition, I include a time-varying instrument that predicts bilateral immigrant inflows between 2001 and year t when estimating both equations C.1 and C.2,

$$IV_{o,d}^t = I_{o,-a(d)}^{2001-t} \times \frac{I_{-c(o),d}^{2001-t}}{I_{-c(o)}^{2001-t}} \tag{C.4}$$

I compute immigrant inflows between 2001 and t as the net change in the bilateral immigrant population as measured in the population registry. Consistent with my baseline specification, I include interaction and squared terms when estimating the first-stage. I estimate equations C.1 and C.2 for the years 2006 through 2016.[44]

As shown in Table C.3, I find that immigrants raise imports and exports, consistent with my baseline results. For imports, a 10% increase in the population of immigrants

[43] These fixed effects also nonparametrically absorb province- or country-specific time-trends.

[44] I start the time series in 2006 because of unexplained gaps in the drug confiscations data in earlier years, suggesting that reporting of drug confiscations was not consistent in this earlier period.

from country o raises the likelihood of import confiscations by 1 percentage point using the coefficient shown in column 1.[45] When including the $\{o, d\}$ fixed effect, I find that a 10% increase in recent net migration from country o raises the likelihood of drug imports from o by 2 percentage points.[46] For exports, a 10% increase in immigrants from country o raises the likelihood of illegal drug exports to o by 0.2 percentage points. When controlling for the bilateral fixed effect, the effect of net immigration on exports of illegal drugs is statistically indistinguishable from 0, suggesting that recent immigrants do not facilitate the export of illegal drugs.

Table C.3: Effect of Immigrants on Drug Trafficking: Panel Analysis

	Drug Confiscations Dummy			
	(1)	(2)	(3)	(4)
	Imports	Imports	Exports	Exports
Log immigrant population	0.194	0.359	0.0325	-0.0750
	(0.0138)	(0.174)	(0.00779)	(0.146)
Observations	58916	58916	58916	58916
Log distance	Y	Y	Y	Y
Country-Year FE	Y	Y	Y	Y
Province-Year FE	Y	Y	Y	Y
Origin-Province FE	N	Y	N	Y
1st-stage F-statistic	521.7	34.8	521.7	34.8

Notes: The table presents estimates of equation C.1 in columns 1 and 3 and equation C.2 in columns 2 and 4 at the country-province-year level. I instrument for the immigrant population using predicted flows defined in equations C.3 (for columns 1 and 3 only) and C.4 as well as their second-order interactions and squared terms. Standard errors are clustered by country-year in parentheses.

C.4 Testing Instrument Exogeneity

Recent work on identification using shift-share instrumental variables has emphasized that shift-share instruments should be uncorrelated with exogenous, predetermined characteristics (Goldsmith-Pinkham et al., 2020; Borusyak and Hull, 2021; Borusyak et al., 2022). While the leave-out push-pull instrument used in this paper does not precisely match a canonical shift-share instrument, the logic still holds that the push-pull IV should be uncorrelated with exogenous characteristics at the country-province pair level.[47]

[45] Using $\hat{\gamma} = 0.192$ from column 1 in Table C.3 and the average country-province-pair immigrant population of 1229, I compute: $\mathbb{1}\left[C_{o,d}^{2011-2016} > 0 | M_{o,d}^{2011} = 1229\right] = 0.192\left(\ln\left(1 + \frac{1229 \times 1.1}{1000}\right) - \ln\left(1 + \frac{1229}{1000}\right)\right) \approx 0.0103$.

[46] As noted above, controlling for $\alpha_{o,d}$ changes the interpretation of β from the effect of changes in bilateral immigrant population to net changes in year-to-year immigrant inflows.

[47] A shift-share instrument typically interacts an initial share with a change over time, summed across one dimension. The push-pull instrument has two differences from this canonical formulation. First, the push-

In the spirit of the "pre-trend" tests recommended by Goldsmith-Pinkham et al. (2020), I regress the instrument from each decade on the log distance, the only predetermined variable available at the country-province pair level. As shown in Table C.4, I find no relationship between the instruments and log distance. Since the leave-out structure ensures that country-province pair specific immigration—as well as neighboring immigration flows—are not used in the instrument, this should not be surprising, though still reassuring that the instruments are not capturing pre-existing bilateral characteristics. For example, Moroccans tend to settle on Spain's southern coast, the provinces in closest proximity to Morocco. The leave-out push-pull instrument will, however, predict Moroccan immigration to those southern provinces from immigration from Europe, Asia, the Americas, and Oceania, origins with widely varying distances to southern Spain.

Table C.4: Relationship between Instruments and Log Distance

	Dep. Var.: Leave-out Push-Pull IV	
	(1)	(2)
	1991-2001	2001-2011
Log distance	0.0267	0.0216
	(0.0321)	(0.0609)
Observations	5564	5564
Country FE	Y	Y
Province FE	Y	Y

Notes: The table presents estimates OLS regressions of the leave-out push-pull IV (defined in equation 2) on the log distance between Spanish province and origin country. Data are at the country-province level. The dependent variable is the leave-out push-pull IV for the 1991-2001 decade in column 1, and for the 2001-2011 decade in column 2. Standard errors are clustered by country.

Second, I implement a test inspired by Borusyak and Hull (2021). The concern this test addresses is that some unobserved bilateral characteristic (e.g., transit connections) may be driving immigration higher for a country-province pair. The concern is particularly heightened when, for example, a country always sends huge flows of immigrants to a particular destination due to there being good bilateral transit connections. The solution is to then consider the potential distribution of immigration push and pull shocks, take the average, and demean the baseline push-pull instruments by this average over potential predicted flows.

Specifically, I compute four versions of the push-pull instrument: 2 factual and 2 counterfactuals. The factual instruments are equation 2 evaluated in decades 2001 and 2011; and the counterfactual instruments are interactions of push and pull components across decades:

pull IV stops short of summing across any dimension, retaining both dimensions of variation. Second, the "share" component of the push-pull IV (equivalent to the "pull" component) is not lagged, as in the canonical shift-share immigration instrument, but rather computed for the same time period with a leave-out structure.

$IV^3_{o,d} = I^{2001}_{o,-a(d)} \times \frac{I^{2011}_{-c(o),d}}{I^{2011}_{-c(o)}}$ and $IV^4_{o,d} = I^{2011}_{o,-a(d)} \times \frac{I^{2001}_{-c(o),d}}{I^{2001}_{-c(o)}}$. I then compute the arithmetic mean of these four instruments and subtract it from each of the baseline push-pull instruments.[48] The demeaned instruments then capture the deviation of predicted immigration flows from what would have been expected had a different set of likely shocks (i.e., predicted immigration flows between the same origin and same destination but using push and pull components from different decades) occurred.

I show the baseline specification re-estimated using these demeaned instruments in Table C.5. The results are quantitatively very close to those estimated using the baseline instruments (Table 1). This exercise reinforces the notion that time-invariant bilateral factors are not driving my results.

Table C.5: Baseline Results, Adjusting for Borusyak and Hull (2021) mean IV values

	Dummy for Any Drug Confiscations	
	Imports	Exports
Log immigrants 2011	0.172	0.0629
	(0.0461)	(0.0348)
Observations	5564	5564
First-stage F-stat.	150.6	150.6
Country FEs	Y	Y
Province FEs	Y	Y
Log distance	Y	Y

Notes: The table presents estimates of IV regressions at the country-province level. The dependent variable is a dummy for whether any confiscation occurred, separately for imports (column 1) and exports (column 2). I instrument for the immigrant population using $\{IV^D_{o,d} = I^D_{o,-a(d)} \times I^D_{-c(o),d}/I^D_{-c(o)}\}$ using the 2001 and 2011 censuses, their interactions across decades, and squared terms, where each instrumental variable is demeaned by its average value across decades within country-province pair. Standard errors are clustered by country.

C.5 Enforcement Intensity

As with many studies of illegal behavior, I only observe drugs which are confiscated by police. The resulting dataset of drug confiscations is therefore a result of willful actions taken by criminals to hide their actions and of police to uncover those actions (Pinotti, 2020). In my context, immigrant networks may either make police discovery of drug trafficking easier (as police target searches to larger populations of immigrants) or harder (as immigrant networks spread knowledge about avoiding detection). I conduct two exercises to assess the importance of the enforcement intensity channel in explaining my results.

[48] Here I assume each push and pull shock has an equal chance of occurring across decades.

C.5.1 Extensive Margin Gravity Exercise

In my baseline gravity estimation, I use a set of country and province fixed effects to control for policing enforcement intensity specific to each Spanish province (and common across all origins) as well as for enforcement intensity specific to each origin country (but common to all Spanish provinces). Moreover, in Section 2.3 I showed that drug confiscations correspond to drug use and availability at the province level, suggesting that confiscations correspond to actual illegal drug imports.

In this section, I present an additional exercise meant to characterize the degree to which measurement error confounds my baseline estimates. The exercise focuses on the set of country-province pairs which I predict to be on the margin of drug trafficking to quantify how much (under certain assumptions) enforcement intensity drives my results.

