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Exporting Murder: US Deportations and the Spread of Violence

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Existing literature on cross-national variation in violence has paid little attention to the transnational transmission of crime. One such channel is the forced return of migrants with a penal record in their country of temporary residence. Responding to this research gap, we study the effect of US deportations of convicts on levels of violent crime in deportees' country of origin for a cross-country panel of up to 123 countries covering the years 2003 to 2014. We find a strong and robust effect of the deportation of convicts on homicide rates in countries of origin, which is to a large degree driven by deportations to Latin America and the Caribbean. An additional inflow of ten deportees with a prior criminal history per 100,000 increases expected homicide rates by roughly two. In addition to controlling for country-specific fixed effects, we provide evidence on a causal effect using an instrumental variable approach, which exploits spatial and time variation in migrant populations' exposure to state-level immigration policies in the United States.

Introduction

Worldwide, deaths from homicides outstrip deaths resulting from war by a large margin.[1] In 2015, the world saw five homicides per 100,000 people, the continent of Africa experienced thirteen homicides per 100,000 people, while Latin America had by far the largest number of homicides, with twenty-three homicides per 100,000 people. This surge in violence not only continues to destabilize regional governments but also contributes to the desire of individuals to emigrate from their homeland. Members of Central American migrant caravans that crossed Mexico on their way to the United States in 2018, for example, cited threats of violence as the main reason for leaving home (Médecins Sans Frontières (MSF) 2018); and epidemic gang-related violence in Honduras, Guatemala, and El Salvador is widely understood to be a leading cause of the mass increase in the migration of unaccompanied minors to the southern border of the United States during the summer of 2014 (Clemens 2017; Renwick and Labrador 2018).

Our research focuses on deportations as one of the determinants of violence. This focus on deported migrants helps extend and bridge two literatures: an extant literature on the social, economic, and political determinants of crime and interpersonal violence;[2] and a separate literature that explains the contagion of violence through an emphasis on social networks (e.g., Buhaug and Gleditsch 2008; Papachristos 2009; Moro and Sberna 2018). These literatures, however, miss an important transnational element of violence transmission: the role of convicts deported back home to their country of origin. We argue that the forced return of migrants with a prior penal record will, all else equal, increase violence in the deportee's homeland. Migrants who have been convicted of a crime prior to deportation may have a higher propensity to recur to criminal behavior upon returning home; and some could have developed connections with transnational organized crime in their host country. In their country of origin, deportees often face an environment where economic, political, and social opportunities are likely to be limited. Paired with social exclusion and stigma, these dire circumstances for deportees are a driving force for crime and violence, in particular when deportees are being returned to a country with weak law enforcement institutions.[3]

Whereas existing qualitative studies and journalistic investigations trace Central American gangs to their roots in the US metropolis (e.g., Dunn 2007; Howell and Moore 2010; Martínez and Sanz 2012a, 2012b; Howell 2015), our

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[1] Authors' calculation using data from the World Bank's World Development Indicators. In 2015, worldwide, there were 670,285 battle deaths compared with 4,245,231 homicides. These are huge increases compared with 2010, but the magnitude of difference remains: in 2010, there were 48,952 battle deaths compared with 373,038 homicides.

[2] See Koeppel, Rhineberger-Dunn, and Mack (2015) for a recent and comprehensive review of this literature.

[3] In addition to these direct effects, deportations may also increase levels of crime and violence through indirect mechanisms. For example, it is possible that deportations add to the fragmentation and multiplication of criminal groups, which can threaten the equilibria in criminal markets and lead to an escalation of violence. Moreover, forced return may also fuel violence and conflict in countries of origin if the return of migrants deprives families of a transnational pillar of economic support. By adding to the vulnerability and poverty of migrants and their families, deportations may therefore indirectly contribute to factors that have been associated with higher levels of interpersonal violence.

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contribution lies in illustrating a systematic pattern of violence contagion through deportations beyond isolated cases. This way, our research also adds to a growing literature on the multiple effects of return migration on countries of origin (Wahba 2014). While some of this literature raises positive expectations with respect to the role of voluntary return for the social and economic development of the countries of origin (e.g., Batista and Vicente 2011; Chauvet and Mercier 2014), the forced return of migrants may have negative external effects as we demonstrate for the case of returning convicts from the United States. Like other migrants who return home voluntarily, deportees bring home with them new ideas, norms, behaviors, and connections. Some of these connections may be in the form of transnational criminal networks that foster the movement of weapons and drugs, and the trafficking of human beings (Dudley 2012; Farah and Phillips Lum 2013).

Our research focuses on the effect of deportations from the United States for the following reasons. First, the United States is the largest destination for migrants in the world, with a population of undocumented migrants estimated at approximately 11.2 million (Passel and Cohn 2016) and an accumulated number of deportees of almost 5.4 million between 1997 and 2015. Of these, roughly 2.2 million had a criminal record[4] (US Department of Homeland Security, various years). Second, the United States is also the country with the highest per capita incarceration rates in the world (Walmsley 2013). Combining this with the fact that the United States has a relatively high homicide rate allows us to explore the mechanism linking exposure to violence in a host country to violence in the homeland.

Estimating the effect of deportations on homicide rates is prone to endogeneity: on the one hand, we may observe reverse causality between homicide rates and the inflow of deportees. For instance, an increase in violence in countries of origin also triggers higher rates of emigration, which might in turn lead to higher deportation rates. Or both deportations and origin countries' rates of violence might respond to a third omitted variable we are not able to observe. For example, networks of transnational crime—which are directly unobservable from a quantitative perspective—may affect levels of violence in both the countries of origin and host countries. In order to identify a causal effect, we deploy a two-step instrumental variables strategy. In our baseline empirical model, we estimate the effect of annual per capita deportations from the United States on per capita homicide rates in country i for a panel consisting of up to 123 countries covering the years 2003 to 2014. Our instrumental variable strategy exploits both spatial and time variation in migrant populations' exposure to US immigration policies as those policies vary across US states. Migrants who happen to live in US states with more restrictive immigration policies, all else equal, face a higher probability of being deported. In a first step, we therefore estimate deportation rates for each country based on migrants' exposure to state-level immigration policies. In a second step, we use predicted values for deportation rates to estimate a causal effect of deportations on homicide rates. Importantly, differences in the implementation of immigration policies across different US states are plausibly exogenous—it is difficult to envision a situation where a US state alters its immigration policy with a view to changing the homicide rate in a foreign country.

The rest of this article is organized as follows. The next section develops our argument and situates it in the broader literature. We then present the baseline model and data in the following section, and in the subsequent section we explain the instrumental variable approach. The next two sections present results for the baseline model and results for the instrumented regression respectively. Both the baseline regression and instrumented regression results show a strong and robust effect of the deportation of convicts on homicide rates in countries of origin: the deportation of an additional ten individuals with a criminal record per 100,000 people in the homeland increases that country's expected homicide rate by almost two per 100,000 people. Notably, the inflow of deportees without a penal record does not increase violence. We show, as expected, that this effect is strongly driven by the subregion of Latin America and the Caribbean, recipients of roughly 90 percent of all deportees from the United States. The final section concludes by discussing the important policy implications of these findings.

