

# Migration and the Demand for Transnational Justice\*

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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 cases? 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 pressure on local governments to conduct criminal investigations and trials for crimes that occurred in other states, by and against foreign nationals. The legacy of colonialism can have an indirect effect by shaping migration decisions and the location of diaspora communities. However, the underlying driver of transnational justice is migration.

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# 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 trial 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 case as vindication for Pinochet’s victims and a triumph of the international community (Roht-Arriaza, 2005). Other observers described the case 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 trial is not an isolated example. Beginning in the mid-1990s, Spanish courts investigated serious international crimes committed in foreign states, including Argentina, Cuba, and Guatemala. 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? We argue that one key factor is migration—more Spanish migrants come from Argentina, Chile, Cuba, and Guatemala than from Bolivia, Honduras and Nicaragua.

Much of the debate over *Pinochet* and similar cases 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 (Lutz and Sikkink, 2001).

In contrast, we argue that migration—the movement of people across state borders—generates demands 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. These grievances can motivate migrants to seek transnational justice when they arrive in a new state. Migrants have numerous tactics that they can use to try to achieve their goals, including: reporting crimes to state authorities; providing evidence and witness testimony; increas-

ing public awareness of prior atrocities; and lobbying government elites to take action. In some states, they can even launch private prosecutions. However, these demands will only be successful in pressuring a receiving state to supply transnational justice if enough migrants demand that the receiving state take action. Our key theoretical claim is that high levels of migration increase the likelihood of criminal cases by the receiving state for acts that occurred in the sending state. We additionally argue that attributes of both the sending and receiving states can affect transnational justice, including: the magnitude of atrocities in the sending state; the responsiveness of government officials in the receiving state to political demands; and the economic, legal and political costs of cases.

We test our theory using extensive statistical analysis of many different measures of universal jurisdiction cases, including the initiation of a case, investigation by state entities, formal proceedings, issuance of an arrest warrant, whether an arrest occurred, and whether a trial was held. Each of these measures is subject to different kinds of potential measurement errors and bias, but they collectively demonstrate robust support for our theoretical arguments. As our main explanatory variable, 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 variables that affect cost. Overall, our statistical analysis provides compelling support for our theoretical argument.

Our argument and findings have two important normative implications for the study of transnational justice. First, we challenge the common narrative that transnational justice is driven by colonialism. Past colonial relationships can affect contemporary migration through factors like shared language and diaspora communities. However, once we control for migration patterns, there is no statistical evidence that colonial status increases transnational justice. Our analysis thus rebuts important policy critiques of transnational cases by demonstrating that these cases are driven by the movement of people, rather than colonialism. Second, our argument and findings highlight the importance of migrants as protagonists with agency. Activists and NGOs in receiving states can certainly help migrants to achieve justice. But migrants themselves shape outcomes, particularly in states that allow victims to participate more in criminal proceedings (Langer, 2011; Michel and Sikkink, 2013).

## 2 Background

We begin by providing a general overview of the intellectual justification for universal jurisdiction cases. We then describe important empirical patterns in the data on these cases. 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. States 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. Universal jurisdiction is one important tool for pursuing *transnational justice*, which is the use of domestic courts in foreign states to secure remedies for prior legal violations.

Under international law, states may assert such universal jurisdiction to prosecute individuals for a set of core international crimes, which include crimes against humanity, genocide, torture, and war crimes (Langer, 2015a). Universal jurisdiction is included in some international treaties, like the UN Convention Against Torture (1984).<sup>1</sup> 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, 2015a; Verdier and Voeten, 2014).

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

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.<sup>2</sup> If no state was willing to project its domestic jurisdiction to include the high seas, pirates could easily escape punishment. 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 punish 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 trial of Adolph Eichmann, a prominent Nazi official 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

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

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, 2019*b*). 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 criminal cases 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 government 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 often 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 new data that significantly expands on data published in Langer (2011) and subsequently updated in Langer and Eason (2019) and by Langer in 2020-2021. 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 2019; and

- fully or partially relied on the principle of universal jurisdiction under which a state may have authority to 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 alleged crime was committed.

The data thus does not include information on civil lawsuits, which are 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 the original 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); updated in a similar manner by Langer and Eason (2019) and by Langer in 2020-2021.

Our expanded data builds on this process by collecting additional information about the multiple possible stages of criminal proceedings, including the timing of:

1. INITIATION: year in which a universal jurisdiction case began by complaint filed by the alleged victim or on their behalf by an NGOs or other groups or by state authorities by their own motion;
2. INVESTIGATION: year (if any) in which a receiving states entity take investigative measures or inquires whether the territorial state or other jurisdiction has investigated or prosecuted the case;
3. FORMAL PROCEEDINGS: year (if any) in which formal charges or proceedings are brought by a prosecutor or judge;

4. ARRESTS: year (if any) in which an arrest warrant was issued for the defendant by the receiving state or the defendant was arrested; and
5. TRIAL: year (if any) in which a criminal trial was held.

Each of these different variables comes with potential measurement error and bias. For example, INITIATION is a broad measure of case 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. This variable almost certainly underestimates migrant demands because we, as researchers, only observe criminal complaints that are documented by the various sources described above. Additionally, it does not accurately distinguish between migrant demands for transnational justice and the receiving state's willingness to supply such justice because of variation across states in domestic criminal procedures. For example, this measure includes migrant complaints to authorities, which can be ignored or overlooked in some states. In contrast, we are extremely confident in our measurement of trials, which are extremely well documented and publicized. Additionally, trials occur when there is both a demand for and supply of transnational justice. However, TRIAL is an under-inclusive measure of transnational justice because 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, and difficulty in apprehending individuals accused of crimes who are still mentally and physically competent to face trial. None of these measures perfectly capture demand and/or supply for international justice. But we believe that they collectively capture the practice of transnational justice, which is our substantive interest, and our empirical analysis examines all of these different variables. Our hope is that by using multiple measures of the prosecution process, we can provide compelling and robust evidence of our theoretical argument.

## 2.2 Variation in Universal Jurisdiction Cases

One empirical question is—are cases like the Eichmann trial aberrations or common events? Figure 1 shows the number of universal jurisdiction cases plotted over time using our INITIATION measure.<sup>3</sup> This figure shows that universal jurisdiction cases are rare — there are only 2,162 cases from 1957

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<sup>3</sup>Appendix Figures A1–A4 show similar plots for the steps to trial and trial.



to 2020. However, these cases 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 while these cases go in waves, they seem to be increasing on average.

[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 total INITIATION count and includes the first year they initiated a case, the year of first trial (if any) and number of TRIALS (if any). 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 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). We also see that trials are much less common but that the states that have more initiations tend to have more trials.

[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 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, Table 2 shows that 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. 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; Mégret, 2015; 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 cases is important because of the possible long-term effects of these cases. Many scholars argue that universal jurisdiction cases can have long-lasting effects on future human rights practices at the domestic level. For example, some scholars have argued that universal jurisdiction cases 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 cases 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 cases thus risk causing more atrocities than they would prevent because these cases 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. In addition, some states allow alleged victims and other actors to become private prosecutors or civil plaintiffs in a criminal case, providing them with more legal tools to file complaints and move a case through different procedural steps to trial. This legal power would thus make more likely that a case moves through different procedural steps towards trial (Langer, 2011; Michel and Sikkink, 2013). 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 (Lutz and Sikkink, 2001). For example, Sikkink (2011) studies 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. This perspective suggests that the first universal jurisdiction case in a receiving state is fundamentally different from later cases. The first case is arguably much harder since lawyers and judges must craft new legal arguments and doctrines that can be more easily applied in later cases. We account for this difference in our empirical analyses below.

### **3 Theory**

Our theoretical argument about the impact of migration on transnational justice is driven by the strategic interactions of two groups of people. The first group of people is migrants who are fleeing atrocities and violence in the sending state. Migrant groups are made up of diverse actors. Many migrants are themselves victims of atrocities and their family-members. Other migrants are often individuals who witnessed atrocities against others and were harmed economically by violence and/or warfare. Finally, some migrants may themselves be the perpetrators of atrocities. These perpetrators can include individuals who are genuinely remorseful for their past actions, as

well as individuals who are not.<sup>4</sup> For the purposes of our argument, we assume that a migrant population contains more individuals who support transnational justice (e.g. victims, witnesses, and remorseful perpetrators) than individuals who oppose transnational justice.

Migrants also vary in their other personal characteristics. News broadcasts often emphasize the poverty and despair of refugees without education or economic resources who flee violence in broken states. Yet many migrants are highly educated, well-informed about the institutions and laws of their receiving state, and have economic resources that allowed them to flee in the first place. Such individuals are often given work visas and qualify as economic migrants, yet they can still be victims and witnesses of atrocities in their home state.