To quantify the extent to which such bilateral enforcement intensity affects my baseline estimates, I start from the intuition that for country-province pairs near the extensive margin of trafficking drugs, enforcement changes caused by variation in the number of immigrants will not substantially affect confiscations. To formalize the intuition, note that confiscations are a product of enforcement intensity and actual drug flows:

$$C_{o,d} = E_{o,d} D_{o,d} \tag{C.5}$$

where $C_{o,d}$ is the number of drug shipments confiscated between o and d, $E_{o,d}$ is the fraction of drug shipments confiscated, and $D_{o,d}$ is the actual number of drug shipments from o to d. Taking the derivative of equation C.5 with respect to the number of immigrants, I obtain

$$\frac{dC_{o,d}}{dM_{o,d}} = E_{o,d}\frac{\partial D_{o,d}}{\partial M_{o,d}} + D_{o,d}\frac{\partial E_{o,d}}{\partial M_{o,d}} \tag{C.6}$$

Conditional on the set of fixed effects (α_o, α_d) in my baseline gravity estimation of equation 1, I have thus far implicitly assumed that $\frac{\partial E_{o,d}}{\partial M_{o,d}} = 0$. This assumption allowed me to estimate the object of interest, $\frac{\partial D_{o,d}}{\partial M_{o,d}}$. Alternatively, I could fix $D_{o,d}$ to be near zero and instead relax this assumption from $\frac{\partial E_{o,d}}{\partial M_{o,d}} = 0$ to $\frac{\partial E_{o,d}}{\partial M_{o,d}} < \infty$.[49] The challenge is subsetting my sample to the country-province pairs in which actual drugs trafficked $D_{o,d}$—which I do not observe—are near zero, and therefore on the extensive margin.

I construct a prediction of actual bilateral flows $D_{o,d}$ based upon a leave-out measure of confiscations. The intuition of the predictor works as follows: suppose Barcelonan police more intensively enforce anti-drug trafficking laws against Moroccan immigrants (relative

[49] Akee et al. (2014) similarly focus on the extensive margin when estimating the determinants of transnational human trafficking.

to other provinces or other nationalities) due to their large local group size. Then data on confiscations in Barcelona will include a disproportionate sample of drugs coming from Morocco (relative to the actual share of true drug flows). To strip out this discrimination from the bilateral confiscations data, I look at how (i) Barcelona confiscates drugs coming from outside Africa, and (ii) how other provinces outside Catalonia confiscate drugs coming from Morocco.

Specifically, to predict when actual flows $D_{o,d} \approx 0$, I use a similar leave-out push-pull structure for confiscations between 2011 and 2016 as I did for immigrant inflows in equation 2:

$$\hat{D}_{o,d} = C_{o,-a(d)} \times \frac{C_{-c(o),d}}{C_{-c(o)}} \tag{C.7}$$

where C denotes the number of confiscation events. $\hat{D}_{o,d}$ interacts the number of drug confiscations originating from o but confiscated outside the autonomous community of d with the fraction of all drug confiscations from outside o's continent confiscated in d. Implicit in this formulation is the assumption that (i) on average, law enforcement in province d will discriminate differently against immigrants from continents outside of $c(o)$, and (ii) on average, law enforcement in other autonomous communities will discriminate differently against immigrants from o.

To gauge the extent to which enforcement intensity variation may affect my results, I re-estimate equation 1 for the subset of observations for which I predict that $D_{o,d}$ is near 0. I show results in Table C.6 subsetting to bilateral links that I predict having at most 1 confiscation event. While the point estimates fall when subsetting to the sample predicted to be on the extensive margin, the extensive margin estimate in column 2 remains statistically significantly positive, suggesting enforcement variation cannot fully explain my bilateral results. For the results on exports shown in columns 3 and 4, I find a modest decline in the coefficient, with a loss of statistical significance in column 4.

C.5.2 Province-level Variation

It may be the case the police capture a larger share of incoming illicit drugs when there are more immigrants, especially irregular immigrants, living locally. While one cannot directly observe the share of illicit drugs captured, I propose[50] a rough proxy for this share to be the ratio of drug confiscations per capita and the share of EDADES survey respondents reporting that it is easy to obtain cannabis or cocaine locally.

[50]Thanks to an anonymous referee for suggesting this exercise.

Table C.6: Effect of Immigrants on Drug Confiscations: Extensive Margin

	Imports Confiscations (Dummy)		Exports Confiscations (Dummy)	
	(1)	(2)	(3)	(4)
Log immigrants 2011	0.172	0.111	0.0625	0.0194
	(0.0461)	(0.0419)	(0.0347)	(0.0227)
Observations	5564	5159	5564	5443
R^2	0.053	0.039	0.019	0.009
Country FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y
Ln dist	Y	Y	Y	Y
1st-stage F-statistic	152.4	352.6	152.4	182.9
Sample	All	≤ 1 predicted confiscations	All	≤ 1 predicted confiscations

Notes: The table presents coefficient estimates from IV regressions of equation 1 at the country-province level. I instrument for *Log immigrants 2011* with $\{IV^D_{o,d} = I^D_{o,-a(d)} \times I^D_{-c(o),d}/I^D_{-c(o)}\}_{1991-2001,2001-2011}$, their interaction across decades, and squared terms. In column 2, I subset the sample to the set of country-province pairs for which the number of predicted confiscations (defined in equation C.7) is less than or equal to 1; I do the same for predicted export confiscations in column 4. Standard errors are clustered by country.

I estimate the equation

$$\frac{Confisc_{dt}}{Ease_{dt}} = \exp(\alpha MigrShr^{\beta}_{dt} X^{\delta}_{dt} \epsilon_{dt}) \tag{C.8}$$

where $Confisc_{dt}$ is the per capita value of drug confiscations in province d in year t; $Ease_{dt}$ is the share of EDADES respondents in d in year t who report it being relatively easy or very easy to obtain cannabis or cocaine[51]; $MigShr_{dt}$ is the share of the population of d in t who are immigrants, either regular or irregular; and X_{dt} is a vector of controls. An estimate of $\hat{\beta} > 0$ would be consistent with a higher share of immigrants raising police enforcement intensity.

I estimate equation (C.8) using PPML and report results in Table (C.7).[52] Column 1 shows the estimated coefficient from the bivariate PPML with the irregular immigrant population share. The estimated relationship is not statistically significant. In column 2 I estimate the bivariate relationship with regular immigrants and also find no statistically significant relationship.

Because the immigrant share, drug confiscations, and the ease of access to drugs may be jointly driven by the local regular immigrant population, as suggested by my gravity estimates in Section (5), I additionally control for the regular immigrant population share in column 3. The coefficients fall in magnitude and remain statistically indistinguishable from

[51]This ease of access variable is only available by drug, so I aggregate to the province level by taking a simple average of the ease of access across both drugs.

[52]Results are qualitatively identical if estimating an OLS version of equation (C.8).

Table C.7: Relationship between Confiscation-Ease of Access Ratio and Irregular Immigrant Share

	Dep. Var.: Ratio of Confiscations to Ease of Acess to Drugs					
	(1)	(2)	(3)	(4)	(5)	(6)
Log irregular migr. pop. share	0.27		0.16	0.26	-0.41	-0.90
	(0.40)		(0.64)	(0.67)	(0.45)	(0.47)
Log regular migr. pop. share		0.34	0.21	0.17	0.088	-0.67
		(0.32)	(0.63)	(0.73)	(0.50)	(1.13)
Observations	312	312	312	312	288	288
Year FE				Y		Y
Province FE					Y	Y

Notes: The table presents coefficient estimates from regressions at the province level. The dependent variable is defined as the annual value of drugs confiscated per capita divided by the share of EDADES reports reporting that it is relatively easy or very easy to obtain cannabis or cocaine. Standard errors are clustered at the province level.

0.[53]

To control for changes over time in national immigration and drug confiscations, I add year fixed effects in column 4 and continue to find a modest, statistically insignificant relationship. Time invariant characteristics of a province, such as its geography or average economic performance may confound the estimated relationship. To address this concern, I add province fixed effects in columns 5 and 6. The coefficients on the irregular and regular immigrant population shares remain statistically significant.[54] Overall these results do not suggest a strong relationship between the local immigrant population share or composition and the efficacy of local police in seizing illegal drugs.

C.6 Heterogeneity Across Subsamples

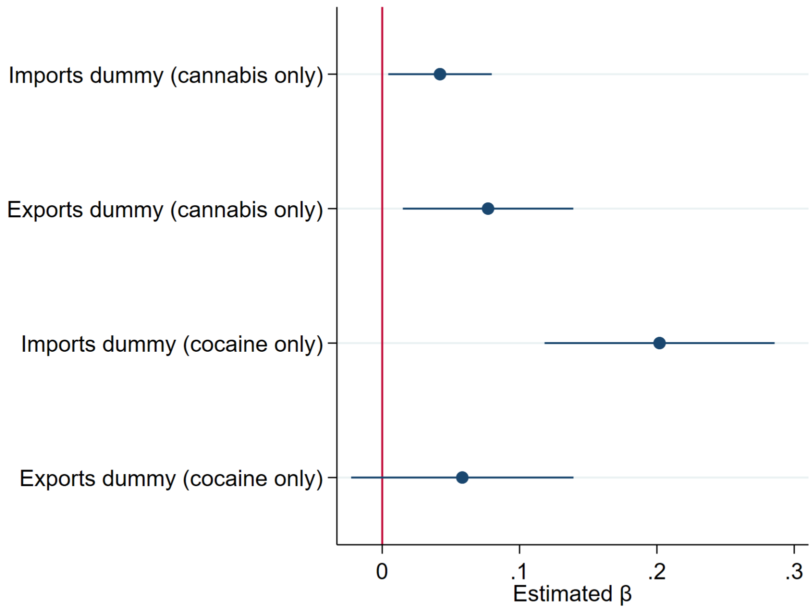
C.6.1 Heterogeneity by drug

Finally, I estimate the immigrant-confiscations relationship separately by the two major drugs trafficked in Spain: cannabis and cocaine, as shown in Figure C.3. I estimate a positive effect of immigrants on both the import and export of both drugs. Cocaine imports are raised by immigrants moreso than cannabis imports, consistent with the fact that cocaine must be imported, whereas cannabis may be produced locally in Spain.

[53] The correlation coefficient between the regular and irregular log population share is 0.63, so collinearity is not so severe.

[54] The sample size falls because 24 observations are separated in the sense of Correia et al. (2019), that is, the dependent variable is always 0 within certain fixed effects.