The Contagion of Violence through Deportations: Theoretical Considerations and Contribution to the Literature

There is already a large literature on the determinants of interpersonal violence and crime (see Koeppel, Rhineberger-Dunn, and Mack 2015 for a comprehensive review of this literature). This literature finds its analytical roots in Gary Becker's decision-theoretic model of the economics of crime: criminal behavior results when the expected gains from a crime outweigh the expected costs associated with apprehension. Becker (1968) and Ehrlich (1973) argue that increasing penalties (more severe penalties) and a higher likelihood of apprehension should, all else equal, dissuade an individual from engaging in criminal enterprise. Becker argues that criminal behavior is a rational calculation: an individual commits a crime based on a comparison of the gains from crime.[5] Research on the potential gains from crime has focused on conditions and opportunities that facilitate criminal activity. Within the framework of the Becker-Ehrlich model, socially deprived groups obtain higher gains from criminal activities because the opportunity costs of committing crime are lower. As a result, higher poverty and inequality are associated with a higher occurrence of criminal activities (Ehrlich 1973; Stack 1984; Hagan and Peterson 1995; Fajnzylber, Lederman, and Loayza 2002) as are high levels of unemployment (see Pratt and Cullen 2005 for a meta-analysis of socioeconomic factors). Additionally, the literature has found that urban areas, as compared with rural areas, provide increased opportunities for criminal behavior due to a higher number of social interactions (Fajnzylber, Lederman, and Loayza 1998). Demographic research has found that young males (Neumayer 2003) and, more generally, areas with larger youth cohorts are associated with higher levels of crime (e.g., Nivette 2011; Rivera 2016). One possible reason is that opportunity costs of crime increase with age (Hirschi and Gottfredson 1995) and education (Lochner 2004). At the national level, a legacy of civil war has been associated

[4] It is important to note that entering the United States illegally or overstaying one's visa does not constitute a felony; it is a misdemeanor. A criminal deportation hearing is initiated for a migrant—whether they are in the United States legally or illegally—if they have committed a felony; not all migrant felons are subject to deportation hearings. The 1996 Illegal Immigration Reform and Immigration Responsibility Act expanded the list of "aggravated felonies" (Martinez and Slack 2013).

[5] See Ehrlich (1973) for an early empirical application; and Dills, Miron, and Summers (2010) for a review of the deterrence literature. Note that studies on the deterrence effect of law enforcement found a crime-reducing effect of larger police forces (Fajnzylber, Lederman, and Loayza 1998). Deterrence through higher incarceration rates, for instance, have not reduced crime, possibly due to offsetting effects such as the loss in human capital or criminal capital acquired in prisons (Dills, Miron, and Summers 2010, 277).

with higher levels of violence (e.g., Rivera 2016), since the availability of firearms, weak states, and the unemployment of armed forces offer both opportunities for violent crime and lower associated opportunity costs.

Within this contextual understanding, our contribution emphasizes the importance of transnational factors in generating an increase in criminal activity. The empirical literature on the contagion of crime has mostly focused on geographical continuity as a mechanism of contagion, using models of spatial analysis (e.g., Anselin et al. 2000; Baller et al. 2001; Osorio 2015). However, recent contributions have identified mechanisms of the contagion of violence at the national level through a focus on social networks. For instance, Buhaug and Gleditsch (2008) study how civil conflicts constitute a threat to other proximate states via ethnic ties to groups in a neighboring conflict, while Moro and Sberna (2018) investigate how violence was perpetrated when Mafia groups in Italy moved outside their usual territory of activity, and Papacristos (2009) studies how gang murders spread through an epidemic-like process of social contagion within networks of group conflict.

Based on our reading of the literature, the deportation of convicts as a channel of violence contagion has been largely neglected in the literature. This is surprising insofar as migration scholars have long emphasized how norms, beliefs, and behavioral patterns—so-called social remittances (Levitt 1998)—are transmitted along migration corridors and may affect social and political variables back home. For example, migrants have been found to transfer political norms back home (e.g., Spilimbergo 2009; Docquier et al. 2016; Barsbai et al. 2017), promote female political incorporation (Lodigiani and Salomone 2015), increase voter turnout (e.g., Pérez-Armendáriz and Crow 2010), and spread norms associated with family planning (e.g., Beine, Docquier, and Schiff 2013), among others. Migrants not only affect home countries across distance, they have also been found to diffuse social norms and behavior upon their return. For instance, in an African context, as well as Batista and Vicente (2011) find that migrants transmitted democratic norms and values after their return to Mali and Cape Verde, respectively. Likewise, return migrants from destinations in the Gulf countries with highly traditional gender roles diffused more conservative gender norms upon their return to Jordan (Tuccio and Wahba 2018) and diffused destination countries' fertility norms upon their return to Egypt (Bertoli and Marchetta 2015). However, the literature has so far ignored how forced return relates to the diffusion of crime and violence along migration corridors. This is where our contribution lies.

We postulate that the deportation of convicts leads to a transmission of violence to home countries through both direct and indirect mechanisms that are not mutually exclusive and may reinforce each other. With respect to direct mechanisms, deportees who have been convicted of a crime may have a higher propensity to recur to criminal behavior upon returning home. Our rationale is twofold. First, deported convicts are likely negatively selected from the migrant population. If incarcerated in the United States prior to deportation, the deportee may have been exposed to more intense criminal behavior, which may alter their future behavior.[6] Although many of the registered crimes refer to relatively minor offenses (Martinez and Slack 2013; Hines and Peri 2019) and even re-entry may be considered a felony (Martinez and Slack 2013, 537), at least a portion of deportees was incarcerated before being removed. Since data on deportees only provide an aggregate category of "convicts" that includes both minor and major offenses, we are not able to distinguish different groups within this category. Those incarcerated are also more likely to have developed connections with transnational organized crime (Farah and Phillips Lum 2013). Moreover, the literature on parole in the United States points to the existence of a "revolving door" whereby once released, criminal offenders who are incarcerated are more likely to commit crime once paroled than those who are sentenced to probation (e.g., Western 2006). Second, those who are deported often face difficult reintegration upon return. Returnees—especially if they left their homeland due to civil conflict or economic crisis—are returning to an environment where economic, political, and social opportunities are likely limited. The post-deportation literature has unambiguously identified the difficulty of reintegration after return (Brotherton and Barrios 2009; Schuster and Majidi 2013; Mojica Madrigal 2017; Silver 2018) that comes along with the social stigma of deportees who are often associated with criminal activities (Burt and Savignac 2016, 7). Both factors—limited economic opportunities plus social exclusion and stigma—have been considered to be driving forces for crime and violence (as discussed above; see Ehrlich 1973; Stack 1984; Hagan and Peterson 1995; Fajnzylber, Lederman, and Loayza 2002). In some cases, gang membership may then provide those who return with a sense of community and belonging; membership that may also be a bridge to engagement in illegal activities (Dudley 2012).

