Migration and the Demand for Transnational Justice[*]

Leslie Johns,[†] Máximo Langer, and[‡] Margaret Peters[§]

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Abstract

Domestic courts sometimes prosecute foreign nationals for severe crimes–such as crimes against humanity, genocide, torture, and war crimes–that are committed on foreign territory against foreign nationals. What factors explain such prosecutions? We argue that transnational justice is driven fundamentally by the political economy of migration. The movement of people across borders, as both economic migrants and refugees, exerts political pressure on local governments to conduct criminal investigations and prosecutions for crimes that occurred in other states, by and against foreign nationals. The legacy of colonialism can have an indirect effect by shaping the location of diaspora communities and domestic legal doctrines that are available to prosecutors. However, the underlying driver of transnational justice is migration.

1 Introduction

In 1998, an unprecedented event occurred in the UK House of Lords: legal experts deliberated whether to extradite General Augusto Pinochet from the UK to face criminal prosecution in Spain for torture that occurred in Chile. As the leader of Chile from 1973 to 1990, Pinochet oversaw widespread killings, torture, illegal detentions, and disappearances of his political opponents by the military and secret police. Political observers were sharply divided about Spain's request. Some observers viewed the Spanish prosecution as vindication for Pinochet's victims and a triumph of the international community (Roht-Arriaza, 2005). Other observers described the prosecution as "judicial tyranny" in which Spanish magistrates threatened the autonomy of Chilean political institutions (Kissinger, 2001, 86). Both sides recognized that Spain was administering justice in a way that Chile either could not or would not do. And both sides recognized that the Pinochet case reflected a broader trend: the spread of transnational justice.

The Pinochet prosecution is not an isolated example. Beginning in the mid-1990s, Spanish courts investigated serious international crimes committed in foreign states, including Argentina, Cuba, and Venezuela. These states all had high levels of political repression in prior decades. Yet Spain did not investigate leaders from other states with similar histories of repression, including Bolivia, Honduras and Nicaragua. How can we explain this variation?

Much of the debate over *Pinochet* and similar prosecutions has focused on the practical implications of transnational justice. However, a growing body of scholarship seeks to explain why cases like *Pinochet* occur. Two dominant narratives spring from this literature. The first narrative suggests that transnational justice is a form of neocolonialism, in which rich European states project their norms onto poor African states (Jalloh, 2010). The second narrative views transnational justice as a tool for activists and nongovernmental organizations (NGOs) to promote human rights by acting outside of the boundaries of the relevant state (Keck and Sikkink, 1998).

In contrast, we argue that migration—the movement of people across state borders—drives the demand for transnational justice. When individuals migrate from repressive or war-torn societies, they often have grievances that were not adequately remedied in the legal system of their home state. Our argument thus unifies and complements the two previous competing narratives. Because colonial relationships can affect migration flows—for example, by ensuring the existence of diaspora communities and shared language—there is often an observable relationship that resembles colonial ties. However, we argue that this apparent relationship is driven by the movement of people with political demands, rather than a straightforward projection of colonial aims. Similarly,

we know the NGOs play an important role in pressuring states to promote human rights. However, we argue that migrants are independent force in pressuring states to implement transnational justice. In the case of Spain, more migrants come from Argentina, Chile, Cuba, and Venezuela than from Bolivia, Honduras and Nicaragua, helping to explain the difference in Spanish prosecutions.

To specify our causal mechanism, we construct a formal model in which a group of migrants from a particular sending state (e.g. Chile) are living in a receiving state (e.g. Spain). We assume that the migrants benefit from criminal prosecutions in the receiving state of crimes that occurred in the sending state. Migrants have the opportunity to mobilize for transnational justice by, for example, reporting crimes to state authorities, providing evidence and witness testimony, lobbying governments to take action, and increasing general public awareness of prior atrocities. However, this mobilization will only be successful if enough migrants participate to persuade the receiving state to take action. Our key theoretical claim is that high levels of migration increase the likelihood of prosecutions by the receiving state of crimes that occurred in the sending state. We additionally argue that higher levels of atrocities in the sending state will lead to more mobilization, which increases the likelihood of prosecution. We show that governments that are more responsive to political action will be more likely to prosecute. And finally, governments that find it more costly to prosecute will be less likely to do so.

We test our theory using statistical analysis. We measure migration using the best available measure of migrant stocks from the sending state to the receiving state in a given year. Additionally, we use several measures of atrocities in the sending state, responsiveness in the receiving state, and the cost of prosecution. Overall, our statistical analysis provides strong support for our theoretical argument.

2 Background

We begin by providing a general overview of the intellectual justification for universal jurisdiction prosecutions. We then describe important empirical patterns in the data on these prosecutions. We finally provide an overview of various explanations that have been provided by previous scholars.

2.1 How Universal Jurisdiction Works

When states create and enforce domestic laws, they must establish their jurisdiction over the behavior that they seek to regulate. State usually assert such jurisdiction based on one or more of

the following principles:

- Territory: did an action (or its effects) occur on domestic territory?

- Personality: was the person who committed the action a national of the state?

- Passive personality: was the person harmed by the action a national of the state?

- Protective principle: did the action have systematic and important effects on the national interests of the state (such as national security threats)?

However, sometimes a state will assert *universal jurisdiction*, which means that it uses its domestic law and institutions to regulate behavior that occurs outside of its domestic territory, does not involve its nationals, and does not have systematic or important effects on its national interests. Universal jurisdiction differs from the traditional bases of jurisdiction because there is not a tangible link, or nexus, between the regulated behavior and the enforcing state at the time the alleged crime is committed.

Under international law, states may assert such universal jurisdiction to prosecute a set of core international crimes, which include crimes against humanity, genocide, torture, and war crimes (Langer, 2015*a*). Universal jurisdiction is included explicitly in some international treaties, like the UN Convention Against Torture (1984).[1] States asserting universal jurisdiction may also rely on customary international law, which is formed by the combination of state practice and acceptance of law (Langer, 2015*a*; Verdier and Voeten, 2014).

The intellectual justification for universal jurisdiction can be traced back to the historic practice of piracy, which had two key attributes. First, piracy usually involved murder, theft, and other acts that were clearly prohibited under all domestic legal systems. Cicero, the ancient Roman politician, described pirates as "a common foe" of the world (Neff, 2014, 469). Similarly, Alberico Gentili, a sixteenth-century Italian-British legal scholar, described pirates as the "common enemies" of mankind (Neff, 2014, 469). This common agreement amongst states that piracy was morally abhorrent gave states latitude to uphold the common prohibition of piracy.

Second, piracy involved acts that were committed on the high seas, and hence fell outside of the clear territorial jurisdiction of any specific domestic legal system.[2] If no state was willing to project its domestic jurisdiction to include the high seas, pirates could easily escape punishment.

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[1]See International Court of Justice, *Questions relating to the Obligation to Prosecute or Extradite (Belgium v. Senegal)*, Judgment of 20 July 2012.

[2]While legal claims to dominion over the high seas varied over time, states lacked the effective capacity to enforce these claims. See Grewe (2000).

The absence of authority over the high seas therefore supported the claim that everyone regulates piracy. This combination of attributes—violations of clear common values and lack of effective enforcement—led many international law experts, including Swiss writer Emer de Vattel, to argue that states could use their domestic courts to prosecute piracy, even when it occurred outside of a state's territory (Neff, 2014, 469).

As international law developed in the twentieth century, states increasingly returned to the idea of universal jurisdiction as one way to enforce international law, particularly human rights and armed conflict law. Namely, several states have argued that they could use their domestic courts to punish violations of clear common values (like the prohibition of crimes against humanity, genocide, torture, and war crimes), especially if no other state or international body was willing or able to enforce international law. For example, one of the earliest and most notorious examples of universal jurisdiction was Israel's 1961 prosecution of Adolph Eichmann, a prominent Nazi officially who help to orchestrate the Holocaust. Israel's claim to legal authority over Eichmann was partially based on the universal condemnation of his acts and the refusal by other states (including Germany and Argentina) to punish him. Universal jurisdiction is thus based on a claim with a fundamental tension—that anyone can prosecute serious international crimes precisely because no one else is willing or able to assert jurisdiction. In the case of crimes against humanity, genocide, torture, and war crimes, this tension is particularly stark because these crimes are often committed by state officials, and these crimes are heinous because, for instance, they dehumanize their victims (Langer, 2015a).

Universal jurisdiction is fundamentally a unilateral, domestic act (Langer, 2013). States that assert universal jurisdiction sometimes ask other states for assistance (in arresting defendants, collecting evidence, etc.), and may inquire on whether the territorial state has exercised jurisdiction over the case. But they do not ask for permission to prosecute. Some states justify universal jurisdiction based on treaty provisions in the Geneva Conventions (1949) and/or the UN Convention against Torture (1984). However, since multiple universal jurisdiction states have asserted that the rules in these treaties have become part of customary international law, they would apply to all states, regardless of whether a state is a treaty member (Johns, 2019b). Universal jurisdiction therefore differs from international prosecutions, which are based on explicit international cooperation from their inception and on an ongoing basis. For example, international tribunals for the former Yugoslavia and Rwanda were both created by the UN Security Council, while the International Criminal Court operates based on the Rome Statute, a treaty with broad state membership,

and they have all needed state cooperation to arrest defendants, gather evidence, and so on.

States vary dramatically in their domestic criminal laws (Langer, 2004). These differences make it extremely challenging to compare the prosecution of crimes across different states. For example, how exactly should a scholar define a "prosecution"? Should we only examine actual trials in which criminal charges are filed against a defendant, or should we also examine formal investigations launched by prosecutors and other law enforcement authorities? The definition of formal investigations varies across states. In common law jurisdictions, formal investigations usually start with an arrest, a grand jury indictment, or an information issued after a preliminary hearing. In contrast, in civil law jurisdictions, investigating judges or prosecutors usually begin formal investigations before anyone is arrested or indicted (Langer, 2004). For example, in the United States, the police may informally investigate a case for long periods of time provided that they do not arrest an individual or the person is not indicted. In other countries, like Argentina, such prolonged informal police investigations are not allowed.