For example, as discussed in the Introduction, many migrants fled atrocities in Argentina and Chile by moving to Spain. This migration was enabled by close cultural links with Spain, as well as large familial and diaspora communities in Spain. Qualitative accounts of transnational justice in Spain highlight how many Argentine and Chilean migrants were highly educated doctors, engineers, and lawyers, all of whom had the economic resources to move to Europe as economic or political migrants (Roht-Arriaza, 2005). While these migrants certainly faced challenges in their new home state, they all already spoke the native language, many had previously travelled extensively between Spain and their home country, and many had familial and professional networks in Spain. These migrants had the knowledge, skill, and tools to seek justice in Spain.

The second group of people who are relevant to our argument are government officials of the receiving state. Transnational justice usually involves the efforts of diverse government officials in the receiving state. Within the justice system, transnational justice requires the cooperation of police officers, public prosecutors, and judges. Political branches of government are also often important, as transnational justice sometimes requires intervention by executives who oversee law enforcement. Successful prosecutions may also require intervention by legislators to provide resources or revise criminal codes. All of these various actors can themselves vary in their resources and attitudes toward transnational issues. While some government officials may support transnational justice on ideological grounds, others may oppose it.

For example, qualitative accounts of Spanish prosecutions highlight the diverse viewpoints of government officials involved in prosecuting international crimes in Argentina and Chile. Some judges—like Baltasar Garzón—were more inherently supportive of transnational prosecutions than other judges—like Manuel Garcia Castellón. Though these two Spanish judges were initially in

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<sup>4</sup>A canonical example of the former category would be Drazen Erdemović, a Serbian soldier who confessed to reporters about participating in atrocities against Bosnian Muslims.

charge of the Argentine and Chilean investigation respectively, only Baltasar Garzón issued an arrest warrant against Pinochet while he was still in London (Roht-Arriaza, 2005, 33-35). Amongst politicians, the United Left party was strongly in favor of transnational prosecutions, as shown by the fact that it became a private prosecutor in the Argentine and Chilean investigations in Spain. In contrast, right-wing Prime Minister José María Aznar was not inherently supportive of these investigations. He had incentives against interfering with them because Spanish popular opinion strongly supported these prosecutions (Roht-Arriaza, 2005, 10-16).

After migrants arrive in a receiving state, they have diverse ways of mobilizing for transnational justice. The first (and most direct) path to transnational justice is to report crimes to police and prosecutors in the receiving state and assist in investigations (Langer, 2015*a*). Migrants have specialized knowledge that is needed for a successful investigation, arrest, and trial. For example, Argentine and Chilean migrants helped to define the legal strategy for the Spanish cases, provided evidence, testified as witnesses, and helped Spanish authorities to collect testimony from individuals still living in Argentina and Chile (Roht-Arriaza, 2005, 8-31). For instance, two Argentine exiles in Spain, economist Gregorio Adonis and labor lawyer Carlos Slepoy, proposed different strategies and paths for the Argentine investigation in Spain and worked as part of the team that was put in place to collect evidence for the case (Roht-Arriaza, 2005, 8-10).

In some states, this process of reporting crimes and assisting in legal proceedings is an explicit part of the immigration process for new arrivals. For instance, in Germany, a Syrian immigrant was tracked down by authorities through statements made by another Syrian immigrant as part of an asylum application and convicted in 2020 for war crimes and terrorism committed in Syria against Syrians in 2013.<sup>5</sup> In Finland, two Iraqi twins were tried for war crimes and terrorism, and ultimately acquitted, in a case that started on the basis of information they submitted in their own asylum application (Langer and Eason, 2019). In Norway, the police looked for 20 suspected war criminals from Syria, following tips from refugees and local immigration authorities.<sup>6</sup>

A second tactic is for migrants to mobilize public opinion in the receiving state. By informing the public about crimes that occurred elsewhere, migrants can gain valuable allies, particularly for their interactions with elected officials. For example, after and during the Rwandan genocide of

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<sup>5</sup> "Höchststrafe nach Massaker auf syrischer Müllkippe" *Stuttgarter Nachrichten*, January 13, 2020. Available at <https://www.stuttgarter-nachrichten.de/inhalt.terror-prozess-in-stuttgart-hoehchststrafe-nach-massaker-auf-syrischer-muellkippe.7453651e-8032-4b06-9d21-41c9f2c7d6b2.html>.

<sup>6</sup> Stine Jacobsen, "Norway police search for Syrian war criminals among asylum seekers" *Reuters*, January 15, 2016. Available at: <https://www.reuters.com/article/uk-europe-migrants-warcrimes-norway/norway-police-search-for-syrian-war-criminals-among-asylum-seekers-idUKKCNOUT1FG>.

1994, many Rwandan migrants—including victims, witnesses, and perpetrators—fled to Belgium. In July 1994, numerous Rwandan victims filed complaints with the Belgium police about crimes committed by perpetrators who had also migrated to Belgium. When Belgian authorities were reluctant to get involved, migrants and their attorneys organized protests and filed legal complaints against prominent Belgian politicians. These events were widely reported in Belgian newspapers, raising public awareness and creating pressure on the government to appoint an examining magistrate (Human Rights Watch, 1999).

Third, migrants also frequently meet directly with elite officials—including judges and legislators—to persuade them to act. For example, fearing that the public prosecutor was inclined to oppose the Argentine case in Spain, two people in exile in Spain, including the grandmother of a baby who had been kidnapped by the military, met with the Spanish chief prosecutor at the time and persuaded him to remain agnostic about the legal merits of the case (Roht-Arriaza, 2005, 14-15). Similarly, growing public concern over the Rwandan genocide led Belgium legislators to amend their domestic laws in 1999 to allow Belgian courts to have universal jurisdiction over genocide and crimes against humanity (Keller, 2001). This legal change facilitated the Belgian trial and conviction of four Rwandan citizens—including two nuns, a government official, and a university professor—for complicity in the Rwandan genocide.<sup>7</sup>

Finally, many states provide a fourth tactic for transnational justice: private prosecutions. This power is mostly unknown in the United States, where public prosecutors are the only ones that may file charges and are the only prosecuting party in criminal proceedings. However, many other countries allow private actors—including the alleged victim of a crime, a private citizen, certain NGOs, or other organizations—to be a party in the criminal process together with or instead of public prosecutors (Langer and Sklansky, 2017, 333). The powers of private prosecutors vary from place to place. But they may include the power to file criminal charges, sue for civil damages in the criminal case, plead before the court, present evidence, interrogate witnesses, appoint and present their own expert witnesses, appeal, and move the case forward toward trial. Some scholars have highlighted how private prosecutors push forward domestic human rights cases (Brinks, 2008; Michel and Sikkink, 2013) and transnational prosecutions of international crimes (Langer, 2011). Others have underlined the link between private prosecution and migration (Mégret, 2015).

Private prosecutions have been important in many transnational cases by giving migrants a way to file these cases and move them forward even when prosecutors and judges are reluctant to

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<sup>7</sup> “Nuns jailed for genocide role” *BBC News (UK)*, June 8, 2001. Available at: <http://news.bbc.co.uk/2/hi/europe/1376692.stm>.

do so. For instance, the Argentine Human Rights Association of Madrid was a private prosecutor in Spain, ultimately leading to the arrest for August Pinochet (Roht-Arriaza, 2005, 10).<sup>8</sup> In France, *Le Collectif des Parties Civiles pour le Rwanda*—an NGO co-founded by Dafroza Gauthier, a Rwandan immigrant, and her French husband—has filed multiple complaints and become a civil party in criminal proceedings against Rwandans living in France.<sup>9</sup>

Migrants are often assisted in all of these various activities—reporting and assisting, mobilizing public opinion, persuading elite actors, and pursuing private prosecutions—by activists and nongovernmental organizations in the receiving states. For example, organizations like Amnesty International and Human Rights Watch provide important expertise and resources to migrants who seek justice. Yet the migrants themselves are key actors with agency and importance in their own right.

Government officials must respond to migrant actions by making their own decisions about whether to pursue a universal jurisdiction case. As described above and shown in Figure 2, the first step in pursuing a case is the initiation of a case. Cases often begin when an alleged victim files a complaint with local police in the receiving state. In some states, NGOs can bring cases on behalf of a victim, independent of support from prosecutors, and/or government officials have authority to initiate a case. The second step is an investigation by public authorities. Third, is the initiation of formal charges or proceedings by a prosecutor or judge. Fourth is the issuance and execution of an arrest warrant. The final step is a criminal trial of the alleged perpetration. Each of these steps is a distinct and ordered outcome of a universal jurisdiction case. As shown in Figure 2, we believe that each of these steps cumulatively increases the overall amount of transnational justice that a prosecuting state provides to the victims of atrocities.