Figure C.3: Effect of Immigrants on Drug Trafficking by Drug



Notes: The figure shows IV estimates of the effect of immigrants on drug trafficking (β from equation 1) for the two major drugs trafficked in Spain: cocaine and cannabis (see Figure D.1).

C.6.2 Dropping major drug senders

Morocco and Latin America are overrepresented both in the volume of drugs confiscated by Spanish authorities (as shown in Appendix Figure D.5) and in the flows of immigrants into Spain. One may worry that these countries drive most of my results, and that the phenomenon described in this paper is not broad based.

First, I note that even excluding these origin regions, there remains a substantial number of country-province pairs with some drug trafficking. In particular, the fraction of country-province pairs with at least one import confiscation falls from 7.7 percent to 2.9 percent. While this is a significantly lower share, it is still substantially above zero.

To show that more immigrants from other regions still correspond to a greater likelihood of drug trafficking from that region, I re-estimate my baseline specification on imports while excluding Morocco and all Latin American countries from the set of origins. I show the results in Table C.8.

In column 1, I drop Morocco only; the coefficient rises relative to my baseline results in Table 1. When dropping Latin American countries, as I do in Column 2, the coefficient falls. Finally, dropping both Morocco and Latin America (column 3) yields a coefficient very similar to my baseline estimate. In all cases, the 95% confidence interval includes the

Table C.8: Baseline Import Results, Excluding Morocco & Latin American

	Dummy for Any Drug Confiscations		
Log immigrants 2011	0.210	0.0977	0.151
	(0.0442)	(0.0457)	(0.0506)
Observations	5512	4576	4524
First-stage F-stat.	220.7	194.3	322.4
Country FEs	Y	Y	Y
Province FEs	Y	Y	Y
Log distance	Y	Y	Y
Sample	Drop Morocco	Drop Lat. Am.	Drop Morocco, Lat. Am.

Notes: The table presents estimates of IV regressions at the country-province level. The dependent variable is a dummy for whether any import confiscation occurred. In column 1, I drop Morocco from the sample; in column 2, all countries in Latin America; and in column 3, both Morocco and Latin America. I instrument for the immigrant population using $\{IV^D_{o,d} = I^D_{o,-a(d)} \times I^D_{-c(o),d}/I^D_{-c(o)}\}$ using the 2001 and 2011 censuses, their interactions across decades, and squared terms. Standard errors are clustered by country.

coefficient estimated in my baseline.

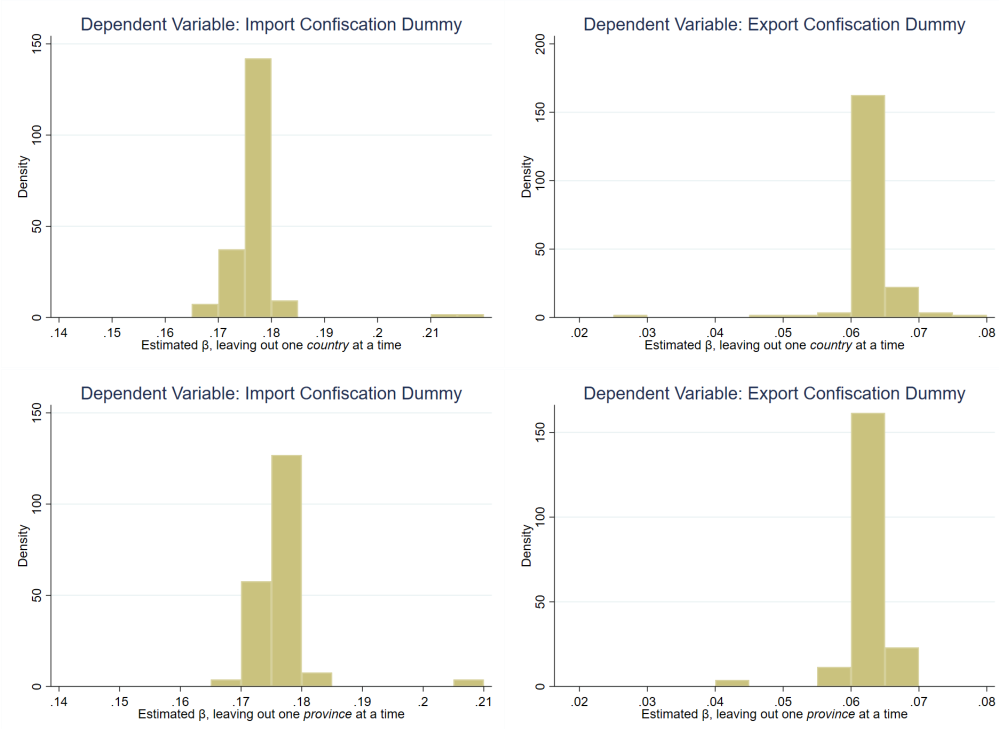
C.6.3 Heterogeneity by country/province

Drug trafficking into Spain is primarily driven by a select few countries—Morocco, for example, is the dominant exporter of cannabis to Spain. To see whether any single country or province drives my baseline results, I re-estimate my baseline gravity specification separately leaving out each province and each country, for a total of 159 regressions. Figure C.4 shows the distribution of β estimates from equation 1 when dropping a single country per regression (top row) or a single province (bottom row), with imports on the left and exports on the right. I estimate a positive β regardless of which region I drop from the sample, suggesting that no single country (including Morocco) or province drives my baseline results.

Moreover, the maximal standard error for the excluded country import regressions (top left) is 0.049, meaning all estimated coefficients are statistically significant as well. For exports (top right), all coefficients are positive and all but four coefficients are statistically significant at the 10% level or lower. Similarly for leaving out one province at a time (bottom row), import coefficients are always statistically significant. Export coefficients when leaving one province out at a time are statistically significant at the 10% level or lower for all but one regression.

I also explore the heterogeneity of the effect of immigrants on drug trafficking across individual countries and Spanish provinces. In Figure C.5, I plot coefficients of the immigrant population's effect on imports (left column) and exports (right column) across provinces (bottom) and countries (top). The red curve displays the threshold for statistical signifi-

Figure C.4: Effect of Immigrants on Drug Trafficking: Dropping Countries, Provinces One at a Time

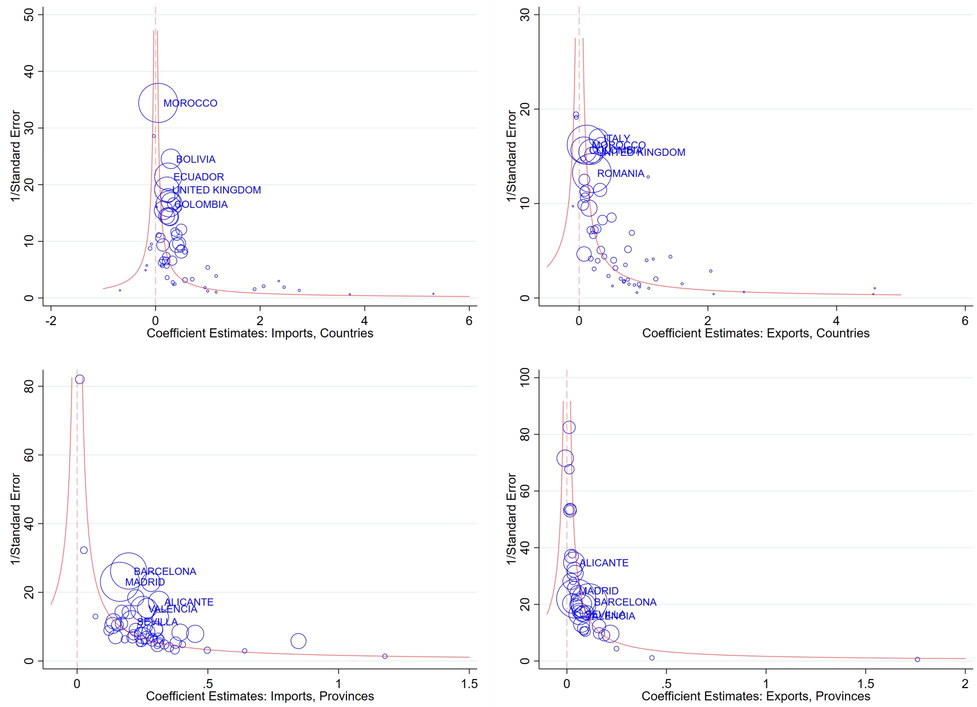


Notes: The figures show the distribution of the estimated effect of immigrants on illegal drug confiscations (β from equation 1) when leaving out one nationality (in the top row) or province (bottom row) for each regression. The figures show the distribution of βs when the dependent variable of equation 1 is a dummy for whether any drug trafficking (imports on the left and exports on the right) with a given origin country was confiscated locally between 2011 and 2016.

cance at the 10% level, with circle size corresponding to province population or immigrant nationality population. I find that nearly all individual provinces and countries exhibit a positive effect of immigrants on illegal trafficking, with most coefficients being statistically significant. However, given that each province regression is identified from 107 observations and each country regression from 52 observations, it is unsurprising that some estimates are statistically insignificant.

Finally, I explore heterogeneity in the effect of immigrants by whether the immigrants' origin country is one of the three in the world that produce cocaine: Bolivia, Colombia, or Peru. I explore this question in Table C.9. I find that cocaine trafficking from cocaine producers is significantly more sensitive to the immigrant population size relative to non-cocaine producers (column 1). This difference is statistically significant at the 5% level. Notably, immigrants from non-producing countries still affect cocaine trafficking, suggesting a role of immigrants in trade diversion. As a placebo check, I run the same regression on cannabis trafficking and reassuringly find no significant difference (column 2).