We overcome this difficulty by using the data published in Langer (2011) and subsequently updated in Langer and Eason (2019). The universal jurisdiction data contains information on every known criminal complaint (or case considered by public authorities on their own motion) that:

- involved the alleged commission of one or more of the four core international crimes — crimes against humanity, genocide, torture and war crimes — by physical individuals;

- was filed or initiated between 1957 and 2017; and

- fully or partially relied on the principle of universal jurisdiction under which a state may have authority to prosecute, try and punish certain crimes even if the state in question did not have any territorial, nationality or national-interest link with the crime when the crime was committed.

The data thus does not include information on complaints filed under universal civil jurisdiction, one common tool for enforcing human rights violations (Johns, 2018). They also do not include criminal cases against corporations or other nonphysical legal entities, or cases that involve the alleged commission of other crimes subject to universal jurisdiction, such as piracy or slave trading.

To create this database, two research assistants independently examined multiple sources to find and code these cases. The sources included: published judicial decisions; LEXIS-NEXIS and Westlaw; specialized journals like the *Journal of International Criminal Justice* and the *Yearbook of International Humanitarian Law*; key books on universal jurisdiction and international criminal

law; the websites of the Center for Constitutional Rights, the Center for Justice and Accountability, the European Center for Constitutional and Human Rights, the Hague Justice Portal, Human Rights Watch, the International Center for Transitional Justice, the International Federation of Human Rights and TRIAL International; reports on universal jurisdiction and international criminal law cases by Amnesty International, Civitas Maxima, Human Rights Watch, and Redress; newspaper articles and other media documents; and the Google search engine. The data was then checked by Langer (2011) and later updated in a similar manner by Langer and Eason (2019).

These data include two measure of universal jurisdiction prosecutions from 1957 to 2017. First, the variable *Cases* is a broad measure of prosecutions that includes all allegations of the commission of crimes against humanity, genocide, torture and/or war crimes against an individual. These allegations were either presented to state authorities by private or public parties, or initiated by public officials. Such allegations would require the state in question to rely in whole or in part on universal jurisdiction to investigate and prosecute the case. The *Cases* measure likely underestimates the true number of investigations for two reasons. First, authorities and complainants may not publicly announce that such a complaint has been lodged. Second, any such public announcements may not have been reported by our sources. This challenge is endemic in cross-national measurement of domestic legal system outcomes.

Second, the variable *Trials* is a narrow measure of prosecutions that only includes cases in which the defendant has pled guilty or has been put on trial. We have more confidence in our measure of *Trials* because of the immense publicity surrounding such events. During the 2016–2018 updating of the database, researchers did not find any trial held before July of 2010 that was not reported in the original survey. Not surprisingly, the total number of *Cases* (1,962) is much higher than the number of *Trials* (61).

Neither of these measures perfectly captures our key theoretical interest: when states are more/less willing to assert universal jurisdiction over serious international crimes. On the one hand, the *Cases* measure is likely over-inclusive because it includes complaints to authorities, which can be ignored or overlooked. On the other hand, the *Trials* measure is likely under-inclusive because it only captures cases in which the defendant has pled guilty or has been put on trial. It does not include situations in which a state may genuinely want to assert universal jurisdiction but be unable to do so because of exogenous factors like limited forensic evidence, witness intimidation, difficulty in apprehending individuals accused of crimes, and limited state capacity to conduct high-profile crimes. Because of the difficulty in estimating statistical models with extremely rare events, we

focus our attention here on the *Cases* measure. In our robustness analysis, we include analyses that use the *Trials* measure. These results are available in the appendix.

2.2 Variation in Universal Jurisdiction Prosecutions

One empirical question is—are prosecutions like the Eichmann trial aberrations or common events? Figure 1 shows the number of universal jurisdiction cases plotted over time. This figure shows that universal jurisdiction prosecutions are rare — there are only 1,962 cases from 1957 to 2017. However, these prosecutions are not isolated or unique occurrences. They also vary tremendously over time. After the first case in 1957, there were no more cases until the early 1980s. These cases then occurred relatively infrequently until the early 1990s. Since then, there have been cases every year and cases were on average increasing until 2011 when they began to decline.

[Insert Figure 1 here.]

A second empirical question is–if any state can assert universal jurisdiction to prosecute serious international crimes, then which states actually do so? Table 1 lists all prosecuting countries by their case count. To help readers distinguish between states that were early users of universal jurisdiction and those that are relatively new to the practice, we also indicate the total number of cases before 2000, the total number after 2000, and the first year they took up a case. The top twenty countries are mostly advanced industrial states. However, middle income and even lower income states (like Argentina, Senegal, South Africa, and Turkey) have received universal jurisdiction complaints or have cases initiated by state officials. Among advanced economies, there are the Scandinavian states and Canada, which we often think of as strong human rights advocates, but also countries like the Australia, the UK, and the US, which have tended to oppose interventionist international law. The set of prosecuting states also includes a number of non-European states, including many states from Africa, Asia, and Latin America. While many states in the set are former colonial powers (such as France, Germany, and Spain), others are themselves former colonies (such as Argentina, Canada, and Senegal).

[Insert Table 1 here.]

A final empirical question is—is universal jurisdiction a legal practice driven exclusively by prosecutions of Nazis for the Holocaust, or is it a tool used more broadly to punish serious criminal acts? Universal jurisdiction cases have come from a variety of crimes. Many cases involve civil wars

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with large-scale human rights abuses, like the Syrian Civil War, the Rwandan genocide, and the conflicts in the Democratic Republic of Congo. Cases have also involved authoritarian repression. For example, Argentina has the fourth highest number of defendants stemming from its Dirty War, defendants from China been the subject of several complaints, and complaints have been lodged against defendants complicit in right-wing violence in El Salvador. Finally, some defendants are nationals of world powers that often engage in foreign military intervention, like France, the UK, and the US.

[Insert Table 2 here.]

2.3 Previous Explanations

An extensive literature discusses universal jurisdiction and other transnational prosecutions of human rights abuses. Much of this literature consists of legal analyses (Reydams, 2003; Ramdhass, 2018; Van Schaack and Perovic, 2013). Additionally, many scholars have examined the philosophical foundations of universal jurisdiction (Arendt, 1994; Luban, 2004; Chehtman, 2010; Hovell, 2018). Human rights activists, NGOs, and many scholars view universal jurisdiction as a crucial tool against impunity that can deter future international crimes and establish a minimum international rule of law to protect basic human rights (Macedo, 2004; Roth, 2001).

Understanding the causes of universal jurisdiction prosecutions is important because of the possible long-term effects of these prosecutions. Many scholars argue that universal jurisdiction prosecutions can have long-lasting effects on future human rights practices at the domestic level. For example, some scholars have argued that universal jurisdiction prosecutions have encouraged domestic prosecutions in the states where atrocities and other serious human rights abuses have taken place (Roht-Arriaza, 2005). Similarly, Sikkink and Walling (2007), Kim and Sikkink (2010), and Sikkink (2011) argue that Latin American prosecutions have positively affected human rights practices, democracy, and conflict.

However, a few scholars are more cautious about the possible effects of universal jurisdiction prosecution on domestic politics. For example, amnesty laws—which limit the prosecution of individuals for certain offenses—are often implemented as part of peace agreements following civil conflicts. Many experts believe that these amnesty laws help end conflicts by convincing combatants to demobilize and reintegrate into society (Dancy, 2018). These laws will have less impact if former combatants can face prosecution in other jurisdictions. Snyder and Vinjamuri (2004) argue that universal jurisdiction prosecutions thus risk causing more atrocities than they would prevent

because these prosecutions prevent both armed groups from using amnesties to resolve conflict and the transition from autocratic to democratic governance.

But what makes such prosecutions more or less likely? Some scholars emphasize international politics as an explanatory variable. Critics of universal jurisdiction often argue that universal jurisdiction is a tool for powerful states to project their authority over weaker states. These critics view the debate over how to end criminal impunity as a battle over hegemonic control of international law (O'Sullivan, 2017). For example, the African Union and several scholars argue that universal jurisdiction is biased against African leaders and officials, and often entails interference by powerful states over their former colonies (Geneuss, 2009; Jalloh, 2010; Mennecke, 2017).

Perhaps ironically, other critics argue that universal jurisdiction prosecutions do not pay enough attention to international power. Goldsmith and Krasner (2003) argue that universal jurisdiction prosecutions assume the utopian premise of a global consensus on when and how normative principles should be applied. They also argue that these prosecutions minimize considerations of power, neglect political prudence and consequentialist ethics, and slight the value of democratic accountability. Finally, Kontorovich (2008) maintains that universal jurisdiction is inefficient because while all nations may benefit from the waiver or trade of prosecutorial entitlements, any one nation—regardless of its power or importance in the international system—can prevent these benefits from being realized by choosing to prosecute.

A more moderate position is that international power politics can influence prosecutions without completely explaining all variation. For example, Langer (2011) argues universal jurisdiction trials are likely to concentrate on "low-cost defendants" whose prosecution does not impose high cost on the political branches of the prosecuting state. Such defendants may come from economic or politically weak states.

An alternative approach to explaining universal jurisdiction prosecutions emphasizes domestic politics. For example, Langer (2011) focuses on the balance of power among the branches of domestic governments. He argues that the political branches of government (i.e. the executive and the legislature of the prosecuting state) employ a cost-benefit analysis when deciding whether to prosecute. The more control that the executive branch has over the opening of formal proceedings and trials, Langer argues, the more that cost-benefit analysis affects prosecution decisions. This argument is supported by evidence that only low-cost defendants were brought to trial, and that legislatures are more likely to amend universal jurisdiction statutes when the costs of universal jurisdiction formal proceedings and trials outweigh their political benefits.

Of course, universal jurisdiction prosecutions should also be affected by the underlying preferences of a government, which are reflected in legal commitments. A large literature examines how factors like regime-type affect the willingness of states to sign, ratify, and comply with human rights and humanitarian law treaties (Hathaway, 2007; Simmons, 2009). These treaties define many of the international crimes—like torture and war crimes—that have been incorporated into domestic criminal laws (Berlin, 2020). Similarly, support for international criminal tribunals can indicate that a government supports the punishment of international crimes. For example, Langer (2015a, 223-228) briefly explores the pattern of universal jurisdiction state prosecutions vis-à-vis the pattern of prosecutions by the International Criminal Tribunal for the former Yugoslavia, the International Criminal Tribunal for Rwanda, and the International Criminal Court. Relatedly, Langer and Eason (2019) hypothesize that ICC implementing legislation can reduce the cost of universal jurisdiction trials, thus partially explaining the dramatic expansion in the number of universal jurisdiction prosecutions in recent decades. They also posited that factors like the creation of specialized investigative units for international crimes, institutional learning, and technological changes that facilitate the gathering of evidence all influence universal jurisdiction cases.