[Figure 2 goes here.]

Four keys explanatory variables affect the outcomes in our theory. Our first—and most important—explanatory variable is migration. This variable can have both direct and indirect effects on transnational justice. For the direct effect, as more migrants from a sending state arrive in the receiving state, there are more individuals who are able and willing to advocate for transnational justice. As more migrants take these costly actions, government officials will receive more benefit from taking the various steps in a universal jurisdiction case.

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<sup>8</sup>The Argentine and Chilean investigations were closely intertwined, with both investigating crimes by Pinochet.

<sup>9</sup>See: <http://www.collectifpartiescivilesrwanda.fr/>



Migration can also have an indirect effect on government officials by reducing the cost of a universal jurisdiction case.<sup>10</sup> All else equal, more victims and witnesses can generate more criminal charges against more perpetrators and provide better and more compelling evidence for trials. Additionally, government officials will find it easier to secure and execute arrest warrants when more perpetrators are present on their territory.

Careful readers will notice that we have not made any specific assumptions about whether transnational justice provides private or public benefits for victims. We also have not explicitly described any strategic interactions amongst migrants about who exactly should invest their time and effort in securing transnational justice. Perhaps activism is hindered if successful cases provide public benefits, rather than private benefits. 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, 2019*a*). To ensure that our account of transnational justice is not derailed by a collective action problem, we constructed numerous formal models of these strategic processes.<sup>11</sup> These formal models show that our explanatory variables clearly and consistently shape outcomes across a host of different assumptions about the nature of private versus public benefits. Overall, political pressure by migrants generates our first empirical hypothesis:

Hypothesis 1 (H1): A larger mass of migrants from the sending to the receiving state will yield more transnational justice.

Our second explanatory variable is the level of atrocities in the sending state. Once again, this variable can have both direct and indirect effects. When atrocities are larger in number and/or more severe, more migrants are likely to be more motivated to demand transnational justice. We also expect that more numerous and severe atrocities will make government officials more sympathetic to the demands of migrants. Activists and NGOs are well aware of this effect, and often ask victims and witnesses to share their experiences privately with government officials in order to persuade them to act (Roht-Arriaza, 2005). Atrocities also have an indirect effect on transnational justice by increasing migration from the sending state, thereby reinforcing the effect in H1.<sup>12</sup> Overall, we expect that:

Hypothesis 2 (H2): A higher magnitude of atrocities in the sending state will yield

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<sup>10</sup>We thank Francesca Parente for suggesting this point.

<sup>11</sup>These models are available to readers in an Online Appendix.

<sup>12</sup>We thank Wayne Sandholtz for this point.

more transnational justice.

Our third explanatory variable is the responsiveness of the government in the receiving state. This concept includes a government's views on human rights and internationalism, which affect its willingness to prosecute universal jurisdiction cases. It also includes a government's overall responsiveness to public pressure of any kind. Responsiveness can therefore be measured by the government-specific measures of party ideology and state-specific measures of government accountability to the public. We expect that more responsive governments are more likely to provide justice, which should make migrants more likely to demand it. These reinforcing effects yield our third hypotheses:

Hypothesis 3 (H3): A higher level of responsiveness in the receiving state will yield more transnational justice.

Finally, we expect that prosecution costs will influence government officials. Many different factors can influence this cost. Poorer states are likely to have less capacity and fewer resources to investigate and prosecute crimes. Similarly, prosecutors are likely to find it more difficult to pursue a state's first universal jurisdiction because such cases require them to craft novel legal arguments before skeptical judges. We believe that migrants who anticipate this concern are likely to invest less time and effort in their efforts to secure justice, thereby lowering the likelihood of justice even further. This yields our final hypothesis:

Hypothesis 4 (H4): A higher prosecution cost in the receiving state will yield less transnational justice.

## 4 Data and Methods

We now turn to our statistical tests of how migration, atrocities, 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) and again by Langer in 2020-2021. 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. First, in our most expansive dataset, we assign these cases to the major perpetrator country, thus assigning Nazi cases to Germany, Yugoslav cases to Serbia, and ISIS cases to Syria. We call this the “All” dataset in our tables. Second, in our most conservative dataset, we drop all instances in which we do not know the subject nationality. We call this the “No J. Doe” dataset in our tables. All of our models are estimated separately on both datasets to ensure that our results are robust.

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 two different ways. First, the variable ANY INITIATION measures whether the receiving state considered any complaint against defendants from the sending state in a given year.

Second, the variable FIRST INITIATION RS examines the first time the receiving state considers a complaint under UJ. The data is still the same dyadic data as in the case of ANY INITIATION; we simply drop all observations after the year in which the first case is considered. This test helps relieve some concerns about reverse causation: given that this is the first initiation, it could not be the case that migrants are moving because they know the state has a history of UJ cases.<sup>13</sup>

We also examine later stages of the process. INVESTIGATION captures whether the state entity takes investigative measures regarding the case or inquires whether other jurisdictions are investigating or prosecuting or have investigated or prosecuted the case. FORMAL PROCEEDINGS

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<sup>13</sup>It could still be the case that migrants think the likelihood of a UJ case is larger in some countries due to an unobservable. We examine this below using sensitivity analysis.

refers to a person being arrested or indicted, or that an information or other formal charge was issued against the person, or that an investigating judge or equivalent prosecutor moved proceeding forward against the person. ARREST refers to an arrest warrant being issued against the defendant or that authorities otherwise tried to arrest the defendant or that an arrest actually took place. Finally, we examine whether there was a TRIAL held. For each of these stages, we examine whether there are ANY of these escalations in the dyad-year and, similar to above, the FIRST (STEP) RS, which is the first time that the receiving state takes one of these actions.

For the steps to trial, especially as we get to later stages of the process—like arrests and a trial—we acknowledge that there is more chance involved in whether those stages happen. The perpetrator’s identity may be unknown or he may be impossible to locate or extradite, making arrest and a trial difficult, if not impossible. Thus, while we expect that our hypotheses will hold at these later stages, the magnitude of the effects may decrease in size.

## **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).<sup>14</sup> 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.<sup>15</sup>

### *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 the variable PTS (SS), the political terror score for the sending state (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

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<sup>14</sup>We log all migration data due to the right-skew of the data.

<sup>15</sup>In our robustness analysis, which is discussed below, we use alternative measures of migration.

results are robust to dropping this variable.

Second, we include a measure of the sending state's regime type. Because the PTS variable is based on State Department reports, there may be undercounting of human rights violations, since states may try to hide these violations. We think that human rights violations are both more likely in autocracies and less likely to be reported, due to the lack of a free press. We use the coding of regime type from Polity (Marshall, Gurr and Jagers, 2016). The variable DEMOCRACY (SS) has higher values when the sending state is more democratic. We expect that higher values of this variable will correspond to fewer atrocities in the sending state and that higher DEMOCRACY (SS) will decrease the likelihood of a prosecution.

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 to 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 10 years.<sup>16</sup>

### *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. We examine two types of responsiveness: political responsiveness and legal responsiveness. Our first measure of political responsiveness is the recipient state's regime type using Polity (Marshall, Gurr and Jagers, 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.<sup>17</sup> 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 partisanship of

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<sup>16</sup>We have used different lag structures as well as the maximum and minimum and found similar results. See Appendix figures A5.

<sup>17</sup>Population is logged due to right-skew of the data.

the executive because prosecutions often fall under the executive branch of government. The variables on partisanship 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.

Our final measure of political responsiveness is 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.<sup>18</sup> States that are members of 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 signals that a state is willing, at least in the abstract, to be responsive to these cases. Ratifying this treaty, then, 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.<sup>19</sup> We expect by H3 that ROME STATUTE will increase universal jurisdiction cases.

We include two measures of legal responsiveness: PRIVATE PROSECUTIONS and BRITISH LEGAL ORIGIN. PRIVATE PROSECUTIONS, coded by Michel and Sikkink (2013), is a measure of whether an individual or organization can be a party in the criminal process and, as such, open an investigation and formal proceedings, plead before the judge, request the production of evidence, interrogate witnesses and appoint its own expert witnesses, appeal judicial decisions, etc. States that have private prosecution should be more responsive to UJ cases because alleged victims and NGOs have more legal tools to press charges and to move these proceedings forward toward trial (Langer, 2011). In contrast, we would expect British Legal Origin to be associated with fewer universal jurisdiction cases because in British Legal Origin systems the decision on whether a case should be opened or move forward to trial is typically exclusively made by public prosecutors that may be affiliated with the Executive Branch and thus be more responsive to the costs that UJ prosecutions may entail for the prosecuting state (Langer, 2011).