Deportations also have an indirect effect on violence. The disruption of equilibria in criminal markets may lead to an escalation of violence. Consider the case of government intervention into the Mexican drug war: this interruption generated a massive increase in gang violence across that country (Dell 2015; Trejo and Ley 2018). This increase in violence occurred because government enforcement efforts resulted in the fragmentation of criminal groups, which, in turn, resulted in an increase in gang warfare as different gangs fought to achieve supremacy in criminal markets (Phillips 2015). We argue that deportations may add to the fragmentation and multiplication of criminal groups, which can threaten the equilibria in criminal markets.

Finally, deportations may also fuel violence and conflict through indirect mechanisms that do not necessarily imply a criminal act on the part of returnees themselves. For example, the forced return of migrants may deprive families of a transnational pillar of economic support. Often deportees return with a considerable stock of outstanding debt used to finance emigration (Brotherton and Barrios 2009; Hernández-Carretero and Carling 2012; Schuster and Majidi 2013). Deportations may therefore also add to the economic vulnerability and poverty of migrants and their families. As noted above, we know that social deprivation can generate violence and may drive family members to join criminal gangs.

To date, evidence on the effect of convicted deportees on violence in their country of origin relies principally on qualitative investigations and journalistic accounts from Central America. Qualitative researchers as well as journalists have traced the origin of Salvadoran gangs back to their origins in the United States (for the creation of Salvadoran gangs in the US, see Dunn 2007; Howell and Moore 2010; Martínez and Sanz 2012a, 2012b; Howell 2015) and argued that mass deportations of migrants with criminal

[6] Carlos Ramírez Landaverde, then deputy director of the department of criminal investigation in El Salvador, stated already in the 1990s: "As the deportations [from the United States] have increased, so has crime, and the deportees are responsible for a disproportional amount of that… Many of the most violent offenses, like murder, kidnapping, and robbery, are committed by people who have been in the United States and are sent back here without any prior notice, with all the bad habits they developed there" (cited in Rohter 1997).

records in the United States have led to the propagation of gang violence first in El Salvador and, subsequently, in other Central American countries (see, e.g., Arana 2005; Rodgers, Muggah, and Stevenson 2009; Lineberger 2011; Cruz 2013; Zuñiga Nuñez 2016). In this situation, Cruz et al. (2017) describe how deported felons enjoy the social recognition they are denied in mainstream society among the marginalized young men who constitute the principal recruitment pool for gangs. Hagedorn (2008, 43) notes, "In Guatemala, Honduras, and El Salvador, gang members returning from Los Angeles possessed powerful cultural symbols that appealed to alienated youth." Based on their migration experiences and criminal experience, deported convicts were therefore able to occupy a position of high status among marginalized youth. Although returning migrants did not constitute the bulk of gang members, as leading figures within these groups, they had a strong influence on the shape and character of their gangs (Cruz 2013). Attracting new gang members from the streets and in prisons (Ranum 2006; Demoscopía 2007), MS-13 and M-18 gradually supplanted, marginalized, or absorbed older native gangs into the new dominant gang culture (Rodgers, Muggah, and Stevenson 2009; Cruz 2013, 19). Today, criminal gangs have spread across El Salvador and toward other Central American countries (see, e.g., Arana 2005; Rodgers, Muggah, and Stevenson 2009; Lineberger 2011; Cruz 2013; Zuñiga Nuñez 2016). Recently, qualitative research has been supported by quantitative tests using municipal-level variation from the case of El Salvador. Kalsi (2018) uses the inflow of deported convicts in El Salvador as an instrument to explain levels of violence in El Salvador. Ambrosius (2018) finds that migrants' exposure to crime in the US explains the contagion of violence along migration corridors in El Salvador.

We argue that the Salvadoran experience is not unique, as suggested by anecdotal assessments from other countries.[7] However, systematic evidence for a pattern of violence contagion through deportations beyond isolated cases is rare. To our knowledge, Blake (2014) offers the only existing quantitative cross-country study on the links between deportation and violence in countries of origin. Relying on a cross-country panel of thirty-four advanced and developing countries over the period 1970 to 2004, he attributes a fourth of the increase in homicide rates in developing countries in the 1980s and 1990s to the inflow of deported felons.

While a useful starting point, we extend Blake's (2014) work in a number of important ways. First, we examine a much larger set of countries (123 versus thirty-four) spanning the bulk of middle- and low-income countries through the year 2014. Extending until 2014 is critical as this more recent period has been characterized by heightened rates of deportation from the United States. Second, we take the issue of causality seriously and generate a plausibly exogenous instrument that exploits variation in migrants' exposure to immigration policies—at the level of US states—which helps us decrease the possibility that our estimates are biased as a result of simultaneity or reverse causality. Finally, we embed our argument in a more comprehensive theoretical understanding of the causes of homicides, which allows us to hold constant a broader set of variables that are related to cross-national patterns of violent crime. The next sections develop the empirical model and introduce the data used to test the hypothesis on violence contagion for a large panel of countries.

The Deportation of Convicts and Homicide Rates in Countries of Origin: Data and Baseline Model

This article's goal is to assess, empirically, whether the deportation of felons from the United States increases violence in those migrants' country of origin. Direct and indirect channels of violence contagion discussed above are not mutually exclusive. Rather, we expect these channels to re-enforce one another. Empirically, we observe their joint effect at the aggregate country level.

Cross-country comparisons of crime pose a challenge: different countries have different definitions for what constitutes a crime and/or for how these crimes are punished. Even if definitions are the same, there are likely (significant) differences in detection rates and reporting biases across countries. We anticipate that the rate of unreported or undetected crime will differ greatly between countries and possibly also over time, depending on differences in legal systems across countries and state capacities (D'Amico and Williamson 2015). This challenge informs our choice of the dependent variable: while our argument applies to violent crime in general, we focus on homicides because homicide rates are the best observable and most comparable indicator of violent crime across countries that suffers from the lowest bias in terms of reporting and definitions as compared with other indicators of criminal activity.

We utilize data from the United Nations Office on Drugs and Crime (UNODC) to measure homicide rates, defined as annual violent deaths per 100,000 people. This allows us to cover up to 123 countries for the years 2003 to 2014. As an alternative, we also provide results using homicide data from the World Health Organization (WHO) in the supplementary online appendix. Although WHO data allow the tracing of homicide rates back to 1980 for some countries, that dataset covers fewer cases. Since our main focus of analysis lies in the years after 2003, we prefer UNODC over WHO data.[8] However, we do utilize WHO data in analyses extending back to the 1990s in order to study longer-term dynamics for the Latin American subcontinent, which is the recipient of roughly 90 percent of all deportees from the United States.