Figure C.5: Heterogeneous Effect of Immigrants on Drug Trafficking by Country and Province



Notes: These figures shows funnel plots of the estimated coefficients and inverse standard errors from 2SLS regressions of drug trafficking dummies (imports in top charts; exports in bottom charts) on *Log migrants* 2011, controlling for log distance between the immigrants' origin country and Spanish province, and estimated separately for each individual Spanish province and origin country. Circle sizes represent the province population (left-hand charts) or the number of immigrants in Spain from the origin country (right-hand charts). Labeled countries/provinces are the top 5 largest by population in Spain. The x-axis is the coefficient estimate, and the y-axis is the inverse of the standard error of that estimate. The curve plots $y = \pm 1.65/x$; hence, circles above this curve are statistically significant at the 10% level. I separately drop countries or provinces for which I observe no import or export confiscations. For readability, I drop China in the top right chart and Ukraine in the bottom right chart, which are both major outliers, though both coefficients are statistically indistinguishable from zero.

C.7 General Equilibrium Responses

While I have shown that immigrants increase drug trafficking with their home country, this effect may be offset by general equilibrium adjustments to immigrant-induced trafficking. For example, trafficker immigrants from one country may reduce their trafficking in response to more immigration (and more trafficking) from another country. If such adjustments offset the effect of immigrants on trafficking, then there should be no effect when aggregating across origin countries. To assess the strength of the general equilibrium response, I conduct two exercises.

First, I regress the immigrant population share on drug market activity at the province

Table C.9: Effect of Immigrants on Drug Trafficking: Heterogeneity by whether Country Produces Cocaine

	(1) Cocaine	(2) Cannabis
Cocaine Producer$_o$=0 × Log immigrants 2011	0.181	0.0414
	(0.0479)	(0.0211)
Cocaine Producer$_o$=1 × Log immigrants 2011	0.281	0.0346
	(0.0246)	(0.0277)
Observations	5564	5564
Country FE	Y	Y
Province FE	Y	Y
Ln dist.	Y	Y
1st-stage F-statistic	73.5	73.5

Notes: The table presents coefficient estimates from regressions at the country-province level. I instrument for *Log immigrants 2011* with $\{IV^D_{o,d} = I^D_{o,-a(d)} \times I^D_{-c(o),d}/I^D_{-c(o)}\}_{1991-2001,2001-2011}$, their interaction across decades, and squared terms, plus each of those terms interacted with a dummy for o being a cocaine producer (Colombia, Peru, or Bolivia). The dependent variable is a dummy for whether any drugs trafficked from country o into province d were confiscated between 2011 and 2016, separately for cocaine (column 1) and cannabis (column 2). All regressions control for province and country fixed effects as well as log distance. Standard errors are clustered at the country level.

level. I start by estimating the effect of immigrants on confiscations of illegal drugs, illegal drug use, and drug trafficking arrests with a panel of Spanish provinces. In particular, I estimate

$$\frac{Y_d^t}{Pop_d^t} = \alpha_d + \alpha^t + \beta \frac{Migr_d^t}{Pop_d^t} + \epsilon_d^t \tag{C.9}$$

for some measure $\frac{Y_d^t}{Pop_d^t}$ of per capita illegal drug activity and the fraction of immigrants in the population $\frac{Migr_d^t}{Pop_d^t}$ for province d in year t. I also control for province and year fixed effects.

There may still be factors affecting both immigration and drug smuggling into a province net of these fixed effects. For example, if immigrants are attracted to regions with rising incomes, and drug traffickers also establish connections to regions with rising incomes (and therefore an expanding market for drugs), then a spurious correlation between immigration and drug trafficking may arise. Therefore, I instrument for the immigrant population using a version of the popular ethnic enclave instrument developed by Card (2001). Specifically, I instrument for the immigrant population share with

$$IV_d^t = \frac{1}{\widehat{Pop}_d^t} \sum_o \frac{Migr_{o,d}^{1981}}{Migr_o^{1981}} Migr_o^t \tag{C.10}$$

where $Migr_o^t$ refers to the number of immigrants from o living in Spain in year t. $\widehat{Pop}_d^t$ is the predicted population of province d in year t. I predict the population following Mayda et al. (2022). First I predict immigrant inflows by summing over origin countries the interaction between the initial immigrant population share and the national change in immigrants from that origin[55]: $\Delta\hat{X}_d^t = \sum_o \frac{X_{o,d}^{1981}}{X_o^{1981}}(X_o^t - X_o^{t-1})$ for X referring to either native-born or immigrants and for each year of data available. Next, I add these predicted immigrant flows and native population changes to the observed 1981 migrant or native populations, respectively. Summing the predicted migrant and native populations together yields $\widehat{Pop}_d^t$ for each province and year. I use data from the years 2006 to 2016.[56]

I show the results of estimating equation C.9 in Table C.10 for a variety of indicators of the local illegal drug market. I show the first-stage regression results in column 1. The ethnic enclave instrument defined in equation C.10 positively and statistically significantly predicts the local share of immigrants. I next estimate the effect of immigrant population share on the per capita value of drugs confiscated in province d in year t for imports (column 2) and exports (column 3). Column 2 of Table C.10 shows the result for imports, and column 3 for exports. I find that an increase in the local migrant population share of 10 percentage points raises per capita confiscations of illegal drug imports by \$107 (SE=\$49). I find an imprecise effect of the immigrant population share on intended export confiscations per capita. These results suggest that more immigrants in a region raise total drug imports into that region relative to other regions.

[55]Following Card (2001) and Mayda et al. (2022), I collapse origin countries into 16 groups. These groups are the top 8 immigrant sending countries (Italy, Venezuela, Argentina, United Kingdom, Portugal, France, Morocco, and Cuba), and the remaining countries in Western Europe, Eastern Europe, South American, Central American and the Caribbean, the U.S. and Canada, Africa, Asia, and Australia and Oceania.

[56]I start the time series in 2006 in part because the data on drug use does not start until 2005. Moreover, there are several months of zero reported confiscations prior to 2006, suggesting that reporting of drug confiscations was not consistent in this earlier period.

Table C.10: Effect of Immigrants on Illegal Drug Activity (Province-level)

	(1) First-Stage: Share immigrants	(2) 2SLS: value imports confiscated per-capita	(3) 2SLS: value exports confiscated per-capita	(4) 2SLS: shr. native-born used drugs last 12 mo.	(5) 2SLS: shr. native-born ever used drugs	(6) 2SLS: native-born drug trafficking arrests per-capita	(7) 2SLS: cannabis plant confiscations per-capita
Ethnic Enclave IV	8.535						
	(2.601)						
Migr. pop. share		1067.8	-176.5	-0.0391	0.832	-0.00231	0.0272
		(489.7)	(185.6)	(0.344)	(1.094)	(0.00129)	(0.0253)
Observations	572	572	572	260	260	364	50
First-stg. F-stat.	10.8	10.8	10.8	6.7	6.7	7.7	7.8

Notes: The table presents coefficient estimates from IV regressions of equation C.9 at the province-year level. I instrument for *Migr. pop. share* using the excluded instrument defined in equation C.10, with the first-stage shown in column 1. The dependent variable in column 2 is the value of illegal drug imports and in column 3 exports confiscated per capita. The dependent variable of columns 4 and 5 is the share of native-born Spaniards reporting to the EDADES survey that they used drugs in the last 12 months (column 4) or ever (column 5). The dependent variable of column 6 is the number of Spanish citizens arrested for illegal drug trafficking per capita. Column 7 shows results using the number of cannabis plants seized per capita as the dependent variable, which is only available for a single cross-section. Per capita values are relative to the 1981 province population. Standard errors are clustered at the autonomous community-by-year level.

As an alternative measure of local drug supply, I next turn to local illegal drug consumption. I estimate equation C.9 with dependent variable of the share of the native-born using illegal drugs, measured using the biennial EDADES survey described in Section 2.3. I find no statistically significant effect of immigrants on the drug use of the native-born as shown in columns 4 and 5 of Table C.10, though due to the biennial nature of the survey, the sample size and therefore estimation precision fall significantly.

To get a sense for whether immigrant labor is substituting for the native-born in high-immigration provinces, I examine how per capita native-born drug trafficking arrests change with immigration in column 6. Using a panel of arrests data between 2010 and 2016, I find that increased immigration drives down arrests of native-born Spaniards for drug trafficking, although the coefficient magnitude is small. This result suggests that immigrants may, to a modest extent, push the native-born out of the drug trafficking business.