### *Cost*

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<sup>18</sup>Our robustness checks also examine the impact of being a signatory of the Rome Statute.

<sup>19</sup>Ratification may also indicate government responsiveness and made a commitment to advance international justice, as it shows that at least some past government thought that respecting human rights is important.

Our final explanatory concept is the cost of prosecutions for the receiving state. First, we examine whether the receiving state's wealth affects prosecutions. We expect that the variable GDP<sub>PC</sub> (RS) (logged; from World 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 GDP<sub>PC</sub> (RS) will increase cases.

Next, 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 initiation or successful prosecution of a universal jurisdiction case. The variable PRIOR INITIATION (RS) is coded as a 1 for all years after the first initiation of a case from any country in a receiving state. The prior initiation could lead to learning and make the next case easier. 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 INITIATION (RS) and PRIOR SUCCESS (RS) will increase cases.<sup>20</sup>

Finally, we include some dyadic measures that may make the receiving state less likely to want to prosecute someone from the sending state or give the sending state the power to use diplomatic measures to stop a prosecution (Langer, 2011). We include two measures of economic power: the sending state's wealth, which is the variable GDP<sub>PC</sub> (SS) (logged; from World Bank, 2015) and TRADE, which is the percent that dyadic trade makes up of the total trade of the receiving state. We also include measures of military power: an indicator for ALLIANCE between the receiving and sending states (Correlates of War Project, 2013) and major power status with an indicator variable for MAJOR POWER (RS) and MAJOR POWER (SS) states, which equals 1 if the receiving or sending state, respectively, is one of the permanent 5 members of the UN Security Council or, after 1991, Germany or Japan. Finally, we also include an indicator variable for SHARED LANGUAGE between the sending and receiving state, as this likely reduces the cost of

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<sup>20</sup>Note that both of these variables are dropped when we examine the first initiation and PRIOR SUCCESS (RS) is dropped when we examine the first trial.

collecting evidence and interviewing witnesses (Melitz and Toubal, 2014)

### *Control Variables*

Finally, we include numerous control variables in our analysis. We include a measure of how strong the NGO network is in the receiving state. Larger, more vibrant NGO networks should make it easier for migrants to connect with NGOs that can help them bring their case to the government. The variable `NGOs (RS)` measures the number of NGOs that are headquartered in the receiving state coded from Smith, Wiest and Hughes (2020). 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 tribunal.<sup>21</sup> 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.<sup>22</sup> We also include a measure of whether there has been an amnesty in the sending state in the past. The variable `AMNESTY (SS)`, coded from Bell and Badanjak (2019), takes a value 1 if there has been an amnesty and zero otherwise. Migrants might be more likely to seek justice in another state if they cannot get justice at home due to an amnesty.

Finally, as dyadic-level controls, we include `FORMER COLONY (DYAD)`, 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. `FORMER COLONY (ANY)` measures if the sending state was a colony in any empire, to understand if former colonies are more likely to have UJ prosecutions against people from their state. We also include `DISTANCE` from Gleditsch and Ward (2001) and `SHARED BORDERS` from Correlates 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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<sup>21</sup>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.

<sup>22</sup>See Schabas (2003) for examples of both possibilities.



## 4.2 Estimation

We test our hypotheses in several different ways. For the all models, 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. For the models on the first case in the receiving state (models 3-4), we capture “onset” of UJ within a dyad by dropping observations for the years after the first case, similar to the conflict literature (Beck, Katz and Tucker, 1998). We also include measures of: years since 1957 (the start of the dataset); years since squared; and years since cubed. These variables correct for temporal dependence between observations in dyads, as suggested by Carter and Signorino (2010). The data in models 3 and 4 are still dyadic data, with all dyadic variables applying to a single sending country, but we drop all dyads-year observations after the first case.

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 of additional explanatory variables, models 1–2 use data from 1976-2007 and models 3-4 use data from 1976-2012. All models were run with robust standard errors clustered by dyad to account for dependence within dyads.

## 5 Results

We begin our results by testing our hypotheses on whether there is any initiation of a case in a given year and on the first initiation of a case in a receiving state. Figure 3 displaces the coefficient on migration from regressions that sequentially add controls. The first coefficient is simply the bivariate correlation between UJ initiations and MIGRANT STOCK; the second includes dyad and year fixed effects;<sup>23</sup> the third removes the dyad and year fixed effects to allow us to add controls beginning with our variables measuring atrocities in the sending state; and subsequent models add in our variables for responsiveness, costs, and additional controls. What we see is that while the coefficient changes in size some—as we would expect as we are including variables that are correlated with both migration and UJ cases—it is always highly statistically significant.

[Insert Figure 3 here.]

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<sup>23</sup>Estimated with GLS rather than OLS for efficiency.

Figure 4 displays the marginal effect of migration on whether there is an initiation in a dyad-year and on the first initiation in the receiving state from the regression models that include all the controls and the John Doe cases. Universal jurisdiction cases are quite rare: between 1960 and 2007 (where we have full data coverage) there are only 84 dyad-years or about 0.05% of cases in which there is at least on UJ case and only 0.01% of cases in which the receiving state first considers a UJ case. As we can see in the graphs, migration has a substantively important effect: going from the 25th percentile (no immigrants form the sending country to the receiving country) to the 75th percentile (about 300 immigrants) leads to an increases the probability of any case from statistically 0 (95% confidence interval of -.0002 to .0004) to 0.0008 (95% confidence interval of .0006 to .001) and for first case in the receiving state leads to an increase in probability from 0 (95% confidence interval of  $-0.0002$  to  $0.00003$ ) to 0.0002 (confidence interval of  $0.00005$  to  $0.0004$ ).

[Insert Figure 4 here.]

Table 3 shows the results of the regressions from the models that include all the variables and allows us to examine our additional hypotheses. We find support that the level of atrocities in the sending state affects the likelihood of a UJ case (H2). Sending state that have been less democratic or that have engaged in more political terror in the last 10 years are more likely to have any UJ cases against citizens from their state. While the coefficients on PTS (SS) and DEMOCRACY (SS) are not always statistically significant at conventional levels in the regressions on the first instance of an initiation, they do have the hypothesized sign.

[Insert Table 3 here.]

Next, we find some support for the responsiveness hypothesis. In contrast to H3, while the vast majority of cases have occurred in democracies, it is the case that more democratic countries, as measured by polity, are less likely to initiate a UJ case once we control for other factors.<sup>24</sup> Similarly, left and right governments are about equally likely to pursue these cases. In support of H3, we find smaller countries are more likely to have any case, have ratified the Rome Statute, and those with more responsive legal systems, in terms of the ability to pursue private prosecutions and non-British legal origin, are more likely to have a case in a given year. Yet, none of these variables help explain the first case.

We also find support for the idea that the cost of pursuing these cases matters, although not necessarily for the first initiation of a case. Wealthier and more developed countries are more

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<sup>24</sup>We have also used the Boix, Miller and Rosato (2013) dichotomous democracy measure and find similar results.

likely to have a case. Having had a prior case seems to provide some type of learning, leading to more cases later on.

We also find that cost within the dyad matters. Most importantly, receiving states are unwilling to go after a defendant from a wealthy country (higher GDPPC (SS)) or a MAJOR POWER (SS). The trading relationship between the receiving state and sending state, the alliance relationship, and the major power status of the receiving state seem to have no effect. Finally, a SHARED LANGUAGE increases the likelihood of a case initiation, likely because it is easier to gather evidence in the same language.

In terms of control variables, we find some surprising results. First, the larger numbers of NGOs headquartered in the receiving state increases the likelihood of having any initiation but not the first initiation. Second, having an AMNESTY law passed in the sending state seems to lower the likelihood of having a UJ case. Other variables that have been hypothesized to have an effect seem to have no effect: INTERNATIONAL PROSECUTIONS (SS) do not affect UJ cases; former colonies of the receiving state are not more likely to have a case brought against one of their citizens and former colonies in general are less likely to have a case brought against one of their citizens; sending states from father away are more likely to see a case brought against one of their citizens; and there is no difference in the likelihood of a case during the Cold War and the War on Terror from the 1990s.

## 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 5. First, 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), 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 5 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 ECONOMIC MIGRANT STOCK, which we calculate as the migrant stock minus the refugee stock (lasso).