Our explanatory variable of interest is annual deportations of convicts from the United States. Data on annual deportations by country of origin are available from The US Department of Homeland Security's (various years) *Yearbook of Immigration Statistics*, which details the number of deportations of individuals with and without criminal convictions. Figure 1 shows the total number of deportations from the United States since the late 1970s. Removals intensified with the passage of the Illegal Immigrant Reform and Immigrant Responsibility Act (IIRIRA) in 1996, which expanded the categories of undocumented immigrants subject to deportation and made it more difficult for them to get relief from removal. Following the IIRIRA in 1996, any "alien" who served a longer-than-a-year sentence became subject to

[7] For instance, Jamaica's Prime Minister P.J. Patterson claimed that "the thousands of criminal deportees, mainly from the United States have added fuel to unacceptable levels of crime: Many of these persons have lived in the United States for considerable periods, been part of criminal gangs and on their return to Jamaica, link up with counterpart criminal elements to continue their illegal activities from our shores." Similarly, Dr. Prem Misir, Pro-Chancellor of the University of Guyana, argues that "criminal deportees have been intensively socialized in the criminal fields in the U.S. These deportees are in full possession of their U.S. criminal tool kit" (cited in Headley 2015).

[8] We are also concerned about measurement errors in the WHO data, especially in earlier periods and for low-and middle-income countries. Since these are also the countries that received most deportees, this could produce biased coefficients.

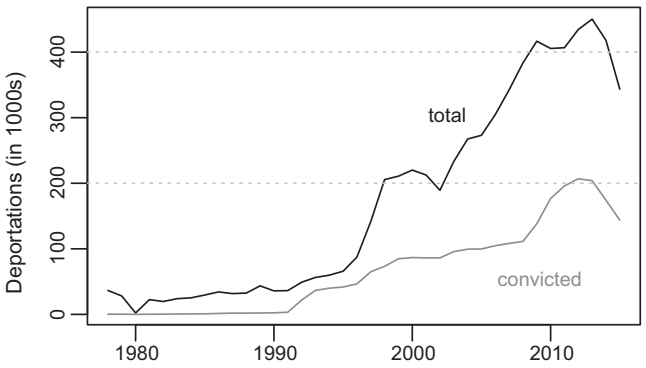

Figure 1. Annual deportations from the United States (total and deportees with penal record)
Note: Annual deportations based on data from the US Department of Homeland Statistics, *Yearbook of Immigration Statistics* (various years).

removal from the United States after completion of their prison term (Cruz 2013, 5).

After a peak of more than 400,000 annual deportations in 2013 (of these, more than 200,000 with a prior penal history), the total number of deportees declined, coinciding with a lower number of new arrivals and higher rates of voluntary returns to countries such as Mexico (Passel, Cohn, and Gonzalez-Barrera 2012). Over the period from 1978 to 2015, the total number of deported persons amounted to more than 6.2 million; of these, approximately 2.4 million had a penal record.[9] This compares to an estimated stock of undocumented migrants in the year 2014 of more than 11.2 million (Passel and Cohn 2016).

Figure 2 plots average levels of annual deportations of convicts per 100,000 people in origin countries against average annual homicide rates per 100,000 persons for the period 2000 to 2015, for 123 countries. This figure shows that the relative importance of forced returns from the United States varies greatly by geographic origin of migrants, as do homicide rates. Many of the most violent countries are situated in Central America and the Caribbean, led by Honduras (sixty-seven), El Salvador (sixty-one), and Jamaica (forty-nine). Peak years for homicide rates are even higher, reaching 108 in El Salvador and ninety-three in the gang-ridden countries El Salvador and Honduras.

Regarding annual deportations of convicts, the countries that received the largest number of convicted returnees relative to home country population are Mexico, the Northern Triangle of Central America (El Salvador, Honduras, Guatemala), as well as Belize and Jamaica. By 2015, the cumulated number of deported convicts in the most affected countries had reached 95,000 in El Salvador, 115,000 in Honduras, and more than 1.7 million in Mexico. The cumulative number of deportees with a penal record corresponds to between 1.3 percent and 1.5 percent of these countries' population stocks in 2015. It is clearly conceivable that such a sizable number of convicted deportees could have affected violence in countries of origin, suggested not only by the correlation in Figure 2 but also by anecdotal and journalistic evidence from Central America. In the following, we explore whether these two factors—deportations and homicides—are linked in a causal way.

[9] It is possible that one and the same person has been deported multiple times after re-entry in the United States. We are therefore not able to give a precise estimate as to how large the cumulative number of deportees (excluding double-counts) is.

As a first step, we estimate the following baseline model:

$$HomicideRate_{i,t} = \beta_1\, DeportationRate_{i,t} + \beta_2\, X_{i,t} + v_i, \quad (1)$$

where *HomicideRate* is the annual homicide rate for country i in year t and Deportation Rate is the annual convicted deportees per home country population received by country i. The time period t refers to two-year intervals. The main reason is that deportations may not necessarily have an immediate effect on rates of violence upon arrival. Instead, they may affect the outcome variable with a time lag. By using a panel of two-year intervals we allow for longer lags in the effect compared with annual data. Two-year intervals proved to be empirically strongest and allowed us to maintain a sufficient number of observations over time while at the same time providing some flexibility in the timing of the effect. Homicide rates and deportations are calculated over two-year means (i.e., as averages over the current and the preceding year). We therefore cover a maximum of six time periods between 2003 and 2014.

All regressions reported below include a set of country fixed effects to account for unmeasured, or unobservable, factors that may influence both deportations and homicide rates. A country's history of civil conflict, for example, may have left that country with a stock of arms and a legacy of violence that may pose more fertile grounds for a posterior increase of homicide rates. Country specific fixed effects may also capture slow-moving institutional and cultural factors in a country that are difficult to observe but may, nonetheless, influence rates of interpersonal violence.

In equation (1), X is a vector of time-varying control variables, which have been identified as potential predictors of cross-country variation in violence in the empirical literature. Following the literature on determinants of crime and violence, we include a number of control variables: we measure average socioeconomic status using both the log of per capita income and the country's aggregate level of Gross Domestic Product (GDP) growth. We control for age (the share of the population under the age of 14) and average education levels (years of schooling) as both of these variables affect opportunity costs of engaging in violent or criminal activities (Hirschi and Gottfredson 1995; Lochner 2004). We include the Gini index as a measure of income inequality as we expect countries with large inequalities to suffer from higher levels of social conflict and violence. A dummy variable measures whether countries were in a civil war during the period in question. We also include the log of population size and the share of the population living in urban areas. The existing literature suggests that democracies, all else equal, do a better job in resolving conflicts that may lead to criminal activity (e.g., Lin 2007); consequently, we include the polity measure of democracy, which ranges from most authoritarian (−10) to most democratic (10). An integrated measure of corruption captures the perception of government corruption in a country, taken from the International Country Risk Guide. This indicator may be interpreted as a measure of institutional quality that is correlated with law enforcement capacities. We also add remittances received relative to home country GDP as an indicator for a country's financial dependence on migrants, the rationale being that socioeconomic benefits of remittances may also affect levels of violence and crime. Finally, we include an indicator of the average exposure of migrants to crime in the United States. This variable controls for the potential that the diaspora is transmitting norms or practices regarding violence back to their homeland. To this end, we measure annual violent crimes per capita at the US state level and then