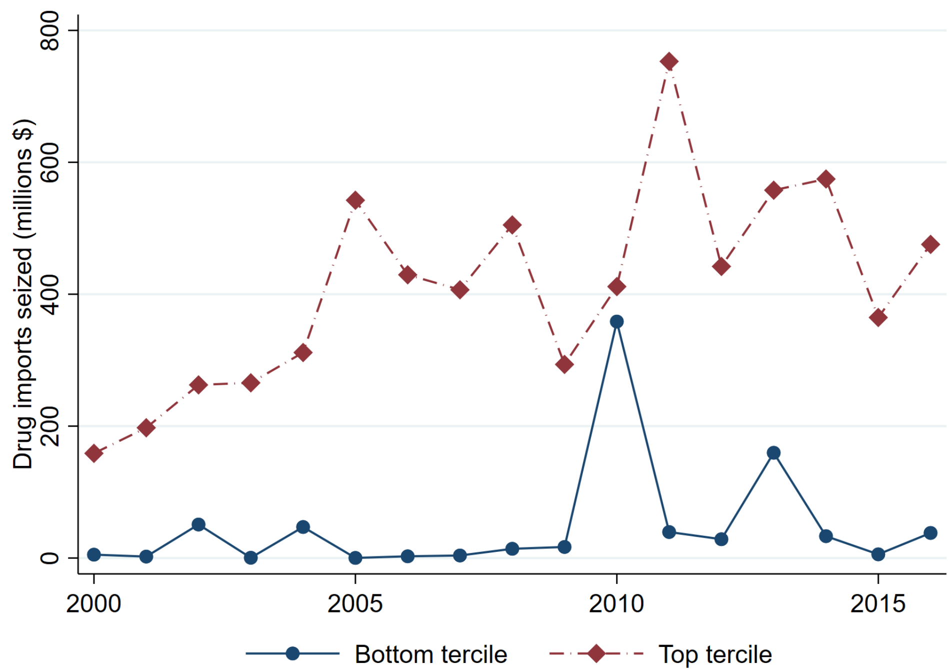
I finally look at how immigration affects the per capita cultivation of cannabis plants within Spain.[57] As immigration reduces trade costs, one may expect trade to displace local production. Alternatively, an increased labor supply (in the form of immigration) may reduce the costs of production, thereby increasing local cannabis production. I find no statistically significant effect of immigrants on cannabis plant confiscations using a cross-section of plant confiscations data.[58]

As a second exercise, in Figure C.6, I plot the evolution of drug import confiscations over time between low and high immigration Spanish provinces. If general equilibrium reallocation were dominant, one would expect that low immigrant population provinces to experience a decline in drug imports to offset an increase in high immigrant population provinces. In contrast, the plot demonstrates that provinces in the lowest tercile of immigrant population share as of 2000 (the solid blue line) experience no significant change in drug confiscations, while high immigrant population provinces (plotted with the red dash-dot line) experienced significant increases in total confiscations. While a lack of controls and exogenous variation means alternative stories can explain the patterns displayed in the chart, the results are nonetheless suggestive that general equilibrium reallocation effects may not override the well-identified causal effects estimated in Section 3 using the gravity equation.

[57]Spain produces a small but non-trivial amount of cannabis. Alvarez et al. (2016) find that in 2013, authorities confiscated almost 200,000 cannabis plants growing in Spain. Combining the United Nations' estimate of the average weight of a cannabis plant (p. 39, UNODC, 2017) with the estimate of wholesale prices of cannabis herb in Spain for 2013, the confiscated plants are valued at approximately \$26 million. This compares to about \$312 million in confiscated cannabis coming from outside Spain in 2013.

[58]I draw on Alvarez et al. (2016), who assemble a dataset on cannabis plant confiscations based on 2013 press reports and public statements by the Spanish government. I do not have access to the microdata compiled by Alvarez et al. (2016), but instead use the approximate number of plants confiscated by province derived from their Figure 4. This leads to some measurement error. Moreover, I do not observe confiscations in the provinces of Ceuta or Mellila.

Figure C.6: Drug Confiscations and Local Immigrant Population



Notes: The figure plots the evolution over time of the value of confiscated illegal drug imports, separately for the 17 Spanish provinces with an immigrant population share in the bottom tercile as of 2000 (blue solid line) and the equal number of Spanish provinces with a top tercile immigrant population share in 2000 (red dash-dot line). Years are weighted by the inverse of the fraction of months with no reported confiscations.

C.8 Global Estimation

The relationship that I show for Spain may not be generalizable to the rest of the world. This may stem from Spain's unique history and institutions, for example its proximity to Morocco as a major trafficking source, Spanish-language affinity with many source countries, and Spain being the primary source of cannabis and cocaine for the rest of Europe.

To establish greater external validity in my results, I estimate an instrumented gravity regression similar to equation 1, but for the entire world, where each geographic unit o and d is a country. I do so using the UNODC Individual Drug Seizures data detailed in section 2.2, which are available for many countries beyond Spain. The drawback of these data is that, apart from Spain, reporting of drug confiscations is often irregular and incomplete (see Figure A.1). To some extent, the country fixed effects in equation 1 will absorb country-by-country variation in data reporting quality. Moreover, I focus on drug confiscations conducted in 2010 and later, a period during which reporting of drug confiscations had much improved relative to prior years. Nevertheless, measurement error may still affect estimates to the extent that countries disproportionately report confiscations for some origin or destination countries but not others.

To measure country-pair immigrant populations, I use the United Nations' database on migrant stocks by origin and destination for 2010. I instrument for the immigrant population using a leave-out push-pull instrument as in equation 2. To construct the instrument's components, cross-country immigrant flows, I calculate net changes in bilateral migrant stocks between each 5-year period that the UN database reports figures. I then have instruments from four 5-year periods: 1990 to 1995, 1995 to 2000, 2000 to 2005, and 2005 to 2010.

I report the results of estimating equation 1 worldwide in Table C.11. Consistent with my findings for Spain, I find that immigrants raise international imports in illegal drugs, both on the extensive margin (column 1) and the intensive margin (column 2). I similarly find immigrants raise global exports of illegal drugs (columns 3 and 4), though the effect of immigrants on the extensive margin of exports is imprecisely estimated. These results suggest that the relationship that I identify between immigrants and illegal trade goes beyond the Spanish context, and extends to many countries around the world.

Table C.11: Effect of Immigrants on Drug Trafficking Worldwide

	Imports		Exports	
	(1) Any seizure	(2) Value seized	(3) Any seizure	(4) Value seized
Log immigrants 2010	0.0332 (0.0156)	0.543 (0.326)	0.0346 (0.0386)	0.615 (0.284)
First-stage residuals		-0.000138 (0.352)		-0.0960 (0.276)
Observations	45365	10663	45365	11771
Log distance	Y	Y	Y	Y
Country-Year FE	Y	Y	Y	Y
Province-Year FE	Y	Y	Y	Y
1st-stage F-statistic	254.2	89.6	254.2	163.6
Estimation method	2SLS	PPML	2SLS	PPML

Notes: The table presents estimates of equation 1 at the country-pair level. I instrument for the immigrant population using predicted flows defined in equation 2. I show results for the extensive margin using 2SLS in columns 1 and 3, and for the intensive margin using PPML in columns 2 and 4. Country-pair immigrant stocks are measured using UN data. Standard errors are clustered by country of origin in parentheses.

C.9 Standard Errors

In my baseline specification, I cluster standard errors at the country level. To test whether my results are robust to alternative standard error clustering, including two-way clustering, I re-estimate my baseline specification using various clustering geographies. Table C.12 shows these estimates, which remain statistically significant across the different clustering geographies for imports and exports. Moreover, the clustering geography used in my baseline estimation, country-level, produces the largest standard errors.

Table C.12: Gravity Specification: Alternative Standard Errors

	(1) Imports (dummy)	(2) Exports (dummy)
Log immigrants 2011	0.172	0.0625
Cluster by country	(0.0461)	(0.0347)
Heteroskedasticity Robust	(0.0237)	(0.0277)
Cluster by province	(0.0219)	(0.0245)
2-way cluster by country & province	(0.0452)	(0.0321)

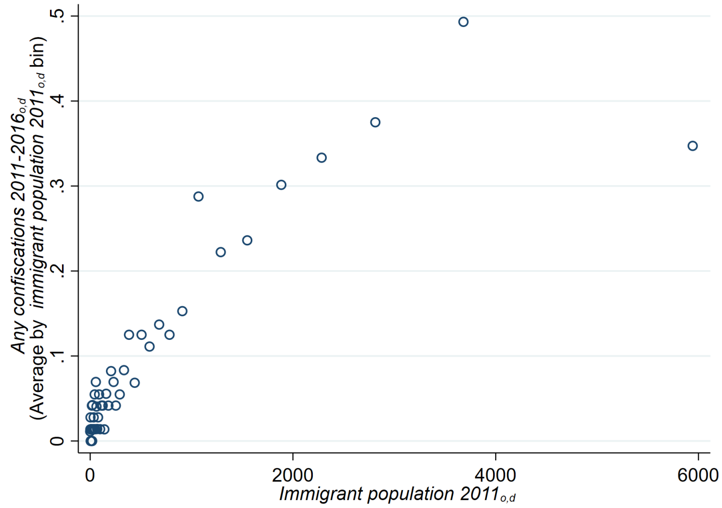
Notes: The table presents coefficient estimates and various standard errors from IV regressions of equation 1 at the country-province level. I control for nationality and province fixed effects as well as log distance. *Log immigrants 2011* is instrumented with the leave-out push-pull IV from equation 2. I cluster by country in my baseline specification.

C.10 Relaxing Functional Form Assumption

In my baseline gravity specification, equation 1, I measure the endogenous variable of interest as the log of one plus the number of immigrants measured in thousands: $\ln\left(1 + \frac{\#\, migrants_{o,d}^{2011}}{1000}\right)$. In this subsection, I first motivate my choice of functional form, and then show that my baseline results are insensitive to alternative functional form choices.

I motivate my choice of a log-functional form with the binscatter plot in Figure C.7 of the relationship between the immigrant population and a dummy variable for whether any import confiscation occurs at the country-province level.

Figure C.7: Relationship between Import Confiscation Dummy and Immigrant Population



Notes: The figure shows the binscatter plot between the immigrant population in 2011 and a dummy variable for whether any import confiscation occurred between 2011 and 2016 at the country-province-pair level. For visual clarity, I drop the highest quantile, which in any case does not change the figure's log curvature.