Figure 5 replicates models 1 and 3, respectively, replacing the migration variable with one of these alternatives and plots the coefficient on migration variable. The figure plots the coefficient on different measures of migration from separate regressions. Dots represent the point estimates and the thick (thin) bars represent the 95% (90%) confidence interval. The dotted vertical line is at 0; coefficients whose confidence interval does not cross the 0-line are statistically significant at conventional levels. We include the coefficient from Table 3 for reference (top coefficient). As we can see from the figure, regardless of which measure of migration we use, the effect of migration is positive and significant at conventional levels for ANY INITIATION and FIRST INITIATION (RS).

We also examine alternative measures of atrocities. Our main analysis used 10-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 20 years and looking at the minimum and the maximum value over those same time periods. We find highly consistent results across all these different measures, with almost all coefficients in the hypothesized direction and significant at conventional levels (see Appendix Figure A5), consistent with support for H2, and migrant stocks are still positive and statistically significant across all these models.

We finally examine alternative measures of universal jurisdiction cases. Recall that our main analysis focuses on initiations. But cases proceed from initiations through investigations and formal proceedings all the way to trials. We replicate models 1 and 3 from Table 3 but change the case for one of the additional dependent variables. We also replicate the models with refugee stocks instead of migrant stocks, to see whether having legally determined refugees, who by definition fled persecution and very likely fled a crime against humanity, help explain further steps towards trial.

Figure 6 displays the coefficients on migration for each of these variables.<sup>25</sup> The figure plots the coefficient `MIGRANT STOCK` and `REFUGEE STOCK`, respectively, from each regression on the different dependent variables. Dots represent the point estimates and the thicker bars represent the 90% confidence interval and thinner bars represent the 95% confidence interval. The dotted vertical line is at 0; coefficients whose confidence interval does not cross the 0-line are statistically significant at conventional levels.

[Insert Figure 6 here.]

We find very similar results on migration. In most cases the coefficient on migration was positive, if not always significant. Interestingly, migration, as measured by `MIGRANT STOCK` or `REFUGEE STOCK`, better explains the first instance of each step to trial, except investigations, than any instance of these steps. This suggest that migration is important for that first case but that other factors are at play in later instances.

Finally, we examine whether the effects of migration are driven by the imputation of migration data. We regress an indicator of whether there were any initiations in the decade on the values from the start of the decade. For example, `DV` takes a value 1 if there was any initiation from 1970 through 1979. We then regress that on the level of migration and the rest of the variables from 1970 and drop all the years in between. Table A5 and Figure A6 present the results of dropping all data except the decade mark. We find highly consistent results.

## 5.2 Sensitivity Analysis

We have shown, thus, that migration robustly predicts universal jurisdiction cases and the steps to and through trial. We have also shown that migration robustly predicts the first time that a receiving state uses UJ. This suggests that there is unlikely to be reverse causation, unless the migrants know of some unobservable characteristic of the state that will lead it to use UJ. While we have controlled for explanations in the literature, we now turn to sensitivity analysis to help understand what additional threats to inference there may be.

To examine the sensitivity of `MIGRANT STOCK`, we examine how large an unobserved confounder would need to be to change our results using the procedure in Cinelli and Hazlett (2020). Assume that there is an unobserved confounder such that our estimate of the effect of `MIGRANT STOCK` is greater than 0 but the true effect is 0. In this case, our estimated coefficient

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<sup>25</sup>See Tables A1-A4 for full regression results.

would be biased. Cinelli and Hazlett (2020) show that this bias can be calculated from the amount of variation ( $R^2$ ) that the confounder explains in both the outcome and explanatory variables. Their procedure then allows us to calculate how much variation this confounder must explain such that the bias in our estimate is large enough that the true estimate is 0. It also allows us to examine how large a hypothetical confounder would need to be in comparison to other variables.

We examine the sensitivity in the following way. For ANY INITIATION and FIRST INITIATION (RS), we examine the sensitivity of the estimate in Model 1 and 3, respectively, from Table 3, using the DEMOCRACY (SS) as our benchmark against which to judge a potential confounder.<sup>26</sup> We use DEMOCRACY (SS) as it has a large effect on both UJ initiations and MIGRANT STOCK.<sup>27</sup>

[Insert Figure 7 here.]

We compare the effect of a hypothetical confounder to our benchmark of DEMOCRACY (SS) in figure 7.<sup>28</sup> The red line shows when the point estimate on MIGRANT STOCK would equal zero. A confounder would have to have as large an effect as that of three times DEMOCRACY (SS) for ANY INITIATION and four times DEMOCRACY (SS) for FIRST INITIATION (RS) to decrease the size of the coefficient on MIGRANT STOCK to zero. A hypothetical confounder that had 3 times the effect of DEMOCRACY (SS) would have to explain about 25% of the remaining variation in MIGRANT STOCK, but all the variables already explain 63% of the variation. Thus, an unobserved confounder would have to have quite a large effect on *both* UJ cases and migration to reduce the effect of migration to zero. It is unlikely that such a confounder exists.

## 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.

<sup>26</sup>We estimate the sensitivity parameters using `sensemakr` (Cinelli, Ferwerda and Hazlett, 2020) in stata.

<sup>27</sup>Using standardized coefficients, DEMOCRACY (SS) has the second largest effect on the outcome, after MIGRANT STOCK. It also has a large effect on MIGRANT STOCK, when MIGRANT STOCK is regressed on DEMOCRACY (SS) and other controls.

<sup>28</sup>Appendix Table 7 displays the full results.

Our argument and evidence thus highlight the role of human rights victims as protagonists in securing transnational justice. While prior scholarship has amply and importantly demonstrated the importance of transnational activism and NGOs, much less attention has been paid to the agency of the individual victims. These victims have agency, both collectively and individually. They can complain to public authorities, provide eyewitness testimony, give prosecutors specialized knowledge, influence public opinion, persuade judges, and lobby legislators. In many states, they can even launch their own private prosecutions against alleged criminals (Langer, 2011; Michel and Sikkink, 2013). Migrants can change outcomes in receiving states.

Additionally, the impact of migration on transnational justice challenges the claim that transnational justice is a manifestation of colonial attitudes (Jalloh, 2010). Past colonial relationships can affect contemporary migration through factors like shared language and diaspora communities. However, once we control for migration patterns, there is no statistical evidence that colonial status increases transnational justice. Prior colonizers are *not* more likely to initiate cases against defendants from their prior colonies. Indeed, states in general are *less* likely to initiate cases against defendants from any prior colony.

Finally, 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, 2015*b*). 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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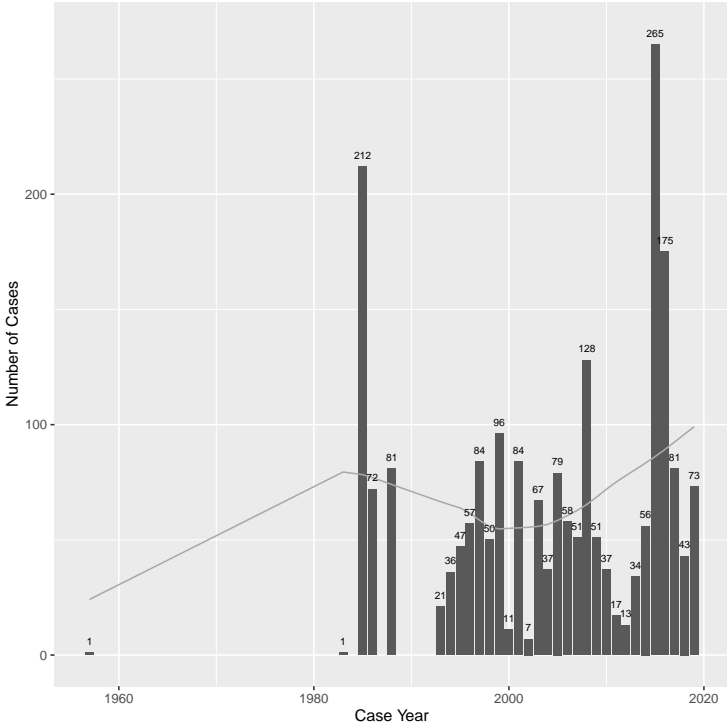
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Figure 1: Universal Jurisdiction Initiations, 1957–2017



Note: This figure shows the distribution of INITIATIONS, with a loess line to depict the general trend over time.