A final approach for explaining universal jurisdiction prosecutions emphasizes transnational politics. Most of this literature focuses on nongovernmental organizations (NGOs) as transnational actors. This literature argues that NGOs pressure states to prosecute serious international crimes, thereby leading to the internalization of human rights norms among state officials and a lessening of repression (Keck and Sikkink, 1998). For example, Sikkink (2011) studied the role of norm entrepreneurs in the launching and growth of domestic, foreign, and international prosecutions and the spreading of a global norm cascade against impunity.

Our analysis of the causes of universal jurisdiction prosecutions emphasizes both domestic and transnational factors. Namely, we argue that transnational migration—the movement of people across borders—creates demand for universal jurisdiction. When individuals arrive in a receiving state, they often have pre-existing grievances about acts that previously occurred in the sending state (which they have left). More migration indicates a higher availability of both defendants and victims (Langer, 2015a). It also indicates that more individuals are available to pressure the receiving state to take actions to prosecute international crimes. While large groups of actors are hindered by collective action problems, they are also helped by expanded opportunities for individual action and an increase in the collective benefit from prosecutions (Johns, 2019a). Overall, we show—both theoretically and empirically—that migration increases universal jurisdiction

prosecutions in receiving states.

3 Theory

Our theory is built on insights from the global games literature in economics (Carlsson and van Damme, 1993; Morris and Shin, 1998, 2003). This literature addresses equilibrium selection in games with large numbers of actors who must coordinate to achieve a common outcome. Here we focus on informally describing our assumptions, strategic behavior, and the impact of our explanatory variables on our outcome variable: whether a government invokes universal jurisdiction to prosecute an alleged international crime. Readers who are interested in seeing the full formal model can consult the Online Appendix [to be added].

3.1 Assumptions

Our theory focuses on the behavior of two types of actors: the government of the receiving state and a group of individual migrants from a sending state. These migrants, which have mass $M > 0$, know that international crimes have previously occurred in the sending state. We let parameter $a > 0$ represent the magnitude of these atrocities. Some of these migrants may be victims , while others may be witnesses. These migrants are contemplating whether to pressure the government of the receiving state to prosecute these atrocities.

We assume that the receiving state has private information about its own preferences about whether to prosecute. We refer to this private information as the government's ideology, θ. While real-world government preferences vary along many different dimensions, we use the term "ideology" to denote a government's views on human rights and internationalism. Namely, we assume that higher values of ideology indicate that the government is more willing to prosecute to universal jurisdiction cases. We assume that the strategic interactions begin when Nature chooses the government's ideology, which is uniformly distributed over the unit interval; i.e. $\theta \sim U[0, 1]$. Each individual migrant then observes a private signal about the likely value of the government's ideology; $x_i \sim_{iid} U[\theta - \epsilon, \theta + \epsilon]$ for small $\epsilon > 0$. These signals are relatively accurate, meaning that migrants have relatively accurate common information about politics within the receiving state. This assumption implies that migrants can know whether the government is left-leaning or right-leaning, whether it welcomes or opposes migration, its general attitude towards criminal justice, etc.[3] However, small differences among individual migrants (which can be caused by dif-

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[3]In this model, we assume that the mass of migrants is exogenous to information about political preferences of

ferent access to political information, variation in education and/or language skills, etc.) create individual-level variation in their assessments of the government's willingness to prosecute.

After each individual migrant observes her private signal, she decides whether or not to mobilize. These decisions are made simultaneously. Such mobilization can include many types of activities. For a crime victim, mobilization can include going to the police and filing a complaint about what happened to her in the sending state. For a crime witness, mobilization can include providing testimony or notifying the police if he sees the likely perpetrator of a crime in the receiving statement. Mobilization can also include individual and group political activism, like protesting or lobbying bureaucrats and politicians in the receiving state.

After migrants make these individual mobilization decisions, we assume that the receiving government observes the overall mass of migrants who have mobilized to demand justice, d where $0 \leq d \leq M$. The government then decides whether to prosecute an alleged crime using universal jurisdiction. Rather than considering the precise details of prosecutions, which vary dramatically across states, we conceptualize prosecutions as law enforcement and legal actions that are costly to the government but provide some remedial justice in expectation.

From the migrant's perspective, we assume that mobilization is always costly, where $t > 0$ represents the mobilization cost. We additionally assume that mobilization provides a benefit $v(a, \theta) > 0$ to those who have mobilized only if the government decides to prosecute. This benefit is increasing in the magnitude of atrocities in the sending state and in government ideology because we assume that prosecutions are more likely to be successful when they involve more severe atrocities in the sending state or receiving governments with higher values of ideology. We normalize a migrant's payoff from not mobilizing to be 0.

Note that this payoff structure means that universal jurisdiction cases are not a public good in our main analysis. We assume that the benefit from mobilization only accrues to those who actually mobilize. For example, a crime victim does not benefit if she refuses to report her crime to the police and seek justice on her own behalf. Alternatively, we could assume that universal jurisdiction cases are a public good, meaning that all migrants benefit when a universal jurisdiction prosecution occurs. This version of our model is discussed below in section 3.4.

From the government's perspective, we assume that prosecution has a fixed cost, $c > 0$. Variation in this cost could represent the legal capacity of the state and/or potential political costs imposed by the sending state if a prosecution occurs. We assume that the benefit of a

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the receiving state. We do not model the initial decision by migrants about where to go.

prosecution, $b(\theta, rd)$, varies in multiple parameters. We assume that the benefit of prosecution is increasing in the government's private ideology (θ) and the overall mass of migrants who mobilize to demand justice (d).[4] We additionally assume that a government's responsiveness to public pressure, which is represented by parameter $r > 0$, amplifies the impact of this migrant demand.[5] We conceive of responsiveness as state-level attributes that affect the ability of individuals to shape government policy, such as the receiving state's regime-type. The overall expected payoff to the government from a prosecution is therefore $b(\theta, rd) - c$. We normalize the government's payoff from not prosecuting to be 0.[6]

3.2 Strategic Behavior

We solve this game to find a weak perfect Bayesian equilibrium. This solution concept requires that player's beliefs are sequentially rational given their beliefs, and that players use Bayesian updating when possible.[7] In the Online Appendix, we show that we can describe the government's equilibrium behavior using a threshold value of how many migrants must mobilize for the government to prosecute. This government threshold is shown by the solid line in Figure 2(a). When the government has a very low ideology (θ is small), it receives little direct benefit from prosecution. Accordingly, it will only prosecute if many migrants mobilize. For extremely low ideology, the government will not prosecute even if all migrants mobilize. However, as the government's ideology increases, it receives more direct benefit from a prosecution and is therefore willing to prosecute for lower levels of migrant mobilization. For extremely high ideology, the government will prosecute even absent migrant mobilization. This would include situations in which the government actively seeks out crimes to investigate by examining asylum requests, building partnerships with non-governmental organizations, etc., rather than being pressured to act.

[Insert Figure 2 here.]

Recall that individual migrants observe signals about the government's ideology, rather than the true value of ideology. These signals are relatively accurate, but have tiny amounts of noise (or uncertainty). We can nonetheless calculate how many migrants in aggregate will mobilize in equilibrium for each possible true value of ideology. This measure of actual migrant behavior

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[4] The benefit function is therefore increasing in both of its arguments, $b_1 > 0$ and $b_2 > 0$.

[5] Many thanks to Terry Chapman for suggesting this operationalization of responsiveness.

[6] As detailed in the Online Appendix, we additionally assume that $b(0, rM) < c < b(1, 0)$ to ensure existence of our equilibrium.

[7] We do not need to make assumptions about off-the-equilibrium-path beliefs in our game.

is shown by the dashed line in Figure 2(a). When true government ideology is very low, migrants receive low signals, meaning that they believe that mobilization will be unsuccessful. For such low ideology values, migrants will not mobilize. However, as the true government ideology increases, migrants receive higher signals, meaning that they are more likely to believe that the government wants to prosecute international crimes. These beliefs in turn make migrants more likely to mobilize to receive the expected benefit of prosecution.

Our key outcome variable is whether a prosecution occurs. This outcome happens anytime the demand for a prosecution (actual migrant behavior) exceeds the willingness of the government to supply a prosecution (the government threshold). Note that in Figure 2(a), the actual migrant behavior and government threshold lines intersect at parameter θ^*. As shown by Figure 2(b), prosecutions occur whenever ideology is larger than θ^*. This is when the mass of migrant who actually mobilize exceeds the government's threshold. In contrast, the government does not prosecute if ideology is smaller than θ^*.

3.3 Impact of Explanatory Variables

How do changes in our explanatory variables affect our outcome variable, whether the government prosecutes? Answering this question entails analyzing the impact of our explanatory variables on parameter θ^* in Figure 2. Each individual parameter can have complex effects on both government and migrant behavior. We accordingly focus here on conveying the intuitions behind the causal effect of explanatory variables. Reader who wish to see precise details on these effects should consult the Online Appendix.

Our first explanatory variable is the mass of migrants. Recall that the mass of migrants does not directly affect the government's preferences. The government only cares about how many migrants actually mobilize. The mass of migrants therefore exerts its primary effect on actual migrant behavior (the dashed line in Figure 2). As the mass of migrants grows larger, each individual migrant will believe that more migrants are likely to mobilize. This makes the likelihood of prosecution more likely, which in turn makes each individual migrant more likely to mobilize herself, shifting parameter θ^* to the left. These reinforcing causal effects generate our first empirical hypothesis:

Hypothesis 1 (H1): A larger mass of migrants from the sending to the receiving state will increase the likelihood of a prosecution.

Our second explanatory variable is the magnitude of atrocities in the sending state. This variable does not directly affect the government's preferences. However, the magnitude of atrocities affects migrant utility from prosecutions. As the sending state commits more atrocities, each individual migrant receives a higher expected utility from mobilizing. She also believes that other migrants are more likely to mobilize, making her more likely to mobilize as well. The overall effect of more atrocities is therefore to move parameter θ^* to the left. These reinforcing causal effects generate our second empirical hypothesis:

Hypothesis 2 (H2): A higher magnitude of atrocities in the sending state will increase the likelihood of a prosecution.