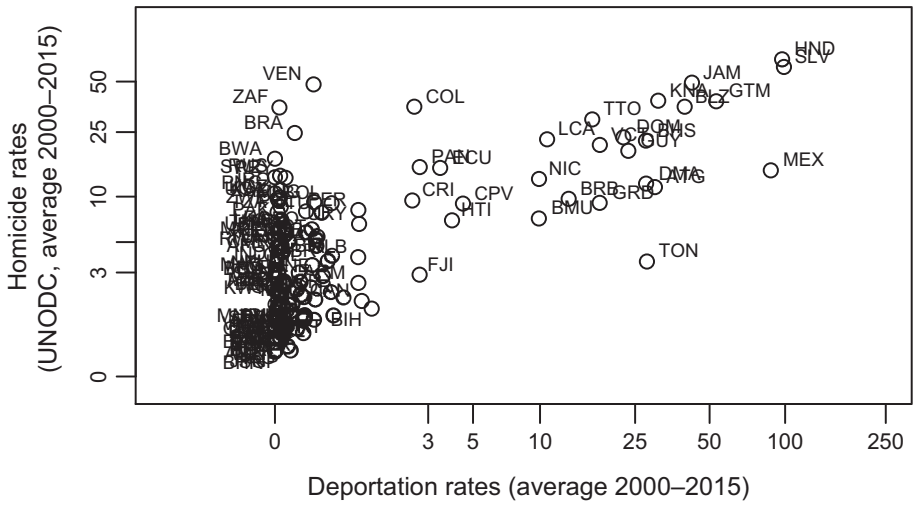

Figure 2. Homicide rates and deportation rates
Note: Homicide rates and deportation rates per 100,000 people, based on data from the United Nations Office on Drugs and Crime (UNODC) and the US Department of Homeland Security.

weigh state-level crime rates by the distribution of the foreign born population from each origin country i in each US state j. See Table 1 in the supplementary online appendix for descriptive statistics and sources of all variables.

Although deportations from the United States can be traced back to 1978, our empirical analysis focuses principally on the period 2003 to 2014, for several reasons. First, deportation rates from the United States actually became quantitatively important only in the mid-1990s, as illustrated in Figure 1. Second, our analysis is limited by the availability and quality of the dependent variable as well as of the control variables. In order to limit the chance that our inferences are influenced by poor measurement and noncomparable country samples, we focus on the most recent decades. Finally, the instrumental variable strategy developed below relies on information available for the period after 2000 only. We therefore focus our analysis on the period from 2003 to 2014 and extend back to the 1990s for the Latin American case, based on WHO data.

State-Level Variation in Immigration Policies as a Source of Exogenous Variation

Our focus is on the estimation of a causal effect of deportations from the United States on homicide rates in the deportee's country of origin. In spite of including country fixed effects in equation (1), a causal interpretation of the coefficient on deportation rates would be problematic under certain conditions. On the one hand, we may observe reverse causality, for example when an increase in violence in countries of origin also triggers higher rates of emigration, which might in turn may lead to higher deportation rates. Others might argue that a rise in violence at origin could be reflected in different migrant characteristics or different migrant behavior, which would make them more prone to engaging in criminal activities. In both cases, sceptics might claim that an increase in origin countries' rates of violence could translate into a higher number of deportations, with causality running from a growth in violence at origin to a growth in deportation rates. A second concern with equation (1) relates to the potential for omitted variable bias. Both deportations and origin countries' rates of violence might respond to a third omitted variable we are not able to observe. For example, networks of transnational crime—which are directly unobservable from a quantitative perspective—may affect levels of violence in both the countries of origin and the countries of residence. In this case, deportations and violence in the country of origin could move in parallel without being related in a causal way.

To address these concerns, we use an instrumental variables approach to estimate equation (1) via two-stage least squares. Our instrument exploits spatial variation in immigration policies across US states as a source of exogenous variation that explains variation in deportation rates by country of origin, but is not (directly) related to changes in violence in countries of origin. The rationale for the instrumental strategy is as follows. While the US Constitution charges the federal government with the creation of laws governing who can enter the United States and requirements for when foreigners can become citizens, individual states have the legal right to determine when and how noncitizens can participate in the labor market, when they can attend local schools, and the conditions under which they can obtain local services. We argue that when migrants—regardless of status—live in states that employ a tougher stance with respect to undocumented migrants, they are more likely to be deported upon the commission of a felony. The toughness of the state's stance regarding migrants should be correlated with the United State's aggregate deportation rate but, at the same time, should not directly influence the homicide rate in the migrant's country of origin other than through the deportation rate.

We measure the toughness of a state's immigration policy with two binary variables. First, we use an indicator for whether states had a policy to mandate that some or all employers use *E-Verify*, an electronic verification system that confirms the employment eligibility of workers and therefore excludes undocumented migrants from formal labor markets. A second indicator reports whether states provide prenatal care, regardless of a woman's migratory status, either through a state-funded program or through a

Table 1. Effect of US deportations of convicts on origin countries' homicide rates

	Homicide rates (UNODC)				
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Deportation rate (convicts)	0.15**	0.17***			
	[2.34]	[2.84]			
Change deportation rate (convicts)			0.15***	0.098***	0.23***
			[3.99]	[2.73]	[8.51]
Change deportation rate (non-convicts)				−0.063***	
				[−11.41]	
Change in deportation rate (convicts, leading values)					−0.011
					[−0.25]
Civil war		−1.5	0.15	0.27	0.35
		[−1.6]	[0.14]	[0.28]	[0.29]
Corruption		−0.049	−0.32	−0.22	−0.36
		[−0.11]	[−0.65]	[−0.46]	[−0.86]
Migrants' exposure to US crime		−0.059*	−0.066*	−0.047	−0.055
		[−1.93]	[−1.76]	[−1.4]	[−1.64]
GDP growth		−0.031	−0.011	0.017	−0.02
		[−0.66]	[−0.27]	[0.35]	[−0.46]
GDP per capita		0.45	−6.6**	−6.7**	−5.6
		[0.2]	[−2]	[−2.12]	[−1.59]
Gini (market)		0.24	0.021	−0.017	−0.03
		[1.39]	[0.13]	[−0.11]	[−0.19]
Polity		−0.033	−0.0035	−0.043	−0.006
		[−0.44]	[−0.05]	[−0.57]	[−0.09]
Population (log)		8.1	7.7	6.9	10
		[1.55]	[1.49]	[1.35]	[1.58]
Remittances		−0.11	−0.077	0.015	−0.11
		[−0.58]	[−0.48]	[0.13]	[−0.6]
Schooling		−0.45	−0.56	−0.83	−1
		[−1.09]	[−1.04]	[−1.47]	[−1.27]
Share population < 14		0.48	−0.79	−0.9	−0.78
		[1.02]	[−1.29]	[−1.6]	[−1.08]
Urban		0.21	0.24	0.34	0.33
		[1.02]	[1.01]	[1.44]	[1.33]
Sample	Full	Full	Full	Full	Full
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
R^2	0.16	0.32	0.18	0.31	0.24
F-stat	15.29	7.92	3.55	6.33	4.36
Number of observations	608	399	399	369	346
Periods (max)	6	6	6	6	5
Countries	123	81	81	79	81

Notes: Heteroscedasticity robust *t*-values in parenthesis. All results are based on ordinary least squares regressions with country and year fixed effects for two-year intervals. Stars denote significance at the 10 percent (*), 5 percent (**), and 1 percent (***) levels.