Figure 2: Steps in a Universal Jurisdiction Case

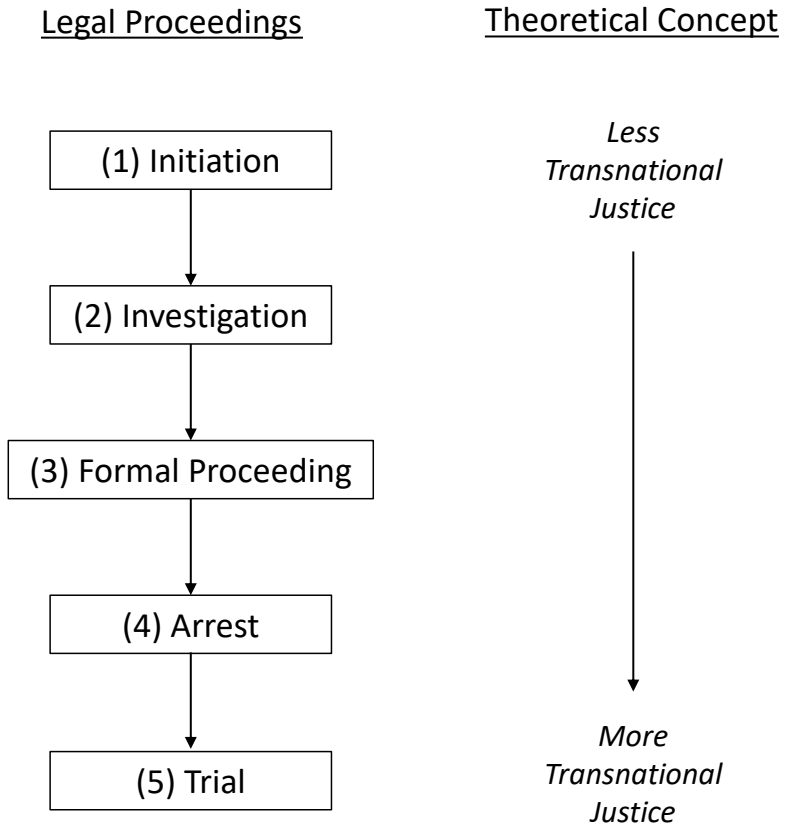
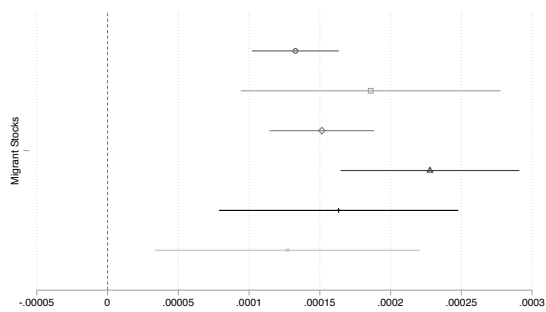
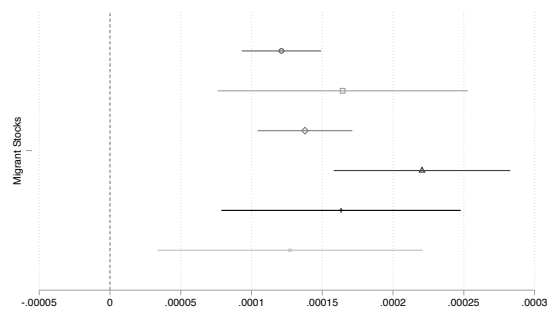


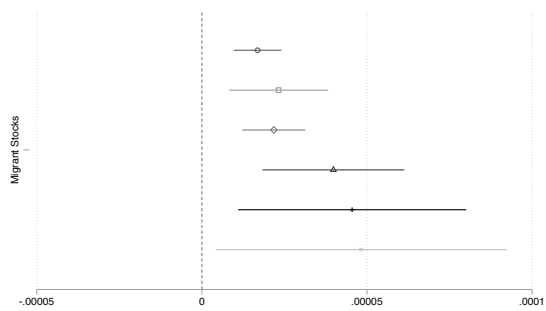
Figure 3: The effect of migration on initiation of UJ cases sequentially adding additional controls



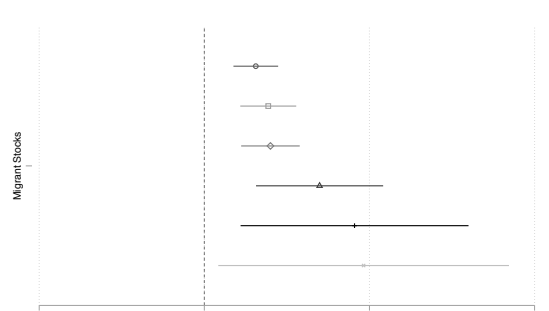
(a) ANY INITIATION INCLUDING JOE DOE CASES



(b) ANY INITIATION EXCLUDING JOE DOE CASES



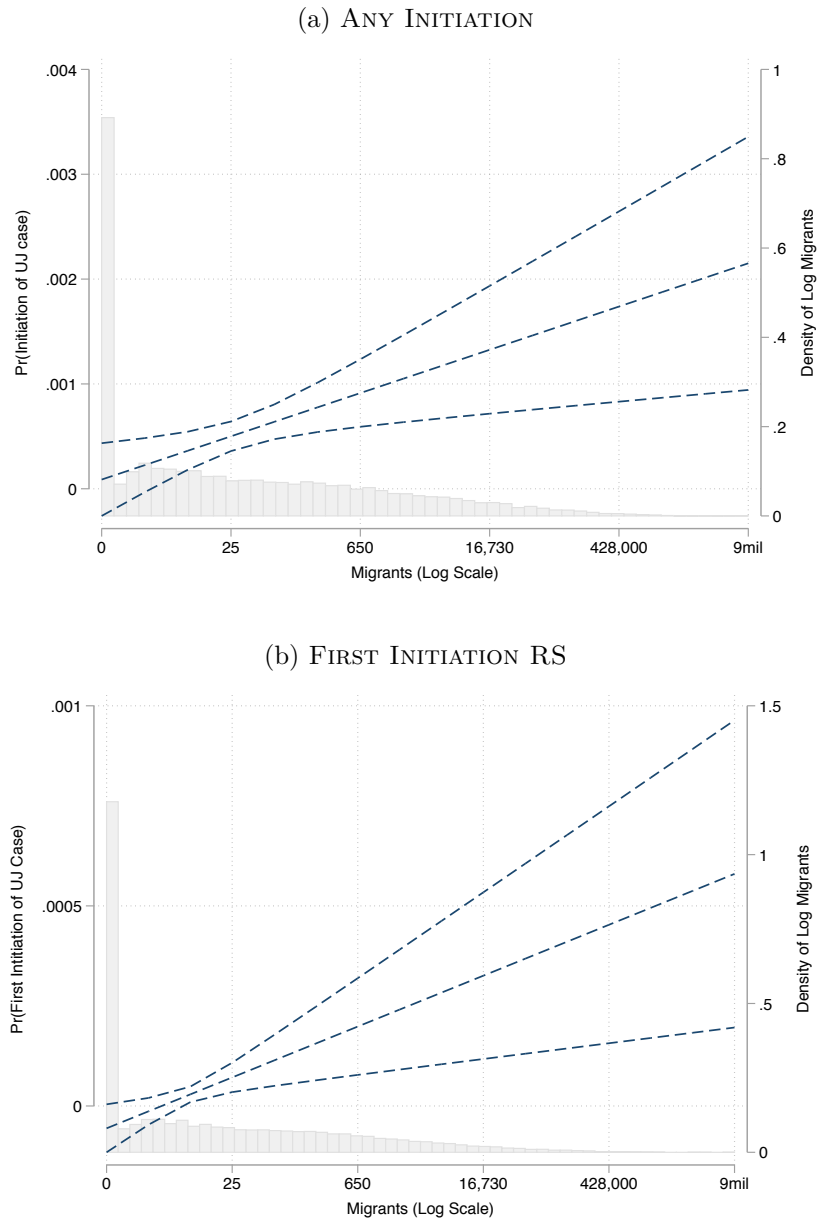
(c) FIRST INITIATION INCLUDING JOE DOE CASES



(d) FIRST INITIATION EXCLUDING JOE DOE CASES

This figure plots the coefficient on `MIGRANT STOCKS` from separate regressions. Each regresses any or first initiation on migration but sequentially adds in additional variables; all regressions with the first initiation drop all dyad-year observation after the first ignition and include years since 1957, its square, and cube. Dots represent the point estimates and thicker bars represent the 95% confidence interval. The dotted vertical line is at 0; coefficients whose confidence interval does not cross the 0-line are statistically significant at conventional levels.