Next, I relax the functional form assumption of my baseline specification that $\pi = \frac{1}{1000}$ for the independent variable $\ln\left(1 + \pi \times \#\ migrants_{o,d}^{2011}\right)$. To do so, I estimate π in my baseline specification using nonlinear Generalized Method of Moments. Specifically, I simultaneously estimate the two baseline gravity equations for imports and exports,

$$\begin{aligned}\mathbf{1}\left[C_{o,d}^{2011\text{–}2016} > 0\right] &= \alpha_o^{Import} + \alpha_d^{Import} + \beta^{Import}\ln(1 + \pi \times \#\ migrants_{o,d}^{2011}) + \delta^{Import}\ln(Dist_{o,d}) + \epsilon_{o,d}^{Import}\\ \mathbf{1}\left[C_{d,o}^{2011\text{–}2016} > 0\right] &= \alpha_o^{Export} + \alpha_d^{Export} + \beta^{Export}\ln(1 + \pi \times \#\ migrants_{o,d}^{2011}) + \delta^{Export}\ln(Dist_{o,d}) + \epsilon_{o,d}^{Export}\end{aligned} \tag{C.11}$$

with moment conditions

$$E\left[\boldsymbol{Z_{o,d}} \times (Y_{o,d} - \alpha_o - \alpha_d - \beta\ln(\pi \times \#\ migrants_{o,d}^{2011} + 1) - \delta\ln(Dist_{o,d})\right] = \mathbf{0} \tag{C.12}$$

$$E\left[\begin{pmatrix}\boldsymbol{\alpha_o}\\ \boldsymbol{\alpha_d}\end{pmatrix} \times (Y_{o,d} - \alpha_o - \alpha_d - \beta\ln(\pi \times \#\ migrants_{o,d}^{2011} + 1) - \delta\ln(Dist_{o,d}))\right] = \mathbf{0} \tag{C.13}$$

for dependent variable $Y_{o,d} = \left(\mathbf{1}\left[C_{o,d}^{2011\text{–}2016} > 0\right], \mathbf{1}\left[C_{d,o}^{2011\text{–}2016} > 0\right]\right)'$, fixed effects $\alpha_i =$

$(\alpha_i^{Import}, \alpha_i^{Export})$, parameters $\beta = (\beta^{Import}, \beta^{Export})$ and $\delta = (\delta^{Import}, \delta^{Export})$, and excluded instrument set $\boldsymbol{Z_{o,d}}$ as in my baseline estimation (i.e., column 4 of Table D.2).

Table C.13 shows the results. My estimate of π includes within the 95% confidence interval my baseline functional form assumption of $\pi = \frac{1}{1000}$ and rejects the more conventional functional form choice $\pi = 1$. In addition, the estimates of (β_1, β_2) also are statistically indistinguishable from my baseline coefficient estimates.

Table C.13: Effect of Immigrants on Drug Confiscations (GMM)

	Drug Smuggling
β^{Import}	0.257
	(0.062)
π	0.038
	(0.030)
β^{Export}	0.074
	(0.061)
Observations	5564

Notes: The table presents coefficient estimates from nonlinear GMM estimation of moments described in equations C.12 and C.13. Standard errors are clustered at the country level.

Finally, I estimate my baseline specification across various alternative functional forms for the number of immigrants. I show the results in Table C.14. Across functional forms, more immigrants tend to lead to more drug confiscations as I find in my baseline estimates.

Table C.14: Robustness to Different Functional Forms

	Any confiscation (2011–2016)					
	(1)	(2)	(3)	(4)	(5)	(6)
	Import	Export	Import	Export	Import	Export
Log immigrant population (2001)	0.214	0.0975				
	(0.0634)	(0.0498)				
$\ln\left(\frac{M_{o,d}^{2011}}{1000}\right)$ (-1 for ∞)			0.125	0.0393		
			(0.0286)	(0.0241)		
$\left(M_{o,d}^{2011}\right)^{1/3}$					0.0239	0.0102
					(0.00792)	(0.00510)
Observations	5564	5564	5564	5564	5564	5564
Country FE	Y	Y	Y	Y	Y	Y
Province FE	Y	Y	Y	Y	Y	Y
Ln dist	Y	Y	Y	Y	Y	Y
1st-stage F-statistic	290.1	290.1	17.1	17.1	388.1	388.1

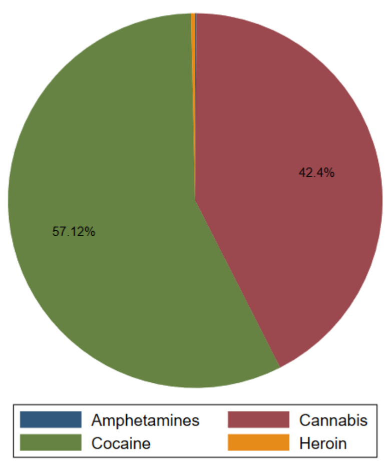
Notes: The table presents coefficient estimates from IV regressions at the country-province level using different functional forms to measure bilateral immigrant population. I instrument for the immigrant population measure with $\{IV_{o,d}^{D} = I_{o,-a(d)}^{D} \times I_{-c(o),d}^{D}/I_{-c(o)}^{D}\}_{1991-2001,2001-2011}$, their interaction across decades, and squared terms. The dependent variable is a dummy for whether any drugs trafficked between country o and province d were confiscated between 2011 and 2016 (separately for imports or exports). All regressions control for province and country fixed effects as well as log distance. Standard errors are clustered at the country level.

D Additional Tables and Figures

Table D.1: Summary Statistics for Gravity Variables

	(1)				
	mean	sd	p50	min	max
Distance (km)	5,073.52	3,442.12	3,818.07	123.62	18,437.94
Predicted immigration, 1991-2001	164.18	981.09	9.96	0.00	37,843.71
Predicted immigration, 2001-2011	559.58	2,840.74	39.77	0.00	100,500.05
$M_{o,d}^{2011}$	942.19	5,298.43	25.67	0.00	195,515.01
=1 if any import confiscation$_{o,d}$	0.08	0.27	0.00	0.00	1.00
=1 if any export confiscation$_{o,d}$	0.05	0.21	0.00	0.00	1.00
Value import confiscations$_{o,d}$	1,043,444.84	15,127,702.38	0.00	0.00	807,059,968.00
Value export confiscations$_{o,d}$	143,760.99	2,491,843.11	0.00	0.00	118,023,448.00
Observations	5564				

Figure D.1: Confiscations by Drug Type



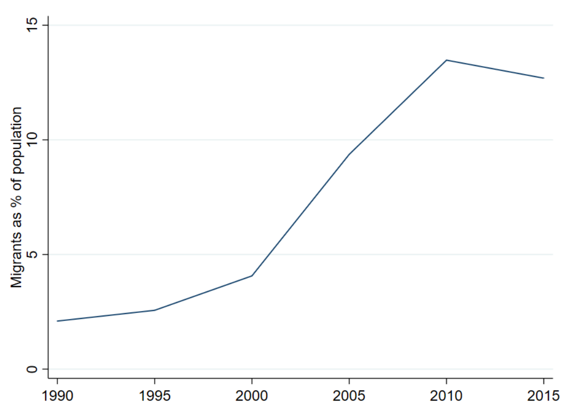
Notes: This figure shows the makeup of drug confiscations in Spain by drug type. Drug prices used are 2012 wholesale prices taken from a survey of Spanish drug prices reported to the United Nations Office of Drugs and Crime (UNODC).

Table D.2: First Stage Regressions

	Log immigrants 2011			
	(1)	(2)	(3)	(4)
Predicted immigration, 1991-2001	0.149		0.154	0.353
	(0.0325)		(0.0331)	(0.0392)
Predicted immigration, 2001-2011		0.0559	0.0370	0.151
		(0.0189)	(0.0203)	(0.0490)
(Predicted immigration, 1991-2001)^2				-0.00895
				(0.00142)
(Predicted immigration, 2001-2011)^2				0.00228
				(0.00194)
(IV 1991-2001)$\times$(IV 2001-2011)				-0.00348
				(0.00204)
Observations	5564	5564	5564	5564
R^2	0.687	0.693	0.698	0.740
Country FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y
Ln dist.	Y	Y	Y	Y
1st-stage F-statistic	21.1	8.8	11.5	152.4

Notes: The table presents coefficient estimates from first-stage regressions at the country-province level. All regressions control for province and country fixed effects as well as log distance. Standard errors are clustered at the country level. In order to interpret each first-stage coefficient as the marginal effect of predicted immigration inflows on the immigrant population, I residualize predicted immigration in 2001-2011 on predicted immigration for 1991-2001. This residualization has no effect on first-stage strength, only on individual coefficient magnitudes. For readability, I divide the instruments by 1,000. Column 4 corresponds to my baseline estimation set of instruments.

Figure D.2: Immigrant Population Share in Spain, 1990–2015



Notes: This figure shows the fraction of the Spanish population born in another country over time. The data are reported by the World Bank but originally come from the United Nations Population Division.

Table D.3: Effect of Immigrants on Drug Trafficking (Excluding confiscations aboard aircraft)

	Dummy for Any Drug Confiscations	
	Imports	Exports
Log immigrants 2011	0.188	0.0914
	(0.0429)	(0.0355)
Observations	5327	5327
First-stage F-stat.	208.8	208.8
Country FEs	Y	Y
Province FEs	Y	Y
Log distance	Y	Y
Sample	Distance in km > 1000	Distance in km > 1000

Notes: The table presents estimates of IV regressions at the country-province level, restricted to country-provinces pairs more than 1000km away from each other. The dependent variable is a dummy for whether any confiscation occurred, separately for imports (column 1) and exports (column 2). I exclude confiscations where the identified mode of transit was by air. I instrument for the immigrant population using $\{IV^D_{o,d} = I^D_{o,-a(d)} \times I^D_{-c(o),d}/I^D_{-c(o)}\}$ using the 2001 and 2011 censuses, their interactions across decades, and squared terms. Standard errors are clustered by country.