Our third explanatory variable is the responsiveness of the government in the receiving state. Responsiveness does not directly affect individual migrant preferences. Migrants only care about whether a prosecution occurs. Government responsiveness therefore exerts its primary effect on the government threshold (the solid line in Figure 2). As government responsiveness increases, the impact of migrant demand on government utility is amplified, meaning that the government receives more benefit from a prosecution, and the government threshold decreases. This makes the likelihood of prosecution more likely, which in turn makes each individual migrant more likely to mobilize herself, shifting parameter θ^* to the left. These reinforcing causal effects generate our third empirical hypothesis:

Hypothesis 3 (H3): A higher level of responsiveness in the receiving state will increase the likelihood of a prosecution.

Our final explanatory variable is the cost of prosecution to the government in the receiving state. Because this cost only directly affects the government's utility function, the prosecution cost exerts its primary effect on the government threshold (the solid line in Figure 2). As the prosecution cost increases, the government receives less benefit from a prosecution, meaning that the government threshold increases. This makes the likelihood of prosecution less likely, which in turn makes each individual migrant less likely to mobilize herself, shifting parameter θ^* to the right. These reinforcing causal effects generate our fourth empirical hypothesis:

Hypothesis 4 (H4): A higher prosecution cost in the receiving state will reduce the likelihood of a prosecution.

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Each of these hypotheses is grounded in a causal theoretical framework. Yet each hypothesis is based on observable attributes and outcomes. We accordingly now proceed to test our theoretical argument using empirical measures of our theoretical concepts.

3.4 Robustness

Recall that in our model we assume that universal jurisdiction cases are a private good: a migrant only receives an expected benefit from a universal jurisdiction prosecution if she actually mobilizes herself.[8] Whether this assumption is reasonable will depend on the specific details of an example. Because successful prosecutions require compelling evidence and typically the physical presence of the defendant, many universal jurisdiction cases focus on relatively low-level individuals, like prison camp guards and local commanders. In such situations, migrants who refuse to mobilize are therefore unlikely to receive justice for their particular injuries, and the assumptions above are reasonable. However, some universal jurisdiction cases may more closely resemble a public good, especially cases that target high-level government officials with responsibility for a broad set of crimes, like Augusto Pinochet.

If we want to examine these prosecutions as public goods, we cannot use a global game approach, which uses a continuum of migrants. In such a model, no individual migrant would ever complain because his action would never be decisive in influencing the government's decision. If we instead construct a model with a large number of discrete migrants and prosecution as a public good, we encounter well-known issues of equilibrium selection. First, there always exists an equilibrium where no migrant ever mobilizes. In this equilibrium, Hypothesis 4 continues to hold, while the mass of migrants, atrocities, and responsiveness have no effect. Second, there always exists an equilibrium in which migrants solve the coordination problem and the minimum number of migrants mobilize to provide the public good when possible. In this equilibrium, Hypotheses 1, 3, and 4 continue to hold, while atrocities have no effect. Overall, this model extension supports our main theoretical claim: that migration matters.

Another alternative would be to assume that the cost of prosecution is itself a function of migration levels. Higher levels of migration should make it easier for prosecutors to collect evidence and interview witnesses, regardless of mobilization levels.[9] If we assume that the cost of prosecution is decreasing in migration, then our main theoretical claim becomes even stronger: more migration will increase the likelihood of a prosecution.

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[8] All of the model extensions that are discussed in this section are available upon request.

[9] Many thanks to Francesca Parente for suggesting this possibility.

4 Data and Methods

We now turn to our statistical tests of how migration, responsiveness, and costs affect the likelihood that a state will take up a case under universal jurisdiction. We use a directed dyad model to understand when a state takes up a case involving defendants from another country. First, we discuss the data we use to test these hypotheses and our estimation strategy. We then turn to the results of our empirical analysis and robustness checks.

4.1 Data

Outcome Measures

Our outcome variable is universal jurisdiction prosecutions. Our data for these prosecutions come from Langer (2011), which was updated in Langer and Eason (2019). These data include variables for both the receiving state (which pursued the prosecution) and the sending state (which is the home-state of the defendant). While we are able to identify the defendant's nationality for most cases (68 percent), some universal jurisdiction cases provide insufficient information to precisely identify the sending state. For example, many prosecutions for crimes under the Nazi regime involved defendants who came from states like Ukraine and Belarus, rather than Germany. Similarly, many prosecutions against ISIS members potentially include individuals who were foreign-born. Finally, for crimes during the dissolution of Yugoslavia, it is often difficult to identify the defendant's nationality. We deal with this challenge in two ways: (1) we assign these cases to the major perpetrator country, thus assigning Nazi cases to Germany, Yugoslav cases to Serbia, and ISIS cases to Syria; and (2) we drop all instances in which we do not know the subject nationality, which we label "no John Does."

Our main analysis uses the dyadic measure of cases, which measures how many universal jurisdiction complaints a receiving state considered against a defendant from the sending state in a given year. These complaints came from individual victims, NGOs, and prosecutors who opened investigations on their own authority. Cases only appear in the data for the year in which they are initiated. We use this variable in three different ways. First, the variable CASE COUNT measures how many universal jurisdiction complaints a receiving state considered against defendants from the sending state in a given year. Second, the variable ANY CASE measures whether the receiving state considered any complaint again defendants from the sending state in a given year. Finally, the variable FIRST CASE equals 1 for dyad-years in which the receiving state considered its first complaint against a defendant from any sending state, and 0 otherwise.

We are agnostic about which measure best captures the concept of transnational justice. In some regimes, like Argentina under military rule, repression was more decentralized and cases were filed again dozens of individuals. In such regimes, the variable CASE COUNT is probably most appropriate. In other regimes, like Pinochet's Chile, repression was more centralized under the command of one individual and cases were filed against fewer individuals. The measures ANY CASE or FIRST CASE are probably most appropriate for such regimes. Because no single measurement is ideal, we use all three measures and carefully consider the robustness of our empirical results.

Explanatory Variables

Migration

Our main explanatory variable is MIGRANT STOCKS, which is a dyadic measure of the total number of individuals from the sending state that are residing in the receiving state. The data is originally from the World Bank (Özden et al., 2011), which provides migrant stocks for each pair of countries every ten years from 1960 to 2000. Because this data is reported at ten year intervals, we use interpolated data from Miller and Peters (2018).[10] The data come from each state's census data. As such, it accounts for: temporary and permanent migrants; those who came as voluntary and forced migrants; and those who came through regular and irregular channels. It also accounts for under-reporting by any single country. We expect by H1 that larger MIGRANT STOCKS will correspond to an increase in prosecutions.[11]

Atrocities

Our second main explanatory concept is the level of atrocities in the sending state as the source for demand for prosecutions. We measure this in two ways. First, we include a measure of the sending state's regime type using Polity (Marshall, Gurr and Jaggers, 2016). The variable DEMOCRACY (SS) has higher values when the receiving state is more democratic. We expect that higher values of this variable will correspond to fewer atrocities in the sending state. By H1, we therefore expect that DEMOCRACY (SS) will decrease the likelihood of a prosecution. Second we examine the political terror score (PTS (SS)) for the country Gibney et al. (2020). The score ranges from 1 (countries under secure rule of law) to 5 (countries where leaders routinely murder, disappear, and torture the general population). We use their data coded from U.S. Department of State reports as they have the most coverage. Due to data coverage, this variable is only available starting in 1976; our regressions thus cover 1976-2017. This covers most of the UJ cases and our results are robust to dropping this variable.

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[10]We log all migration data due to the right-skew of the data.

[11]In our robustness analysis, which is discussed below, we use alternative measures of migration.

We expect there to be a delay between the time period in which an atrocity occurs, and the actual filing of a universal jurisdiction case. In addition, we want account for conflicts and human rights abuses that may go on over a longer period. In our main analysis, we use the average value of both of these variables over the last 20 years.[12]

Responsiveness

Our third explanatory concept is government responsiveness in the receiving state, which affects the extent to which government preferences are affected by public pressure from migrants. Our analysis uses three measures of this concept. First, we include a measure of the recipient state's regime type using Polity (Marshall, Gurr and Jaggers, 2016). The variable DEMOCRACY (RS) has higher values when the receiving state is more democratic. We expect that a more democratic government should be more responsive to the concerns of individual residents, including migrants. Additionally (or alternatively), democracies maybe more likely to be concerned about reputational costs of *not* pursuing a case. We therefore expect by H3 that DEMOCRACY (RS) will increase the likelihood of a prosecution.

Second, we include a measure of the recipient state's population from Heston, Summers and Aten (2011). We believe that higher values of POPULATION (RS) will make a government less responsive to the concerns of a given set of migrants.[13] Accordingly, we expect by H3 that POPULATION (RS) will decrease prosecutions.

Third, we include the left-right orientation of the receiving state because we expect that left parties are more likely to support both human rights and internationalism and thus should be more likely to prosecute UJ cases. Specifically, we include a measure of the ideology of the executive because prosecutions fall under the executive branch of government. The variables on ideology comes from Scartascini, Cruz and Keefer (2018). We include an indicator for CENTER (RS) and LEFT (RS) governments; Right governments are the excluded category. Accordingly, we expect by H3 that CENTER (RS) and LEFT (RS) governments will increase prosecutions.

Cost

Our final explanatory concept is the cost of prosecutions for the receiving state.

First, we examine whether the recipient state has ratified the Rome Statute, which created the International Criminal Court. The variable ROME STATUTE (RS) is coded as 1 for years in which the receiving state is a member of the treaty, and 0 otherwise.[14]. States that are members of

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[12]We have used different lag structures as well as the maximum and minimum and found similar results. See Appendix figures **??**.

[13]Population is logged due to right-skew of the data.

[14]Our robustness checks also examine the impact of being a signatory of the Rome Statute.

this treaty have an international legal obligation to give full effect to the treaty by implementing it into their domestic legislation. This implementation includes defining international crimes in their domestic criminal code and may include giving universal jurisdiction to the state's laws and courts. Membership in the Rome Statute should therefore increase the likelihood that crimes against humanity, genocide, torture, and war crimes are defined as crimes under domestic law, and that domestic courts have universal jurisdiction over them, thereby enabling universal jurisdiction prosecutions.[15] We expect by H4 that ROME STATUTE will increase universal jurisdiction cases.