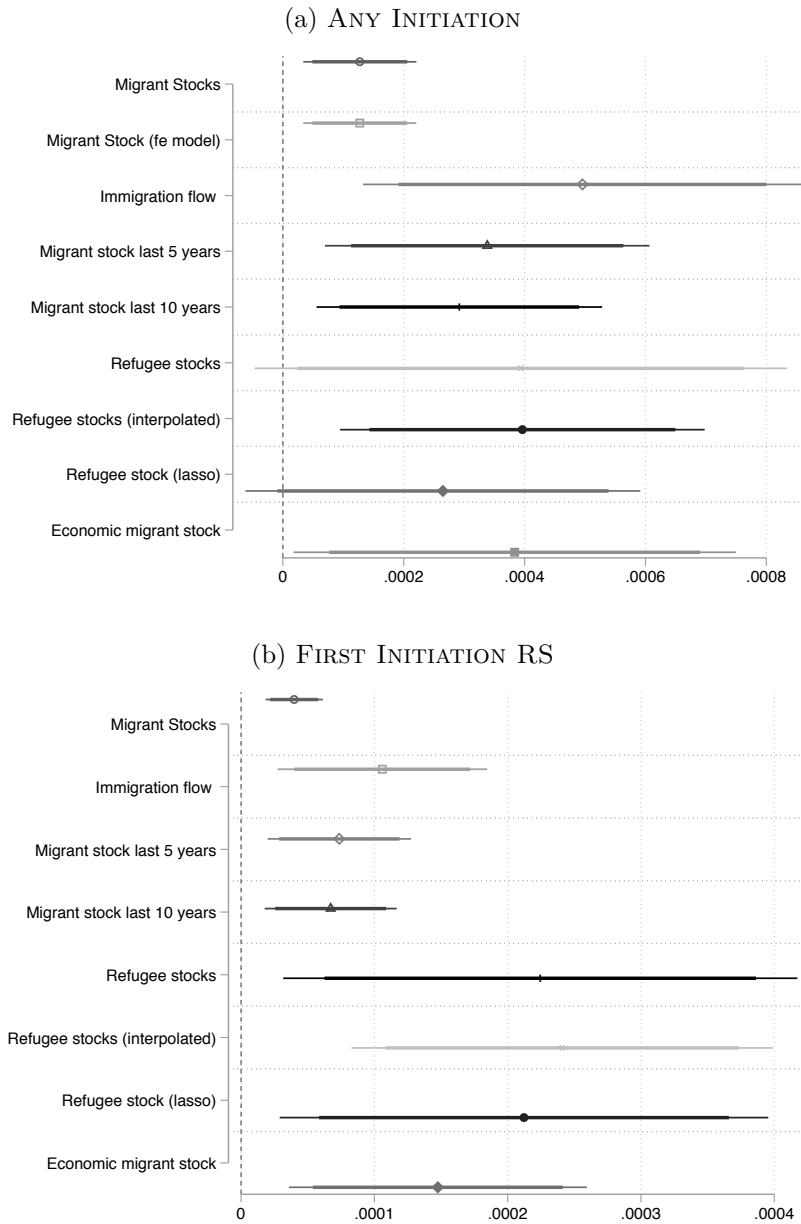
Figure 4: Marginal Effect of Migration on ANY INITIATION and FIRST INITIATION RS



This figure shows the marginal effect of migration, with 95% confidence intervals, on whether there is any case in the dyad and the first case in a receiving state and the distribution of the MIGRANT STOCK variable. Marginal effects for ANY INITIATION and FIRST INITIATION RS are from Models 1 and 3 of Table 3, respectively. Migration stocks levels have been transformed from logs for clarity.

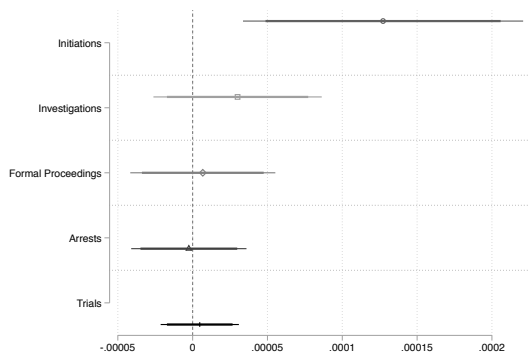


Figure 5: The effect of alternative measures of migration on ANY INITIATION and FIRST INITIATION RS

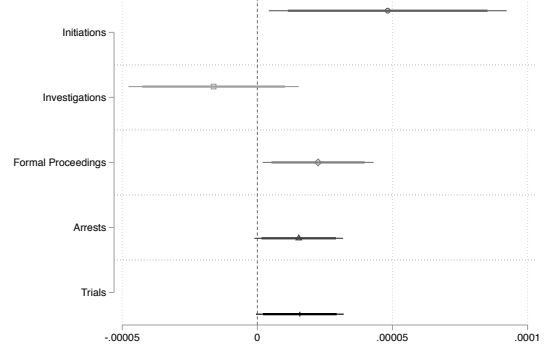


This figure plots the coefficient on different measures of migration from separate regressions. Each regression replicates model 1 or 3, respectively, of Table 3 but replaces migrant stock with another measure. Dots represent the point estimates and thicker bars represent the 90% confidence interval and thinner bars represent the 95%. The dotted vertical line is at 0; coefficients whose confidence interval does not cross the 0-line are statistically significant at conventional levels.

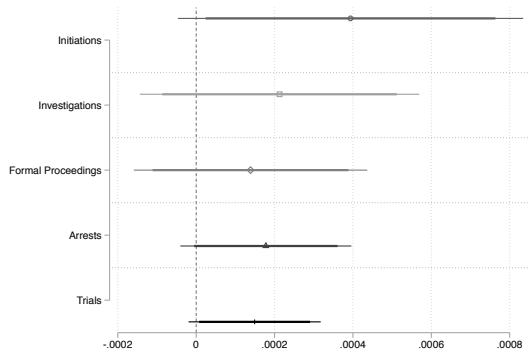
Figure 6: The effect of migration on alternative dependent variables



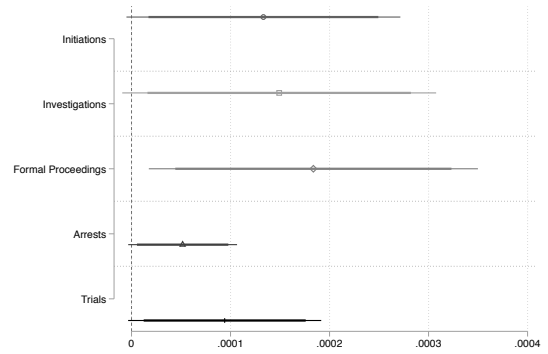
(a) ANY ACTION WITH MIGRANT STOCK



(b) FIRST ACTION IN RS WITH MIGRANT STOCK



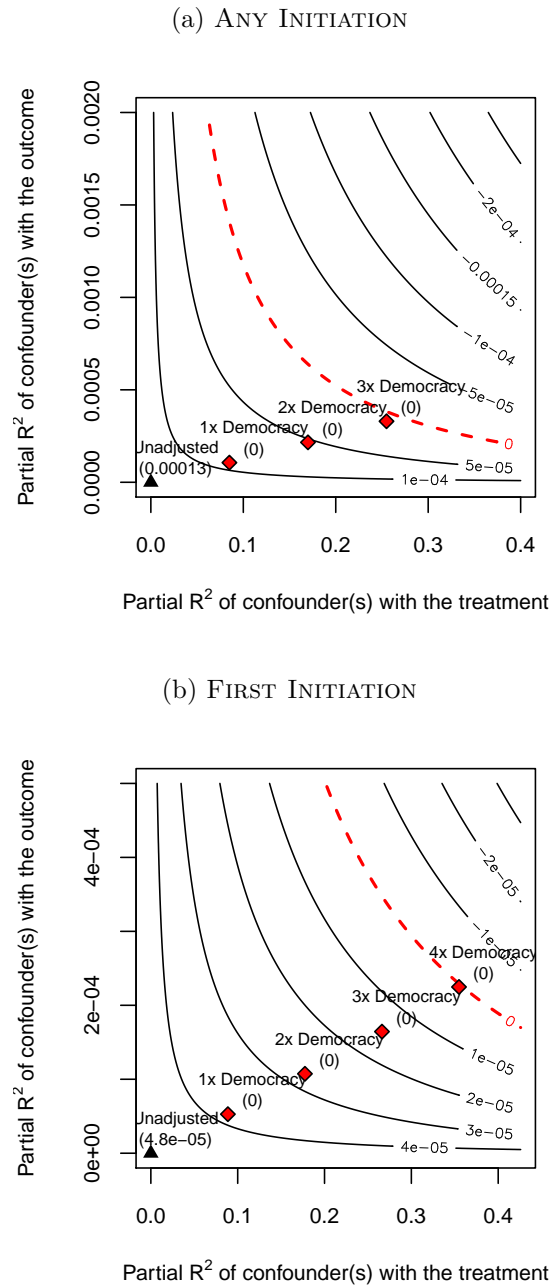
(c) ANY ACTION WITH REFUGEE STOCK



(d) FIRST ACTION IN RS WITH REFUGEE STOCK

This figure plots the coefficient on MIGRANT STOCK and REFUGEE STOCK from separate regressions. Each regression replicates model 1 or 3, respectively, of Table 3 but replaces INITIATIONS with another step towards trial or TRIAL. Dots represent the point estimates and thicker bars represent the 90% confidence interval and thinner bars represent the 95%. The dotted vertical line is at 0; coefficients whose confidence interval does not cross the 0-line are statistically significant at conventional levels.