Table D.4: Effect of Immigrants on Cocaine Trafficking by Mode of Transit

	Imports		Exports	
	(1)	(2)	(3)	(4)
	Airport	by Sea	Airport	by Sea
Log immigrants 2011	0.194	0.109	0.0614	0.0160
	(0.0431)	(0.0395)	(0.0403)	(0.0134)
Observations	5564	5564	5564	5564
Country FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y
Ln dist.	Y	Y	Y	Y
1st-stage F-statistic	152.4	152.4	152.4	152.4

Notes: The table presents coefficient estimates from regressions at the country-province level. I instrument for *Log immigrants 2011* with $\{IV^D_{o,d} = I^D_{o,-a(d)} \times I^D_{-c(o),d}/I^D_{-c(o)}\}_{1991-2001,2001-2011}$, their interaction across decades, and squared terms. The dependent variable is a dummy for whether any drugs trafficked between country o and province d were confiscated between 2011 and 2016 (separately for imports in columns 1 and 2 and exports in columns 3 and 4). All regressions control for province and country fixed effects as well as log distance. Columns 1 and 3 refer to confiscations made in airports, while columns 2 and 4 refer to confiscations of drugs transported by sea. Standard errors are clustered at the country level.

Table D.5: Effect of Immigrants on Drug Trafficking including Meth and Heroin

	Dummy for any confiscation	
	(1)	(2)
	Imports	Imports
Log immigrants 2011	0.159	0.0579
	(0.0468)	(0.0348)
Observations	5564	5564
Country FE	Y	Y
Province FE	Y	Y
Ln dist.	Y	Y
1st-stage F-statistic	152.4	152.4

Notes: The table presents coefficient estimates from regressions of equation 1 at the country-province level. I instrument for *Log immigrants 2011* with predictions for two decades of immigrant inflows $\{IV^D_{o,d} = I^D_{o,-a(d)} \times I^D_{-c(o),d}/I^D_{-c(o)}\}_{1991-2001,2001-2011}$, interactions across decades, and squared terms. The dependent variable is a dummy for whether any drugs trafficked between country o and province d were confiscated between 2011 and 2016 (imports into Spain in column 1 and intended exports out of Spain in column 2). All regressions control for province and country fixed effects as well as log distance. Standard errors are clustered at the country level.

Table D.6: Effect of Immigrants on Cocaine Trafficking by Mode of Transit

	Imports		Exports	
	(1)	(2)	(3)	(4)
	Airport	by Sea	Airport	by Sea
Log immigrants 2011	0.194	0.109	0.0614	0.0160
	(0.0431)	(0.0395)	(0.0403)	(0.0134)
Observations	5564	5564	5564	5564
Country FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y
Ln dist.	Y	Y	Y	Y
1st-stage F-statistic	152.4	152.4	152.4	152.4

Notes: The table presents coefficient estimates from regressions at the country-province level. I instrument for *Log immigrants 2011* with $\{IV^D_{o,d} = I^D_{o,-a(d)} \times I^D_{-c(o),d}/I^D_{-c(o)}\}_{1991-2001,2001-2011}$, their interaction across decades, and squared terms. The dependent variable is a dummy for whether any drugs trafficked between country o and province d were confiscated between 2011 and 2016 (separately for imports in columns 1 and 2 and exports in columns 3 and 4). All regressions control for province and country fixed effects as well as log distance. Columns 1 and 3 refer to confiscations made in airports, while columns 2 and 4 refer to confiscations of drugs transported by sea. Standard errors are clustered at the country level.

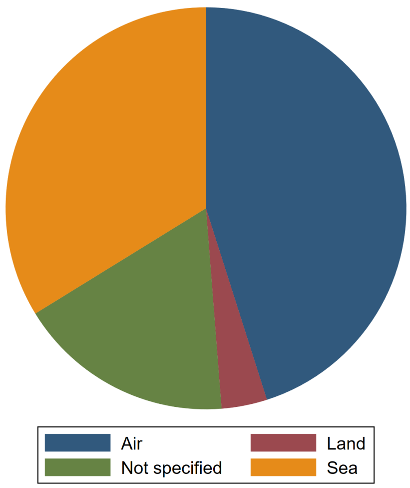
Table D.7: Effect of Immigrants on Export Drug Confiscations: Heterogeneity by Destination EU/Schengen Status

	Either EU or Schengen		Not EU/Schengen	
	(1)	(2)	(3)	(4)
PANEL A: Exports				
Log immigrants 2011	0.125		0.0144	
	(0.0501)		(0.0232)	
Log regular immigrants 2011		0.103		0.0607
		(0.0512)		(0.0449)
Log irregular immigrants 2011		0		-0.0404
		(.)		(0.0611)
Observations	1404	1248	4160	4004
Country FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y
Ln dist.	Y	Y	Y	Y
1st-stage F-statistic	291.8	74.3	165.8	19.0
Dep. var. mean	0.131	0.131	0.017	0.017
	Either EU or Schengen		Not EU/Schengen	
PANEL B: Imports				
Log immigrants 2011	0.203		0.211	
	(0.0585)		(0.0458)	
Log regular immigrants 2011		0.191		-0.144
		(0.0815)		(0.0637)
Log irregular immigrants 2011		0		0.482
		(.)		(0.0828)
Observations	1404	1248	4160	4004
Country FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y
Ln dist.	Y	Y	Y	Y
1st-stage F-statistic	291.8	74.3	165.8	19.0
Dep. var. mean	0.040	0.040	0.090	0.090

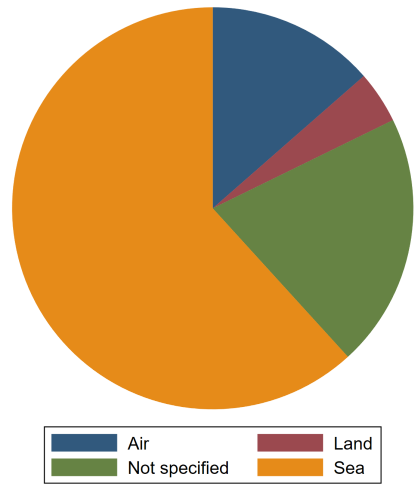
Notes: The table presents coefficient estimates from regressions at the country-province level. In columns 1 and 3, I instrument for *Log immigrants 2011* with $\{IV^D_{o,d} = I^D_{o,-a(d)} \times I^D_{-c(o),d}/I^D_{-c(o)}\}_{1991-2001,2001-2011}$, their interaction across decades, and squared terms. In columns 2 and 4 I instrument using the comparable variable defined in equation 6. The dependent variable is a dummy for whether any drugs trafficked between country o and province d were confiscated between 2011 and 2016 (exports into Spain in Panel A and imports out of Spain in Panel B). All regressions control for province and country fixed effects as well as log distance. Standard errors are clustered at the country level.

Figure D.3: Drug Confiscations by Mode of Transport

(a) By Number of Confiscations

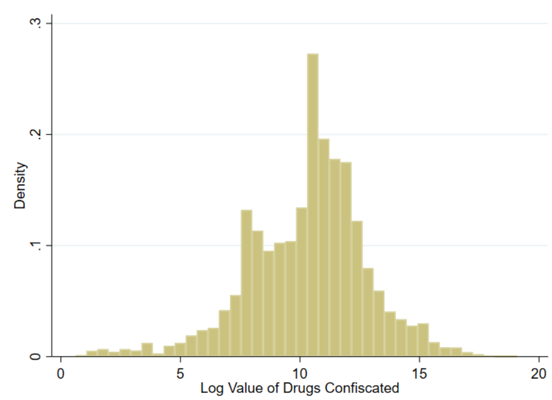


(b) By Value of Confiscations



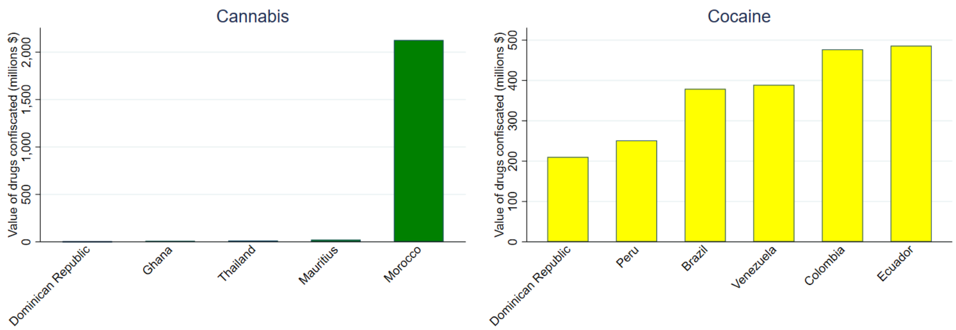
Notes: The figure shows the shares of mode of transportation of confiscated drugs. On the left I plot fraction of confiscation events, on the right, I plot the share of dollar values confiscated. Data come from the United Nations Office of Drugs and Crime.

Figure D.4: Distribution of Log Value of Confiscations



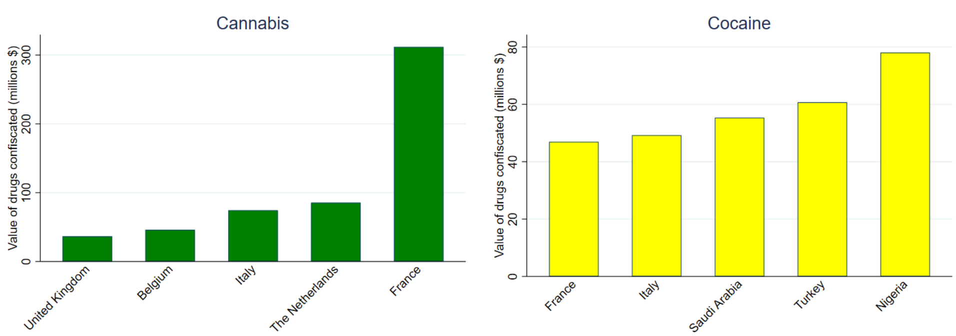
Notes: This figure shows the distribution of the log value of drug confiscations in Spain between 2011 and 2016 as reported to the United Nations Office of Drugs and Crime (UNODC). Drug prices used are 2012 wholesale prices taken from a survey of Spanish drug prices reported to the UNODC.