Next, we ask whether the receiving state's wealth affects prosecutions. We expect that the variable GDPPC (RS) (logged; from Bank, 2015) will correspond to more state capacity to investigate and prosecute crimes. This capacity should lower the relative cost of prosecution for the receiving state. We therefore expect by H4 that GDPPC (RS) will increase cases.

Third, we include a measure for whether the receiving state is an OECD Member (OECD (RS)). Such states are both democratic and highly industrialized, suggesting that they have the resources to conduct universal jurisdiction prosecutions. We expect by H4 that being an OECD Member will increase the likelihood that a receiving state prosecutes.

We then control for whether the receiving state has had a prior successful prosecution of a universal jurisdiction case. The variable PRIOR SUCCESS (RS)) is coded as 1 for all years in which the receiving state has previously held a universal jurisdiction trial and the domestic court found the defendant guilty. Such a successful prosecution demonstrates to victims (and to us as researchers) that the recipient state's domestic laws are amenable to claims of universal jurisdiction. Additionally, a Prior Successful Prosecution creates judicial precedent in states with common law system and enables officials to learn how these cases should be prosecuted and tried (Langer and Eason, 2019), lowering the cost of future prosecutions. We expect by H4 that PRIOR SUCCESS (RS)) will increase cases.

We also include a measure of the sending state's wealth, which is the variable GDPPC (SS) (logged; from Bank, 2015), in case receiving states are concerned about prosecuting defendants from wealthier states.

Finally, we include some dyadic measures that may make the receiving state less likely to want to prosecute someone from the sending state (Langer, 2011). These include TRADE, which is the percent that dyadic trade makes up of the total trade of the receiving state and an indicator for ALLIANCE between the receiving and sending states (of War Project, 2013). We also include an

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[15]Ratification may also indicate government responsiveness as it shows that at least some past government thought that respecting human rights is important.

indicator variable for SHARED LANGUAGE between the sending and receiving state, as this likely reduces the cost of collecting evidence and interviewing witnesses (Melitz and Toubal, 2014)

Control Variables

Finally, we include numerous control variables in our analysis. For the sending state, we include a measure of whether international prosecutions are occurring during a given year. The variable INTERNATIONAL PROSECUTIONS (SS) is coded as 1 for years in which individuals from the sending state are either under investigation by the ICC, or subject to the jurisdiction of an international or mixed criminal tribunals.[16]. We are agnostic about the expected effect of this variable. While international prosecutions may encourage receiving states to respond to migrant demands for justice, they may also result in the transfer of defendants to international tribunals.[17]

For the receiving state, we include variable PRIOR CASE (RS), which indicates whether the state has previously considered a universal jurisdiction complaint from any sending state. We expect that this variable will have a positive effect on universal jurisdiction cases by motivating new demands for justice by migrants from other sending states. We include an indicator variable for MAJOR POWER (RS) states, which equals 1 if the receiving state is one of the permanent 5 members of the UN Security Council or, after 1991, Germany or Japan. We also include fixed effects for the receiving state. These fixed effects capture any unchanging characteristics like legal origin and unobservable characteristics of the receiving state that might affect whether they will prosecute these states.

Finally, as dyadic-level controls, we include FORMER COLONY, which is an indicator variable that equals 1 if the sender-receiver state pair had a prior colonial relationship, as former colonies tend to send many migrants to the former colonial power and the former colonial relationship might lead to increased attention to UJ eligible crimes. We also include DISTANCE from Gleditsch and Ward (2001) and SHARED BORDERS from of War Project (2007), as both migration and attention to crimes abroad may decrease with distance. We also include a variable for the COLD WAR and the WAR ON TERROR as these periods of time may affect the likelihood of UJ cases, with fewer cases during the Cold War and more during the War on Terror.

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[16]These latter tribunals include: the International Criminal Tribunal for the former Yugoslavia, the International Criminal Tribunal for Rwanda, the Special Court for Sierra Leone, the Extraordinary Chambers in the Courts of Cambodia, the Special Tribunal for Lebanon, and the Extraordinary African Chambers.

[17]See Schabas (2003) for exampes of both possibilities.

4.2 Estimation

We test our hypotheses in three ways. For the count of cases (models 1 and 2) and the any case (models 3 and 4) variable, we run an OLS regression of the dependent variable on our variables of interest. We use OLS as it requires fewer assumptions about the data and is easier to interpret. We also include receiving country fixed effects to control for any unobserved factors.[18] Second, we run a logit on the first instance in which a case is filed in the receiving country(models 5 and 6). Similar to the conflict literature, these models capture onset of UJ within a dyad by dropping observations for the years after the first case (Beck, Katz and Tucker, 1998). Also included are a year since 1957 (the start of the dataset) variable, years since squared, and years since cubed to correct for dependence between observations in dyads, as suggested by Carter and Signorino (2010). In odd numbered models, we include all cases while in even numbered models we drop cases in which the defendant is listed as a "John Doe." This is because we cannot identify the sending state of the defendant, which is especially important for cases that arise from conflicts that cross borders. Due to data availability, all regressions cover 1976 through 2017. All models were run with robust standard errors clustered by dyad.

We note that we are not claiming causality with this estimation strategy. While we do control for confounders, we think that reverse causality is possible. It is possible that victims of a crime that is eligible for a UJ case but has gone unpunished at home may seek to move to a state that engages in UJ prosecutions, although we leave this for future research. In the logit models, we drop all subsequent prosecutions of UJ in a dyad pair to see if migration predicts the first case and examine only the first case a receiving state has ever investigated. We think there will be less certainty about whether a state would prosecute a case before the first prosecution and therefore individuals would be less likely to migrate to that state specifically in hopes of having a UJ case heard.

5   Results

Table 3 shows the results of the regressions on the total count of cases per year; whether there was any case in a given year; and the first case that the receiving state undertook. We begin by discussing what variables predict the total number of cases (model 1 and 2). We find a highly statistically significant effect of the stock of migrants in the country on all three versions of UJ

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[18]Results are robust to not including these fixed effects.

cases, consistent with H1. The substantive effect of the migrant stock is also large; going from the 25th percentile (no immigrants form the sending country to the receiving country) to the 75th percentile (300 immigrants) leads to three times as many cases in a year; increases the probability of any case 8-fold; and increases the probability of the first case 9-fold.

[Insert Table 3 here.]

We also find support that the level of atrocities in the sending state affects the likelihood of a UJ case. Sending state that have been less democratic or that have engaged in more political terror in the last 20 years are more likely to see UJ cases against citizens from their state. Both DEMOCRACY (SS) and PTS (SS) have the hypothesized sign and are statistically significant at conventional levels, consistent with H2.

Next, we find little support for the responsiveness hypothesis on the count of cases. In contrast to H3, more democratic states, as measured by polity, are less likely to have these cases than less democratic countries and states that are more populous are likely to have more of these cases. Yet, this may be an artifact of the polity score as only 3 cases have been brought in an autocracy. Larger states are more likely to pursue these cases, perhaps because they have more legal resources. Finally, the cases are equally likely regardless of the ideology of the government.

We also find less support for H4 on the costs of pursuing these cases. Signing the Rome Statute has a positive effect on whether the state has any cases but does not seem to have an effect on the number of cases or the first case. OECD states are more likely to pursue a case. Concerns over relations with the sending country don't seem to matter: the wealth of either state has no effect nor does the trade or alliance relationship. Experience taking a case all the way to trial and winning does not seem to have much of an effect; however, shared language has a positive effect, likely because it makes collecting information easier.

In terms of control variables, we find some surprising results. Whether there is an international investigation does not seem to matter, perhaps because some states are emboldened to take action by an international investigation while others are put off. While having had taken a case to trial and winning had not had an effect, once states have a case, they are more likely to have more cases. This effect likely comes from migrant learning about how the UJ process works. Whether or not the receiving state is a major power or if the sending country is a former colony also has little effect. States are more likely to prosecute individual from distant states, which may be an artifact of the fact that most of the states that prosecute these cases are in the Global North and most of

the crimes are committed in the Global South. Finally cases were somewhat less likely during the Cold War than after.

5.1 Robustness checks

How robust are our findings? We begin with a series of robustness checks using alternative measures of migration. Our theory suggests that migrant stocks will affect universal jurisdiction cases. However, another possibility is that the arrival of new migrants will be most salient. To test this alternative explanation, we replicate our results using several alternative measures in figure 3. First, we examine the results above without fixed effects. Next we examine migrant flows, which are the number of migrants entering a receiving state from a sending state in a given year. We use migrant flow data from Fitzgerald, Leblang and Teets (2014) (IMMIGRATION FLOWS), which collected the data from national statistics bureaus and other sources. While these datasets provide more granular data than our measure of migrant stocks, they are only available for a small set of mostly OECD country. From this data, we create two additional variables: MIGRANT STOCK LAST 5 YEARS, which sums the immigration flows data for the last 5 years, and MIGRANT STOCK LAST 10 YEARS which does the same for the last 10 years.

[Insert Figure 3 here.]

We also consider the impact of stocks of refugees, which are migrants who demonstrate that they were persecuted in the sending state. For our first measure of refugees (REFUGEE STOCKS), we use data from the United Nations High Commissioner for Refugees, which has extensive missing data. For our second two measures of refugees (REFUGEE STOCKS (INTERPOLATED) and REFUGEE STOCKS (LASSO)), we use the two imputed measures from Marbach (2018). Finally, we consider the stock of economic migrants alone, which we calculate as the migrant stock minus the refugee stock (lasso).

Figure 3 replicates model 1, 3, and 5, respectively, replacing the migration variable with one of these alternatives and plots the coefficient on migration variable. We include the coefficient from Table 3, listed as the fixed effects model in subfigures (a) and (b) and as the first variable in subfigure (c). As we can see from the figure, regardless of which measure of migration we use, the effect of migration is positive and often significant at conventional levels. This provides additional support for H1.

Finally, recall that our main analysis used 20-year average measures of atrocities in the

sending state. This average was meant to represent delays between the time period in which atrocities occurred and the time at which migrants complained to the receiving state, as well as the duration of atrocities. To ensure the robustness of our results, we estimated our results using alternative measures, calculating the average over the last 5 and 10 years and looking at the minimum and the maximum value over those same time periods. As in figure 3, figure 4 plots the coefficients on the different measures of democracy and political terror, respectively. The coefficients come from regressions that replicate model 3 but replace the atrocity variables for both democracy and political terror. Again, the results are very robust to using these alternative measures of atrocities.