2002 Children's Health Insurance Program (CHIP) option. Both indicators are taken from Gelatt, Bernstein, and Koball (2018) and cover the years since 2000.[10]

Each of the instrumental variables varies by groups of immigrants i according to their exposure to immigration policies at the state level. We obtain a different indicator for each immigrant group i by weighting the state-level indicator on immigration policies with immigrant groups' share of migrants living in US state j. Note that all annual variation in the instrumental variable is attributed to period-to-period changes in immigration policies at the level of US states. The spatial distribution of migrants refers to the cross-sectional distribution of the foreign-born population extracted from the American Community Survey for the years 2005–2009 (US Census Bureau 2011). Due to the importance of networks to reduce the costs of migration, migration routes tend to develop along specific corridors that are very stable over time (McKenzie and Rapoport 2007). Variation in migration corridors and, as a result, different exposures to conditions in migrants' country of residence has frequently been used by migration researchers as a source of exogenous variation in instrumental strategies (e.g., Yang 2008; Adams and Cuecuecha 2010; Anzoategui, Demirgüç-Kunt, and Martínez Pería 2014; Ambrosius and Cuecuecha 2016).

[10] Among a larger set of policies targeted toward undocumented immigrants, we chose those policies that we consider to not violate exogeneity restriction and that proved to be the strong predictors for deportation rates. Other possible variables include *secure communities* (a federal data-sharing program through which fingerprints submitted by local law enforcement agencies to the FBI are shared with immigration enforcement agencies for checks against immigration databases), access to education for undocumented migrants, different health and social benefits, and different forms of cooperation in terms of immigration enforcement policies. See Gelatt, Bernstein, and Koball (2018) for details and an overview of other state-level immigration policies (URL: https://urbn.is/2vLfY5Z).

The first-stage instrumental regression can be formulated as follows:

$$DeportationRate_{i,t} = \beta_1 ImmigPolicy\_State_{i,\ t-1} + \beta_2\ X_{i,t} + v_i, \quad (2)$$

As in equation (1) above, *DeportationRate* is the number of deportations for each country i relative to the population size of the country of origin. *ImmigPolicy_State* refers to the two instrumental variables as described above. To ensure that the instruments precede deportations and are not themselves affected by deportation rates, we use the lagged values of the instrument in $t-1$. X refers to the same vector of control variables as explained above, and v is the set of country fixed effects. In the second-stage regression, we replace *DeportationRate* with the estimated values $\widehat{DeportationRate}$ as obtained from equation (2); this provides us with estimates of the country- and time-specific deportation rates, which are uncorrelated with homicide rates in the deportee's homeland. This enables us to obtain a causal estimate of the effect of deportation on homicide rates.

The validity of our instrument rests on two assumptions. First, the instrument has to be relevant, i.e., state-level variation in deportation policies has to be a sufficiently strong predictor for the deportation of convicts. We expect countries whose expatriates live in US states with more hostile immigration policies to suffer from much larger threats of deportation. This assumption will be tested formally below. Second, the instrument has to be excludable, i.e., state-level variation in deportation policies must be exogenous and not (directly) related to violence in countries of origin. Although this assumption cannot be formally tested, it should hold as long as state-level variation in deportation policies does not respond to migrants' behavior or changes in rates of violence in countries of origin. In the supplementary online appendix, we provide a detailed discussion of the determinants of state-level immigration policies. Both the available literature on the adoption of immigration policies as well as statistical analysis on the determinants of immigration policies among fifty US states validate the exogeneity of our instruments: it is primarily local economic conditions that explain changes in immigration policies rather than the size or characteristics of the migrant population hosted by US states.

In order to ensure that the instrumented effect is not driven by migrants' characteristics, regressions include controls for social, economic, and demographic characteristics of countries of origin. The vector of control variables in X also includes an indicator for the average exposure to state-level violent crime of the migrant population. This assures that results are not driven by higher levels of crime at migrants' place of residence. Moreover, we use lagged values of the instruments. Lagging instruments by one period ensures that homicide rates and subnational variation in deportation policies do not respond to unobserved simultaneous events in $t = 1$ or the possibility that deportation policies respond to current rates of violence in countries of origin. Together with country fixed effects, state-level variation in US immigration policies should provide a strong and exogenous instrument: there is no reason to believe that regional variation in immigration policies bears any direct effect on the origin countries' homicide rates in the following period, other than through the deportation of convicts.

Deportation of Convicts and Violence: Results from Panel Data Analysis

Before turning to the instrumental variables analysis, we present results from ordinary least squares (OLS) regressions for a cross-country panel in Table 1 as well as for the Latin American subsample—Latin America being the recipient of 90 percent of all deportees from the United States—in Table 2.

Table 1 for the full country sample provides five specifications, covering the years 2003 to 2014. All of the specifications use country as well as time fixed effects, hence controlling for time-invariant country characteristics and eventual time trends in the variables. As mentioned above, the panel uses two-year intervals for both homicide rates and deportation rates. Column 1 shows the effect of convicts' deportation rates on homicide rates without additional controls. Column 2 includes a set of time-varying social, demographic, and economic control variables. Column 3 uses change in convicts' deportation rates rather than their level, addressing any concern that levels of deportation rates and homicide rates could follow common trends. Column 4 includes the change in convicts' deportation rates together with the change in non-convicts' deportation rates. Column 5 includes leading values of changes in convicts' deportation rates.

Table 2 shows regression results for the subset of Latin American and Caribbean countries. Columns 1 and 2 follow specifications (2) and (3) from Table 1 for twenty-one countries from Latin America and the Caribbean. Column 1 uses levels in convicts' deportation rates, whereas column 2 uses the change in convicts' deportation rates as an explanatory variable. In columns 3 and 4, regressions are run using homicide rates from WHO data as a dependent variable instead of UNODC data for up to twenty-two countries. This permits us to cover a longer time period (1996 to 2014). We do not control for "migrants' exposure to US crime" in columns 3 and 4 since we have information on this variable only for the years from 2003 onwards.

Few of the control variables in Tables 1 and 2 show a strong and statistically significant pattern. In columns 3 and 4 of the full sample in Table 1, higher GDP shows a statistically significant association with lower homicide rates. It is noteworthy that migrants' exposure to crime at destination is, if anything, negatively related to homicide rates at origin. This supports our view that it is deportations rather than a shared trend in violence at places of residence and origin that explain our findings. For the subset of Latin American and Caribbean countries, higher inequality and a younger population are associated with higher homicide rates in columns 1 and 2 using UNODC data, confirming patterns found in previous literature (Rivera 2016). In line with expectations, countries with higher perceived corruption, countries with less democratic regimes, and countries with lower education levels tend to report higher homicide rates, although relationships are not robust across all specifications. When using WHO data for the longer period since 1996 (columns 3 and 4), a higher share of remittances relative to GDP is associated with more violence. This coefficient likely suffers from reverse causality as violence, as we argued above, may drive individuals to enter the United States either legally or illegally.