Figure D.5: Top Five Origins by Drug



Notes: This figure shows the top five countries of origin of illegal drugs confiscated in Spain between 2011 and 2016 by drug. Data come from the United Nations Office of Drugs and Crime.

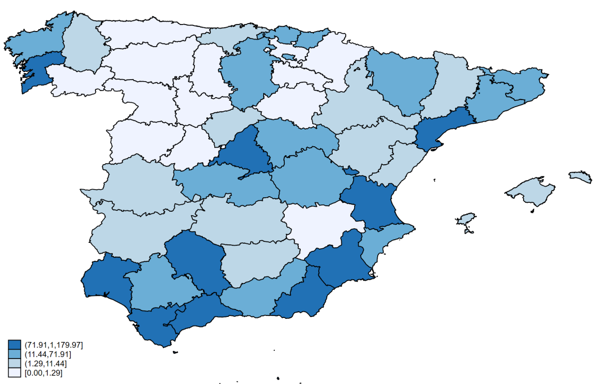
Figure D.6: Top 5 Intended Destinations by Drug



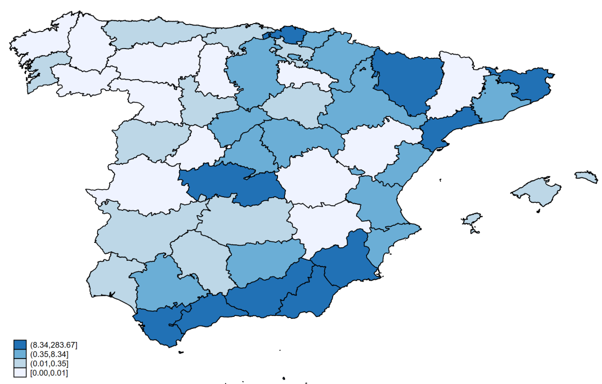
Notes: This figures shows the top five countries of intended destination of illegal drugs confiscated in Spain between 2011 and 2016. Data come from the United Nations Office of Drugs and Crime.

Figure D.7: Geography of Drug Import and Export Confiscations in Spain

(a) Confiscated value of imports per capita



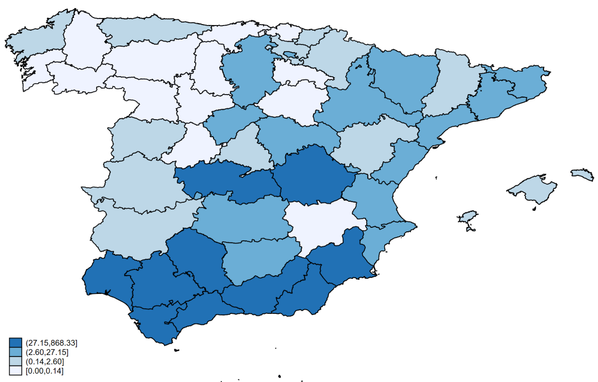
(b) Confiscated value of exports per capita



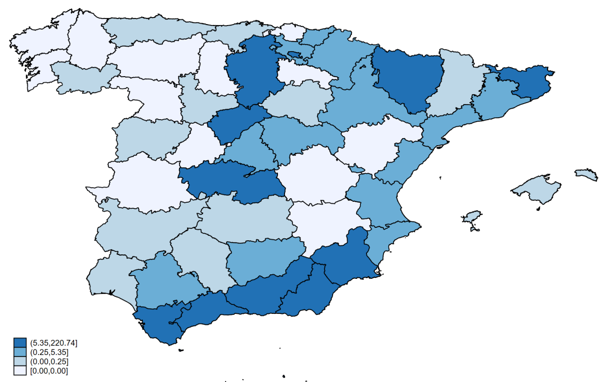
Notes: This figure shows quartiles for the distribution of drug confiscations of imports (measured in dollars by the estimated wholesale value of confiscated drugs) per capita on the left and exports per capita on the right, across Spanish provinces for confiscations occurring between 2011 and 2016 as reported by Spain to the United Nations Office of Drugs and Crime. Darker shades indicate higher quartiles.

Figure D.8: Geography of Drug Import and Export Confiscations in Spain by Drug

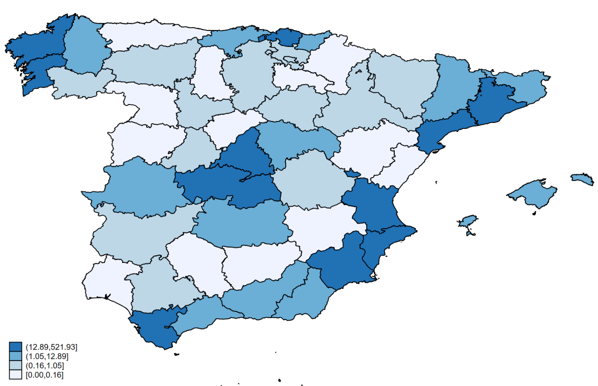
(a) Imports, Cannabis



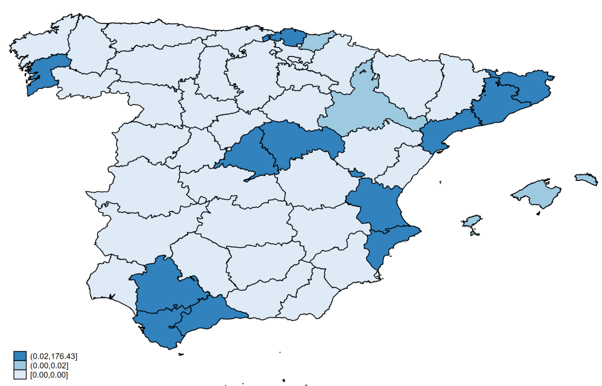
(b) Exports, Cannabis



(c) Imports, Cocaine

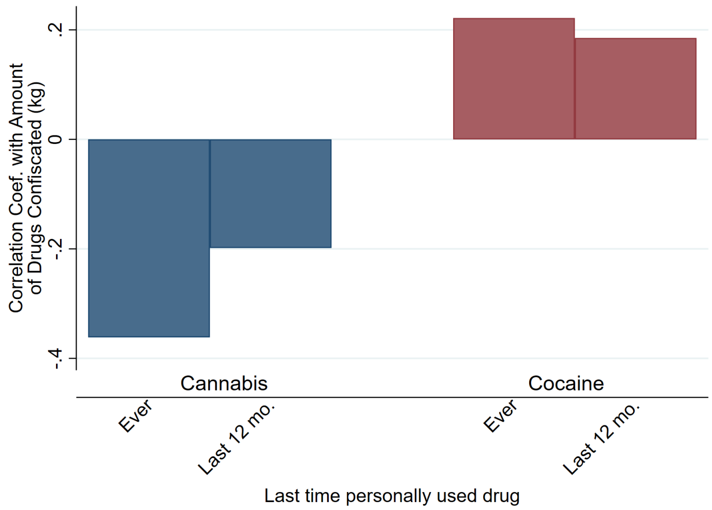


(d) Exports, Cocaine



Notes: This figure shows quartiles for the distribution of drug confiscations of imports (measured in dollars by the estimated wholesale value of confiscated drugs) per capita on the left and exports per capita on the right, across Spanish provinces for confiscations occurring between 2011 and 2016. Data are drawn from reports by Spain to the United Nations Office of Drugs and Crime Individual Drug Seizures database. Darker shades indicate higher quartiles. The bottom right map, confiscated cocaine exports per capita, lists only three quantiles because the first and second quartile of exports are both 0.

Figure D.9: Correlation of Drug Confiscations to Personal Use by Drug



Notes: This figure shows the correlation coefficient between the amount confiscated per capita for each drug with the fraction of respondents in a province who report having ever used the drug or having used the drug within the last 12 months. Correlations are estimated on a cross-section of 52 Spanish provinces, averaged across 2011 to 2016 for drug confiscations and across the 2011, 2013, and 2015 waves of the EDADES (Survey on Alcohol and Drugs in Spain).

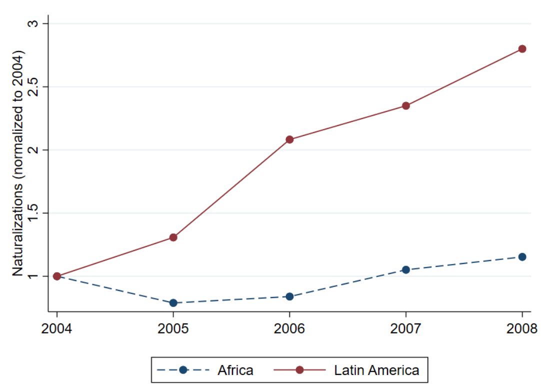
Figure D.10: Immigrant Work Permit Issuance

Notes: The figure shows the number of residency permits granted to immigrants over time in Spain. Data come from Spain's Ministerio de Empleo y Seguridad Social.

Figure D.11: Effect of 2005 Immigrant Legalization on Illegal Drug Exports

Notes: The figure shows event study plots of the effect of the 2005 immigrant regularization on export confiscations of cannabis (on the left) and cocaine (on the right). The dependent variable is whether any drugs were confiscated intended to go to the origin country in that quarter. Plot is estimated using equation 7. The dark grey area shows the 90% confidence interval while the light grey area shows the 95% confidence interval.

Figure D.12: Immigrant Citizenship Acquisition by Continent



Notes: The figure shows the number of immigrants obtaining citizenship, for both African immigrants (dashed blue line) and Latin American immigrants (solid red line). Data come from Spain's Ministerio de Empleo y Seguridad Social.

Appendix References

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