We finally examine alternative measures of universal jurisdiction cases. Recall that our main analysis focuses on complaints. Cases that proceed all the way to trial are exceptionally rare because they require that prosecutors have both abundant evidence and the physical presence of the accused. Nonetheless, as a second set of robustness checks, we re-estimate models 1 through 6 on TRIALS.[19]

We find very similar results on migration; more migration leads to more trials. Interestingly, the level of atrocities does not affect whether or not a case goes to trial. This may be the product of the ability to gather enough evidence or to find the defendant to go to trial. In terms of the other variables, trials are more likely against defendants from poorer countries; when there has been a prior case in the receiving country; and against defendants from sending states that are farther from the receiving state.

6   Conclusion

Our evidence collectively suggests that migration generates demands for transnational justice. Many migrants who flee repression and war arrive in receiving states with grievances about prior crimes that occurred in the sending state. These grievances can motivate them to seek justice in their new state. As one human rights activist noted, "A major challenge in any universal jurisdiction case is creating the necessary political will in the forum state" (Brody, 2017, 23). Our empirical evidence suggests that migrant populations help to generate this political will.

To return to our opening example, anger over Pinochet's impunity prompted Joan Garcés and the President Allende Foundation to file a complaint against Pinochet with a Spanish court in 1996 (Roht-Arriaza, 2005, 13). Garcés lived in Chile in the early 1970s and worked as a political

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[19]See Appendix Table A1 and Figure A1.

advisor to Salvador Allende, the elected president who was overthrown by Pinochet in 1973. Garcés fled Chile and ultimately settled in Spain. The President Allende Foundation was a Spanish philanthropic group that filed a complaint on behalf of the family members of Pinochet's victims. Both complainants argued that Spanish courts could (and should) prosecute Pinochet because he had committed serious international crimes that Chile refused to punish. This complaint was accompanied by ongoing political pressure from Chilean migrants (Roht-Arriaza, 2005, 25). The ultimate result was a Spanish investigation, arrest warrant, and request for extradition, followed by a prolonged legal battle in the UK courts.

Even if criminal proceedings do not ultimately result in a trial and conviction, they nonetheless are important tools for securing justice. For example, the UK ultimately declined to extradite Pinochet because of his medical condition, meaning that Pinochet never faced trial in Spain. Yet action by Spain and the UK ultimately persuaded the Chilean government to pursue prosecutions against Pinochet and his collaborators in Chile (Roht-Arriaza, 2005). Criminal proceedings can also provide victims with important psychological benefits, like proving a venue for them to testify and validating their perceptions of injustice.

More broadly, we believe that our evidence counters the claim of "judicial tyranny" by domestic courts (Kissinger, 2001, 86). While domestic courts may not be able to base their jurisdiction on traditional bases of jurisdiction—like territory or nationality—they do usually have links to the alleged crimes because of migration by victims. Additionally, most states do not allow actual trials to occur unless the defendant is physically present, meaning that prosecuting states often act to deny "safe harbor" to an international criminal within their borders, rather than to merely project neo-imperial power abroad (Langer, 2015b). Indeed, the UK's refusal to extradite Pinochet ultimately stymied the Spanish prosecution. Extradition proceedings therefore serve as a check on overzealous domestic prosecutions (Roth, 2001).

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Figure 1: Universal Jurisdiction Case, 1957–2017
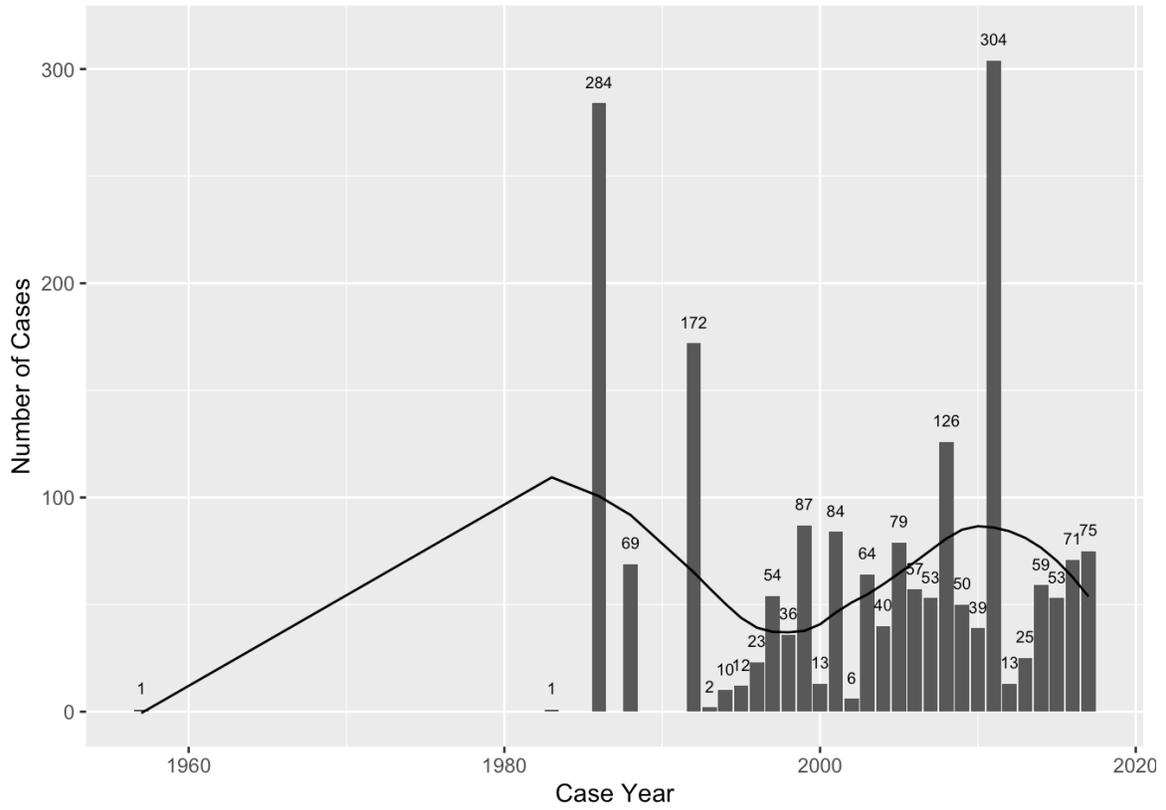
Note: This figure shows the distribution of *Cases*, with a loess line to depict the general trend over time.

Figure 2: Theoretical Expectations

(a) Equilibrium Behavior

M ┄┄┄

Migrant Mobilization

←Government Threshold

←Actual Migrant Behavior

θ*   1

Government Ideology

(b) Equilibrium Outcomes

Don't Prosecute   Prosecute

├──────────────┼──────────────┤
0   θ*   1

Government Ideology
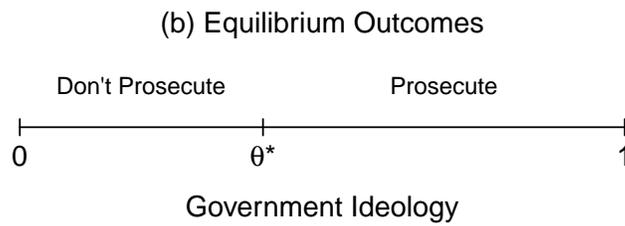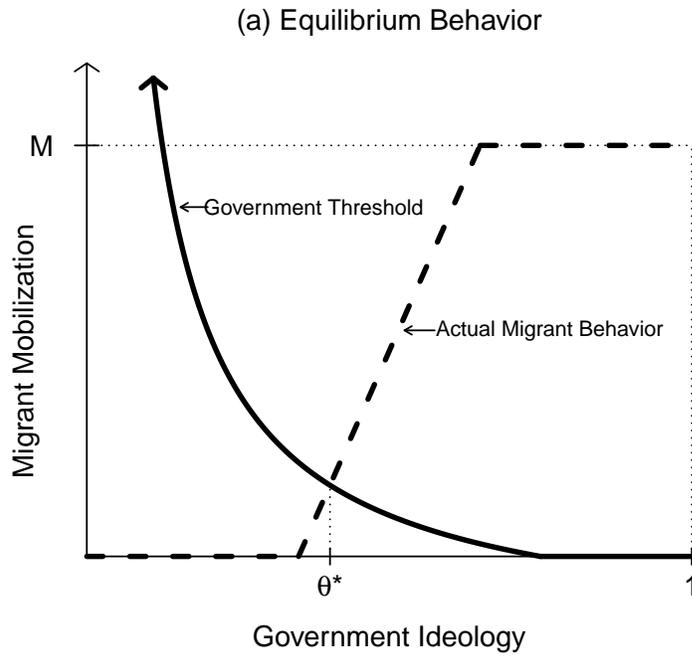
Figure 3: Comparing the Impact of Alternative Migration Measures