Our main interest lies in the estimated effect of deportation rates. In all specifications, the effect of convicts' deportation rates on homicide rates is positive, large, and statistically significant. In columns 1 and 2 of Table 1 (full sample), an inflow of ten additional deportees with a criminal record

Table 2. Effect of US deportations of convicts on origin countries' homicide rates (only Latin America and Caribbean countries)

	Homicide rates (UNODC)		*Homicide rates (WHO)*	
	(1)	*(2)*	*(3)*	*(4)*
Deportation rate (convicts)	0.28***		0.12***	
	[4.92]		[3.35]	
Change deportation rate (convicts)		0.13***		0.17***
		[3.52]		[4.15]
Corruption	−0.4	−0.8	−1.6	−2.1*
	[−0.22]	[−0.31]	[−1.49]	[−1.84]
Migrants' exposure to US crime	−0.15***	−0.14		
	[−2.75]	[−1.54]		
GDP growth	−0.3*	−0.2	0.14	0.21
	[−1.68]	[−1]	[1]	[1.61]
GDP per capita	11	−22	1.1	−12
	[0.9]	[−1.14]	[0.12]	[−1.08]
Gini (market)	0.8	−0.21	−0.15	−0.09
	[1.62]	[−0.31]	[−0.35]	[−0.18]
Polity	0.12	0.73	−0.65**	−0.36
	[0.41]	[1.06]	[−1.98]	[−1.09]
Population (log)	35	41	−15	−19
	[0.8]	[0.7]	[−0.84]	[−1.45]
Remittances	−0.88	−0.7	0.62***	0.87***
	[−0.9]	[−0.67]	[3.08]	[3.6]
Schooling	−1.1**	−1.1	−2.3*	−1.6
	[−2.31]	[−1.33]	[−1.66]	[−1.14]
Share population<14	5.4***	0.64	0.88	1.1
	[3.49]	[0.16]	[1.24]	[1.43]
Urban	0.34	0.98	0.61	0.87
	[0.81]	[0.97]	[0.95]	[1.23]
Sample	LAC	LAC	LAC	LAC
	2004–2015	2004–2015	1996–2015	1996–2015
Country fixed effects	Yes	Yes	Yes	yes
Year fixed effects	yes	Yes	Yes	yes
R^2	0.51	0.26	0.33	0.33
F-stat	3.72	1.24	2.89	2.83
Number of observations	99	99	161	159
Periods (max)	6	6	10	10
Countries	21	21	22	22

Notes: Heteroscedasticity robust t-values in parenthesis. All results are based on ordinary least squares regressions with country and year fixed effects for two-year intervals. Stars denote significance at the 10 percent (*), 5 percent (**), and 1 percent (***) levels. Latin America and Caribbean (LAC) = 000.

per 100,000 people increases expected homicide rates by 1.5 or 1.7 per 100,000 people. Put differently, a one standard deviation increase in the 2012 deportation rates (≈24) is associated with an increase in homicide rates of ≈ four per 100,000 people, based on the point estimate in column 2 (24 * .17 ≈ 4.1). For Honduras, the country with the highest deportation rate (≈162) as well as the highest homicide rates (≈ ninety-three per 100,000 people) in 2012, the point estimate assigns almost a third of its predicted homicides to forced deportations of convicts (162 * .17 ≈ 27.5, corresponding to ≈ 30 percent of its 2012 homicide rate).

Using changes in deportation rates rather than levels in columns 3 to 5 confirms the positive effect of convicts' deportations on homicides. Interestingly, a change in deportation rates of nonconvicts in column 4 does not have the same effect, and has a negative effect on homicides when included together with the change in deportation rates for convicts. Moreover, the leading values for changes in deportation rates do *not* increase homicides rates. This placebo regression increases our confidence that we are not measuring spurious correlation: changes in future homicide rates should not affect present homicide rates—and they do not—but changes in deportation rates of convicts over the same two-year intervals do affect homicide rates. Results are robust to the use of different indicators, samples, and intervals.[11]

For the Latin American subset (Table 2), coefficients for deportation rates are larger in size for levels and at a similar magnitude when measured in changes. The fact that coefficients are statistically strongly significant in spite of much fewer observations supports our expectation that the observed effect is to a large degree driven by experiences of the Latin American subcontinent, which provides the largest variation in terms of homicide rates as well as deportation rates. Coefficients for homicide rates are smaller in columns 3 and 4 when using WHO data instead of UNODC data for

[11] Table 2 in the supplementary online appendix repeats specifications from Table 1 for a subset of emerging markets and developing countries. Table 3 in the appendix repeats specifications from Table 1 but uses WHO data instead of UNODC data. Table 4 in the appendix repeats specifications from Table 1 for three-year intervals instead of two-year intervals. Whereas some of the coefficients lose statistical significance when using three-year intervals, the main message remains unaltered.

homicide rates and a longer time period, but of a similar magnitude of roughly 0.2.

Instrumental Variables Model Results

A causal interpretation of coefficients in Table 1 could be questioned on several grounds: violence may give rise to a surge in out-migration (e.g., Clemens 2017), which may, in turn, give rise to increased border security in the United States, a strategy which may include increased deportation rates. Or, it may be the case that both US immigration policy and violence in a country of origin may be responding to a third, unobserved factor such as the growth of transnational organized crime.

We confront these potential challenges to inference via an instrumental variables strategy using two-stage least squares. We argue that migrants' exposure to state-level variation in immigration policies provides a source of exogenous variation that is correlated with the deportation rates of convicts. As discussed above, we use two instrumental variables to capture the exposure of migrants to immigration policies along different dimensions: whether private or public employers had to use "E-verify" to check employment eligibility; and whether child and health care benefits extended to undocumented pregnant women. Each country's diaspora in the United States—measured in terms of the size of the foreign-born population from country i residing in the United States at time t—is weighted by its exposure to these state-level immigration policies.

Table 3 provides results from two-stage least squares. Columns 1 and 2 show results from the first-stage model for the full sample (column 1) and the subset of Latin American and Caribbean countries (column 2). Columns 3 and 4 show second-step results for both samples. The instrument of lagged immigration policies has strong joint effects on deportation rates of convicts. F-statistics (Chi square) for their joint significance are above 20 in both specifications (see test statistics in Table 3, columns 3 and 4), which is above the benchmark critical value for weak instruments (Stock and Yogo 2002). Exposure to a more restricted labor market has a negative effect on convicts' deportation rates, whereas more generous benefits for pregnancy and prenatal care are associated with higher rates of convicts' deportations.[12] One explanation for the latter result is that the enforcement of deportations, the exclusion from labor markets, and the extension of social benefits could pose trade-offs for policymakers: a more generous stance in labor markets and social policies could be justified by a strict application of "zero tolerance" deportation policies to those who have committed crimes.

As with the OLS results, all specifications include year and time fixed effects, hence all coefficients are estimated from within-country variation (i.e., changes in deportation rates over time). Control variables from the first-step regressions in columns 1 and 2 indicate that migrants from countries that are poorer, more unequal, and have a younger age structure experience more deportations. Latin Americans who are exposed to higher levels of violent crime at their place of residence also experience higher deportation rates. Overall, migrants from countries that are characterized by more precarious conditions are also deported at higher rates.

Results from the second-stage regression in Table 3 (columns 3 and 4) repeat regressions from the previous section but replace the variable $DeportationRate$ with predicted values $\widehat{DeportationRate}$ from first-step regressions in Table 2. The instrumental variables models provide consistent results: instrumented point estimates for the rate of deported convicts are 0.26 for the full sample and 0.36 for the Latin American subsample, slightly larger compared to the un-instrumented regressions results.[13] The signs of control variables are similar to those in Table 2, with only minor differences in terms of size and statistical significance. As in Table 2, results hold for the sample of just Latin American countries. In sum, the use of a lagged instrumental variable together with country fixed effects and a large number of control variables confirms the existence of a strong causal relationship between the deportation of convicts and homicide rates in countries that receive deportees. The Sargan overidentification test is not rejected, hence we find no sign that one of the instruments is endogenous.

Conclusions

This research examined the effect of deportations on violent crime in the returnee's home country. Focusing on the effect of convicts who have been deported from the United States, we observe a strong and robust effect of convicts' deportations on violence in migrants' country of origin, but not for the deportation of non-convicts. The causal effect is striking: for every ten deportees per 100,000 people, we observe an increase of homicide rates by roughly two. These results are to a large degree driven by Latin America and the Caribbean, the region with the largest variation and the largest levels of both deportation inflows and homicide rates. Using an instrumental variable approach where we use the exposure of migrant populations to variation in immigration policies as an exogenous source of variation backs the causal interpretation of our results.

We want to emphasize two things. First, the fact that deportations do not *decrease* crime in the United States (Hines and Peri 2019) supports our belief that the effect is not so due to a violent nature of deported convicts. Rather, violence is a result of social factors and conditions. What results in violence is the deportation of convicts to countries where they not only face weak institutions and limited economic opportunities but also suffer strong stigma and social exclusion (Brotherton and Barrios 2009; Schuster and Majidi 2013; Mojica Madrigal 2017; Silver 2018). Second, our study provides an aggregate estimate, whereas the causal mechanisms require additional research. Violence contagion may occur through both direct and indirect mechanisms. For instance, as discussed, the deportation of convicts may alter equilibria in criminal markets and incite violent reactions by other groups that compete in the same criminal market. Deportations may also fuel levels of violence and conflict if the forced return of migrants deprives families of a transnational pillar of economic support. The resulting economic vulnerability has been identified in the literature as a key predictor for violence. Finally, it is also possible that deportees who initially fled threats of violence in their home country become victims of violence themselves after being deported.

Our empirical findings have several important implications. First, our study emphasizes the transnational roots

[12] Strength in the first-step regression comes mainly from the variable on maternal benefits. We keep both instruments because the implementation of e-verify was individually significant in some of the first-step regressions we ran. Including either maternal benefits alone or in combination with other instruments related to state-level immigration policies did not qualitatively alter the results.

[13] A Wu-Hausman test on the coefficient for deportations rejected the null hypothesis of endogeneity at the 10 percent level, indicating that coefficients in the instrumented regressions are moderately downward biased.

Table 3. Two-stage least squares regression results (1st and 2nd step): effect of US deportations of convicts on origin countries' homicide rates

	First-stage deportation rate (convicts)		*Second-stage homicide rate*	
	(1)	*(2)*	*(3)*	*(4)*
E-verify (lagged)	−26	31		
	[−1.07]	[1.09]		
Prenatal care (lagged)	120***	266***		
	[2.66]	[6.43]		
Deportation rate (convicts)			0.26***	0.36***
			[3.25]	[7.04]
Civil war	9.3*		−2.3*	
	[1.86]		[−1.68]	
Corruption	−1.9	−0.32	0.05	−0.38
	[−1.06]	[−0.07]	[0.1]	[−0.21]
Migrants' exposure to US crime	0.03	0.23**	−0.051*	−0.15***
	[0.37]	[2.23]	[−1.82]	[−2.59]
GDP growth	0.14	0.61***	−0.043	−0.32*
	[0.97]	[2.8]	[−0.8]	[−1.8]
GDP per capita	−34***	−88***	3.8	20*
	[−2.83]	[−3.5]	[0.99]	[1.81]
Gini (market)	−1.4*	−5.5***	0.33	1.1*
	[−1.85]	[−3.2]	[1.49]	[1.79]
Polity	0.072	1.2	−0.056	−0.091
	[0.2]	[1.34]	[−0.58]	[−0.23]
Population (log)	−26	−110	8.3	31
	[−0.95]	[−1.18]	[1.2]	[0.63]
Remittances	0.024	1.5***	−0.12	−0.93
	[0.12]	[2.9]	[−0.61]	[−0.97]
Schooling	0.12	1.4	−0.39	−1.2**
	[0.14]	[0.89]	[−1.02]	[−2.25]
Share population<14	−5.5***	−13***	1	6.6***
	[−2.9]	[−3.08]	[1.51]	[3.6]
Urban	0.49	4.7***	0.18	0.19
	[0.71]	[2.72]	[0.82]	[0.34]
Sample	full	Lat. Am & Caribbean	full	Lat. Am & Caribbean
Country fixed effects	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes
Weak instrument F-test			29.22	33.68
Wu Hausman			0.04	0.07
Sargan			0.34	0.06
Number of observations	399	99	399	99
Periods (max)	6	6	6	6
Countries	81	21	81	21

Notes: Heteroscedasticity robust *t*-values in parenthesis. All results are based on two-stage least squares regressions with country and year fixed effects for two-year intervals. Stars denote significance at the 10 percent (*), 5 percent (**), and 1 percent (***) levels.

of violence; roots that have largely been neglected in the quantitative empirical literature on determinants of violent crime. Second, it highlights one channel through which emigration may produce negative long-term social outcomes at origin. Most importantly, the findings carry strong policy implications. The deportation of convicts bears huge follow-up costs for countries receiving deportees. At the same time, existing evidence points, against all rhetoric, to a null-effect of deportations on crime and violence in the United States (Hines and Peri 2019) as well as likely negative effects on GDP and employment (Edwards and Ortega 2016). Last but not least, deportation policies tend to provoke new emigration. Violence in Central America—that our empirical model at least partly attributed to deportations from the United States—have led to a new wave of immigration to the United States (Clemens 2017; Renwick and Labrador 2018).

One limitation of our study is that we are able to estimate effects of convict deportees from the United States only; data restrictions do not allow us to assess the effect of deportations along other migration corridors. Although findings presented here are driven by dynamics from the specific context of US deportations to Latin America—a region characterized by a long history of social conflict with one of the highest rates of violence in the world—our results should also be read as a warning against the possible negative consequences of forced returns in other contexts and situations.

Supplementary Information

Supplementary information is available at the *International Studies Quarterly* data archive.

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