(a) CASE COUNT

(b) ANY CASE
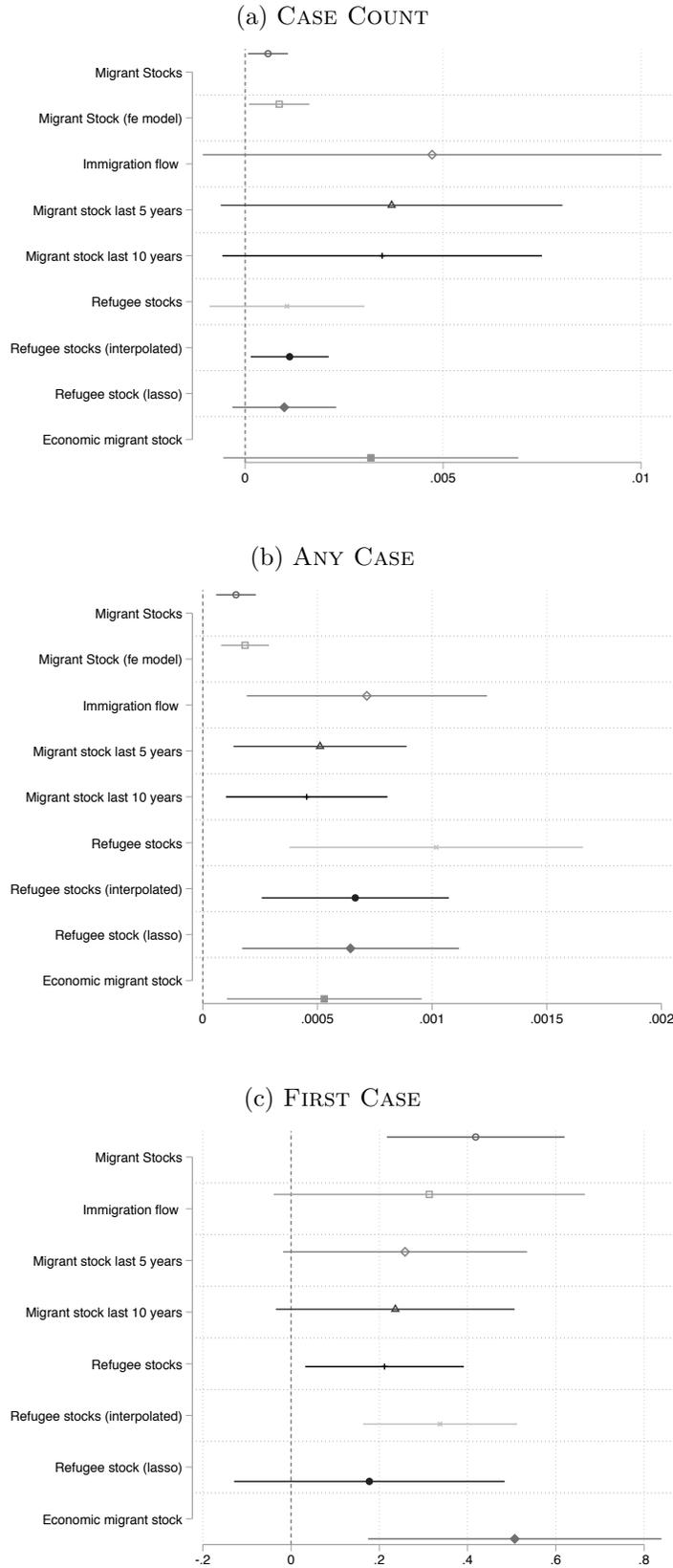
(c) FIRST CASE

Figure 4: Comparing the Impact of Alternative Lag Structures

(a) Lags of DEMOCRACY

(b) Lags of POLITICAL TERROR
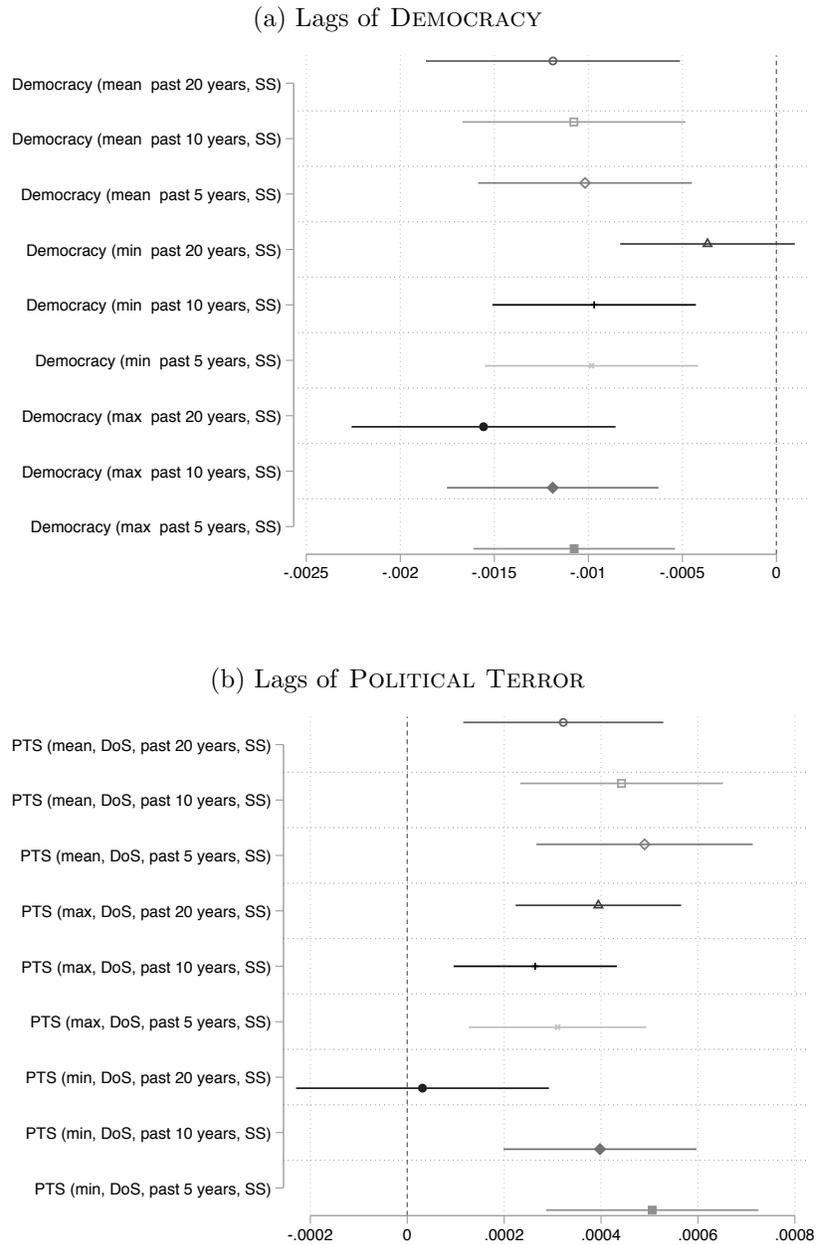
Table 1: Universal Jurisdiction Cases Across Prosecuting States

Prosecuting State	Total Cases	Cases: 1950–2000	Cases: 2000–2017	First Case
Germany	647	177	470	1992
Spain	370	168	202	1996
Canada	217	214	3	1986
France	131	15	116	1993
Sweden	106	2	104	1995
Argentina	93	0	93	2005
United Kingdom	89	75	14	1988
Belgium	78	26	52	1994
Australia	71	71	0	1986
Switzerland	37	3	34	1995
Norway	33	0	33	2005
Austria	30	2	28	1994
Turkey	23	0	23	2009
Netherlands	16	4	12	1994
Denmark	9	2	7	1994
South Africa	7	0	7	2008
Finland	6	0	6	2003
Chile	3	0	3	2016
Greece	3	0	3	2004
Israel	3	2	1	1957
Senegal	3	1	2	2000
Iceland	2	0	2	2003
South Korea	2	0	2	2003
Armenia	1	0	1	2003
Brazil	1	0	1	2017
Colombia	1	0	1	2017
Cyprus	1	0	1	2003
Ireland	1	0	1	2004
Luxembourg	1	1	0	1998
New Zealand	1	0	1	2006
Poland	1	0	1	2004
Russia	1	0	1	2004
Tanzania	1	0	1	2004
United States	1	1	0	2000

Table 2: Universal Jurisdiction Cases Across Defendant Nationalities

Defendant Nationalities	Total Cases	Cases: 1950–2000	Cases: 2000–2017	First Case
Syria	407	0	407	2011
Nazis*	360	356	4	1957
Former Yugoslavia	257	183	74	1992
Argentina	145	145	0	1996
Rwanda	118	27	91	1994
China	76	0	76	2002
Israel	76	0	76	2001
United States	75	0	75	2003
Spain	67	0	67	2010
Morocco	66	1	65	1999
Iraq	47	1	46	1999
Tunisia	43	0	43	2001
Turkey	29	0	29	2016
Mauritania	22	1	21	1999
El Salvador	21	0	21	2008
Cuba	19	7	12	1998
Chile	17	15	2	1994
Paraguay	17	0	17	2013
Cameroon	12	0	12	2001
Uzbekistan	12	0	12	2005
Guatemala	9	9	0	1999
Zimbabwe	8	0	8	2003
Algeria	7	0	7	2001
Democratic Republic of the Congo	7	6	1	1998
Libya	7	0	7	2007
Afghanistan	6	1	5	2000
Congo	6	0	6	2001
Côte d'Ivoire	4	0	4	2001
Liberia	4	0	4	2012
Palestine	4	0	4	2001
Peru	4	4	0	1998
Sri Lanka	4	0	4	2007
Cambodia	3	3	0	1999
Central African Republic	3	0	3	2001
Chad	3	2	1	2000
Indonesia	3	0	3	2004
Russia	3	0	3	2005
France	2	0	2	2002
United Kingdom	2	0	2	2003
Venezuela	2	0	2	2003
Bahrain	1	0	1	2015
Colombia	1	0	1	2005
Ethiopia	1	0	1	2015
Gambia	1	0	1	2017
India	1	0	1	2003
Iran	1	1	0	2000
Lebanon	1	0	1	2001
Madagascar	1	0	1	2012
Nepal	1	0	1	2013
Nigeria	1	0	1	2014
Somalia	1	0	1	2005
Sudan	1	1	0	1997
Suriname	1	1	0	1996

* Includes individuals who committed crimes on behalf of the Nazi German regime.

Table 3: Regressions of Cases on Explanatory Variables, 1976-2007/2012

	Count		Any		First	
Migration (SS → RS)						
Migrant Stocks	0.000861*	0.000797*	0.000184***	0.000184***	0.450***	0.399***
	(0.000388)	(0.000353)	(0.0000533)	(0.0000533)	(0.115)	(0.115)
Atrocities (SS)						
Democracy (mean past 20 years, SS)	-0.00728**	-0.00572*	-0.00119***	-0.00119***	-4.053***	-3.850***
	(0.00278)	(0.00230)	(0.000345)	(0.000345)	(0.929)	(0.901)
PTS (mean, DoS, past 20 years, SS)	0.000151	0.0000254	0.000322**	0.000322**	0.895*	1.076*
	(0.000370)	(0.000310)	(0.000105)	(0.000105)	(0.444)	(0.461)
Responsiveness (RS)						
Democracy (RS)	-0.00121***	-0.00118***	-0.000511***	-0.000511***	0.0609	-0.103
	(0.000362)	(0.000343)	(0.000137)	(0.000137)	(2.674)	(2.612)
Population (RS)	-0.00711*	-0.00516**	-0.00219***	-0.00219***	-0.304	-0.322
	(0.00312)	(0.00177)	(0.000492)	(0.000492)	(0.178)	(0.192)
Center (RS)	-0.000195	-0.000579	0.000205	0.000205	0.783	0.751
	(0.000960)	(0.000692)	(0.000158)	(0.000158)	(0.933)	(0.935)
Left (RS)	0.00377	0.00263	0.000265	0.000265	0.555	0.464
	(0.00209)	(0.00146)	(0.000180)	(0.000180)	(0.662)	(0.685)
Cost (RS)						
Rome Statute (RS)	-0.0000146	-0.0000609	0.000474*	0.000474*	-1.062	-1.075
	(0.000964)	(0.000921)	(0.000225)	(0.000225)	(0.616)	(0.636)
GDPpc (RS)	0.00114	0.00183	-0.000242	-0.000242	0.433	0.483
	(0.00287)	(0.00235)	(0.000276)	(0.000276)	(0.380)	(0.417)
OECD (RS)	0.0490	0.0222	0.0155***	0.0155***		
	(0.0389)	(0.0188)	(0.00424)	(0.00424)		
Prior Success (RS)	-0.0157	-0.00957	0.000707	0.000707		
	(0.0109)	(0.00508)	(0.00182)	(0.00182)		
GDPpc (SS)	0.000324	0.0000735	0.000123	0.000123	0.868	1.048
	(0.000275)	(0.000153)	(0.0000696)	(0.0000696)	(0.546)	(0.610)
Trade	0.000603	0.000469	0.000245*	0.000245*	-1.755***	-1.722**
	(0.000396)	(0.000350)	(0.0000992)	(0.0000992)	(0.532)	(0.544)
Alliance	-0.00651	-0.00434	-0.000567	-0.000567		
	(0.00501)	(0.00374)	(0.000312)	(0.000312)		
Shared language	0.00235	0.00179	0.00113*	0.00113*	2.253***	2.236***
	(0.00184)	(0.00156)	(0.000552)	(0.000552)	(0.610)	(0.668)
Controls						
International Prosecutions (SS)	0.0280	0.0289	0.00729	0.00729		
	(0.0185)	(0.0185)	(0.00388)	(0.00388)		
Prior Case RS	0.0123*	0.0115**	0.00242***	0.00242***		
	(0.00485)	(0.00445)	(0.000576)	(0.000576)		
Major Power (RS)	-0.0170	-0.000702	-0.00255	-0.00255		
	(0.0181)	(0.00505)	(0.00199)	(0.00199)		
Former Colony	0.0204	0.00855	0.00295	0.00295		
	(0.0153)	(0.00794)	(0.00296)	(0.00296)		
Distance	0.00213**	0.00203**	0.000540**	0.000540**		
	(0.000760)	(0.000737)	(0.000182)	(0.000182)		
Cold War	-0.000314	0.0000190	-0.000304*	-0.000304*		
	(0.00101)	(0.000826)	(0.000127)	(0.000127)		
War on Terror	0.00330	0.00257	0.000235	0.000235		
	(0.00196)	(0.00170)	(0.000318)	(0.000318)		
Observations	161247	161247	161247	161247	238336	238336

Models 1-4 include receiving country fixed effects; data is from 1976-2007 due to data availability. Models 5 & 6 are for 1976-2012. Contiguity variables included in models 1-4 but not shown; they are dropped in models 5 & 6 to ensure the model converges. See text for details about the variables. All models include robust standard errors clustered by dyad. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix: Additional Results

Figure A1: Comparing the Impact of Alternative Migration Measures on Trials

(a) CASE COUNT

(b) ANY CASE

(c) FIRST CASE
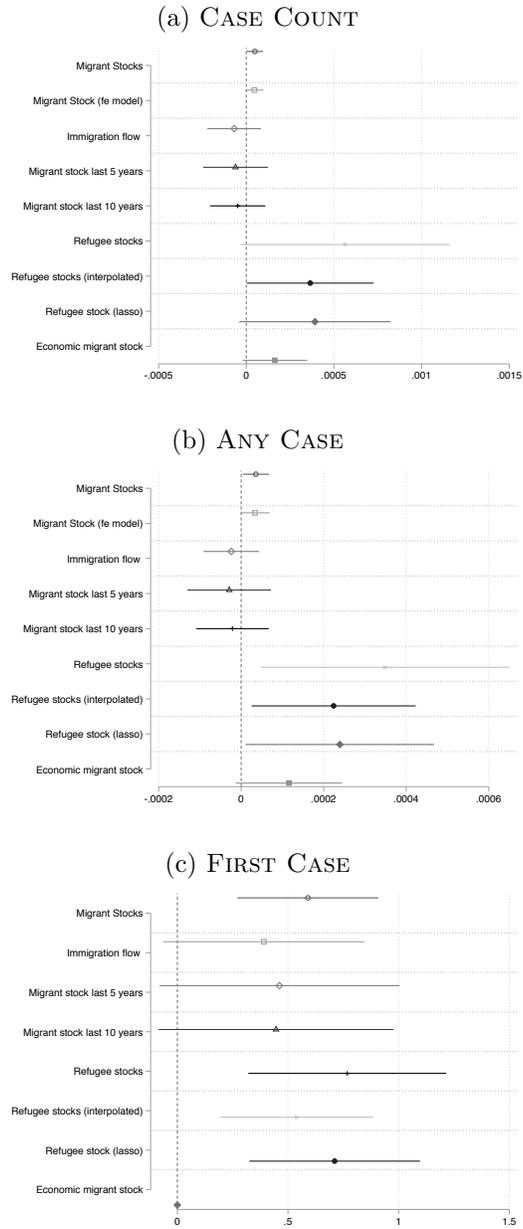
Table A1: Regressions of Trials on Explanatory Variables, 1976-2007/2012

	Count		Any		First	
Migration (SS → RS)						
Migrant Stocks	0.0000462^{+}	0.0000462^{+}	0.0000338^{+}	0.0000338^{+}	0.596^{***}	0.596^{***}
	(0.0000263)	(0.0000263)	(0.0000181)	(0.0000181)	(0.169)	(0.169)
Atrocities (SS)						
Democracy (mean past 20 years, SS)	-0.000174	-0.000174	-0.0000883	-0.0000883	-0.526	-0.526
	(0.000157)	(0.000157)	(0.0000723)	(0.0000723)	(1.536)	(1.536)
PTS (mean, DoS, past 20 years, SS)	0.00000485	0.00000485	0.0000135	0.0000135	-0.492	-0.492
	(0.0000447)	(0.0000447)	(0.0000311)	(0.0000311)	(0.346)	(0.346)
Responsiveness (RS)						
Democracy (RS)	-0.000121	-0.000121	-0.0000953	-0.0000953	8.175	8.175
	(0.000147)	(0.000147)	(0.0000799)	(0.0000799)	(18.40)	(18.40)
Population (RS)	-0.000391	-0.000391	-0.000155	-0.000155	-0.396^{*}	-0.396^{*}
	(0.000461)	(0.000461)	(0.000177)	(0.000177)	(0.158)	(0.158)
Center (RS)	-0.00000445	-0.00000445	-0.00000716	-0.00000716	0.302	0.302
	(0.0000445)	(0.0000445)	(0.0000239)	(0.0000239)	(1.173)	(1.173)
Left (RS)	-0.0000872	-0.0000872	-0.0000614	-0.0000614	0.0954	0.0954
	(0.0000793)	(0.0000793)	(0.0000607)	(0.0000607)	(0.786)	(0.786)
Cost (RS)						
Rome Statute (RS)	-0.0000171	-0.0000171	-0.00000298	-0.00000298	0.702	0.702
	(0.0000955)	(0.0000955)	(0.0000836)	(0.0000836)	(1.349)	(1.349)
GDPpc (RS)	-0.000143	-0.000143	-0.0000344	-0.0000344	1.166^{+}	1.166^{+}
	(0.000194)	(0.000194)	(0.0000646)	(0.0000646)	(0.691)	(0.691)
OECD (RS)	0.00177	0.00177	0.000459	0.000459		
	(0.00309)	(0.00309)	(0.00126)	(0.00126)		
Prior Success (RS)	0.000311	0.000311	0.000448	0.000448		
	(0.000676)	(0.000676)	(0.000762)	(0.000762)		
GDPpc (SS)	-0.0000431^{+}	-0.0000431^{+}	-0.0000293^{+}	-0.0000293^{+}	-0.747^{*}	-0.747^{*}
	(0.0000235)	(0.0000235)	(0.0000171)	(0.0000171)	(0.357)	(0.357)
Trade	0.00000739	0.00000739	0.0000124	0.0000124	0.0792	0.0792
	(0.0000275)	(0.0000275)	(0.0000193)	(0.0000193)	(1.580)	(1.580)
Alliance	0.000408	0.000408	0.000265	0.000265	-0.337	-0.337
	(0.000344)	(0.000344)	(0.000228)	(0.000228)	(1.249)	(1.249)
Shared language	-0.000163	-0.000163	-0.000145	-0.000145	0.242	0.242
	(0.000172)	(0.000172)	(0.000127)	(0.000127)	(0.999)	(0.999)
Controls						
International Prosecutions (SS)	0.00351	0.00351	0.00192	0.00192		
	(0.00283)	(0.00283)	(0.00131)	(0.00131)		
Prior Case RS	0.000680^{+}	0.000680^{+}	0.000368^{+}	0.000368^{+}		
	(0.000377)	(0.000377)	(0.000190)	(0.000190)		
Major Power (RS)	0.000221	0.000221	-0.0000172	-0.0000172		
	(0.000769)	(0.000769)	(0.000647)	(0.000647)		
Former Colony	0.00354	0.00354	0.00181	0.00181		
	(0.00303)	(0.00303)	(0.00138)	(0.00138)		
Distance	0.000174^{+}	0.000174^{+}	0.000110^{*}	0.000110^{*}		
	(0.0000930)	(0.0000930)	(0.0000551)	(0.0000551)		
Cold War	0.00000202	0.00000202	0.000000685	0.000000685		
	(0.0000854)	(0.0000854)	(0.0000274)	(0.0000274)		
War on Terror	0.000172	0.000172	0.0000658	0.0000658		
	(0.000167)	(0.000167)	(0.0000873)	(0.0000873)		
Observations	161247	161247	161247	161247	281285	281285

Models 1-4 include receiving country fixed effects; data is from 1976-2007 due to data availability. Models 5 & 6 are for 1976-2012. Contiguity variables included in models 1-4 but not shown; they are dropped in models 5 & 6 to ensure the model converges. See text for details about the variables. All models include robust standard errors clustered by dyad. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$