Figure 7: Sensitivity of the effect of migration to confounders, benchmarked against democracy in the sending state



This figure plots the coefficient on MIGRANT STOCK from a regression of ANY INITIATION (Table 3 model 1) and FIRST INITIATION (RS) (Table 3 model 3) given the inclusion of a hypothetical confounder that is one to three or four times as strong as DEMOCRACY (SS), respectively. The x- and y-axes are expressed in the partial  $R^2$  of the treatment (MIGRANT STOCK) and the outcome (ANY INITIATION or FIRST INITIATION (RS)) and the red line is when the point estimate would be equal to zero

Table 1: Universal Jurisdiction Initiations Across Prosecuting States

Prosecuting State	First Initiation	Total Initiations	First Trial	Total Trials
Germany	1993	653	1997	19
Spain	1996	362	2005	1
Canada	1985	219	1989	3
France	1993	188	2005	5
Sweden	1995	133	2010	10
United Kingdom	1988	104	1999	3
Argentina	2005	96		0
Belgium	1994	85	2001	9
Australia	1986	72	1993	1
Norway	2005	51	2008	2
Switzerland	1995	36	1999	1
Austria	1994	30	1994	3
Turkey	2009	23		0
Netherlands	1994	19	2004	7
Denmark	1994	9	1994	1
Finland	2003	7	2009	6
South Africa	2008	7		0
Chile	2016	3		0
Greece	2004	3		0
Israel	1957	3	1961	2
Italy	2019	3		0
Senegal	2000	3	2015	1
Iceland	2003	2		0
South Korea	2003	2		0
Armenia	2003	1		0
Brazil	2017	1		0
Colombia	2017	1		0
Cyprus	2003	1		0
Hungary	2019	1	2019	1
Ireland	2004	1		0
Luxembourg	1998	1		0
New Zealand	2006	1		0
Poland	2004	1		0
Russia	2004	1		0
Tanzania	2004	1		0
United States	2000	1		0

Table 2: Universal Jurisdiction Initiations Across Defendant Nationalities

Defendant Nationalities	First Initiation	Total Initiations	First Trial	Total Trials
Syria	2013	487	2015	14
Nazi*	1957	371	1961	5
Serbia	1993	257	1994	10
Argentina	1996	142	2005	1
Rwanda	1994	141	1999	25
China	2002	76		0
Israel	2001	76		0
United States	2003	76		0
Spain	2010	67		0
Morocco	1999	66		0
Iraq	1999	51	2016	8
Tunisia	2001	43	2008	1
Turkey	2011	33		0
Mauritania	1999	22	2005	1
El Salvador	2008	20		0
Cuba	1998	19		0
Paraguay	2013	17		0
Chile	1994	15		0
Cameroon	2001	12		0
Uzbekistan	2005	12		0
Democratic Republic of the Congo	1998	9	2004	1
Guatemala	1999	9		0
Libya	2007	8		0
Zimbabwe	2003	8		0
Afghanistan	2000	7	2005	5
Congo	2001	6		0
Algeria	2001	5		0
Liberia	2012	5		0
Sri Lanka	2008	5	2019	1
Chad	2000	4	2015	1
Côte d'Ivoire	2001	4		0
Palestine	2001	4		0
Peru	1998	4		0
Cambodia	1999	3		0
Central African Republic	2001	3		0
Indonesia	2003	3		0
Malta	2019	3		0
Myanmar	2018	3		0
Russia	2005	3		0
Saudi Arabia	2018	3		0
Egypt	2019	2		0
France	2002	2		0
Iran	2000	2		0
United Kingdom	2003	2		0
Venezuela	2003	2		0
Bahrain	2015	1		0
Colombia	2005	1		0
Ethiopia	2009	1	2017	1
Gambia	2017	1		0
Guinea	2019	1		0
India	2003	1		0
Lebanon	2001	1		0
Madagascar	2012	1		0
Nepal	2013	1	2015	1
Nigeria	2014	1		0
Somalia	2005	1		0
Sudan	1997	1		0
Suriname	1996	1		0

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

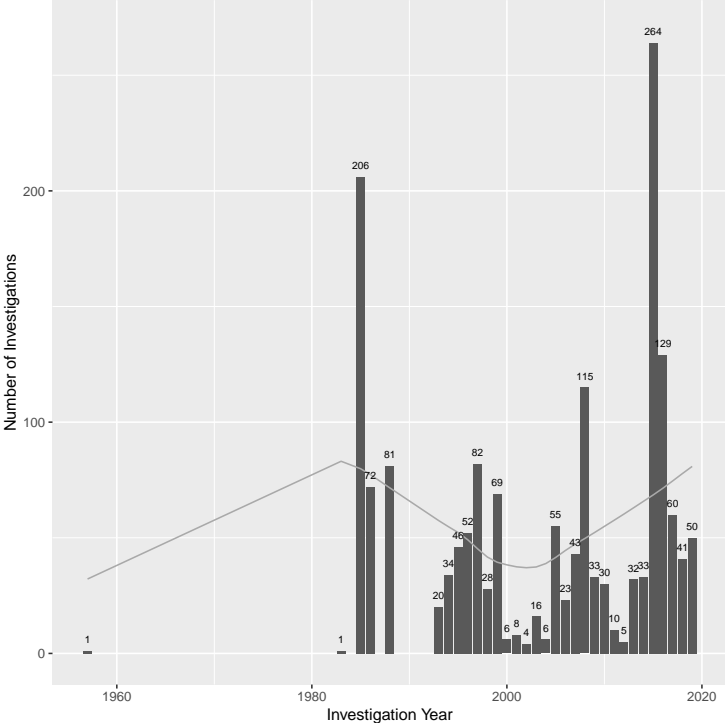
Table 3: Regressions of Cases on Explanatory Variables, 1976-2007/2012

	Any			FirstRS		
	All Model 1	No J. Doe Model 2	All Model 3	All Model 3	No J. Doe Model 4	No J. Doe Model 4
<b>Migration (SS → RS)</b>						
Migrant Stocks	0.00013** (0.000048)	0.00013** (0.000048)	0.000048* (0.000022)	0.000048* (0.000022)	0.000037** (0.000014)	0.000037** (0.000014)
<b>Atrocities (SS)</b>						
PTS (mean, DoS, past 10 years, SS)	0.00040*** (0.000082)	0.00040*** (0.000082)	0.000027 (0.000023)	0.000027 (0.000023)	0.000029* (0.000015)	0.000029* (0.000015)
Democracy (mean past 10 years, SS)	-0.00078*** (0.000021)	-0.00078*** (0.000021)	-0.000020 (0.000011)	-0.000020 (0.000011)	-0.000087 (0.000055)	-0.000087 (0.000055)
<b>Responsiveness (RS)</b>						
Democracy (RS)	-0.00039** (0.00013)	-0.00039** (0.00013)	-0.00014 (0.000099)	-0.00014 (0.000099)	-0.000053 (0.000054)	-0.000053 (0.000054)
Population (RS)	-0.00023*** (0.000065)	-0.00023*** (0.000065)	-0.000034 (0.000030)	-0.000034 (0.000030)	-0.000024 (0.000016)	-0.000024 (0.000016)
Center (RS)	-0.000015 (0.00014)	-0.000015 (0.00014)	0.00010 (0.00010)	0.00010 (0.00010)	0.000038 (0.000056)	0.000038 (0.000056)
Left (RS)	-0.000018 (0.00013)	-0.000018 (0.00013)	-0.0000019 (0.000047)	-0.0000019 (0.000047)	0.000025 (0.000035)	0.000025 (0.000035)
Rome Statute (RS)	0.00069** (0.00022)	0.00069** (0.00022)	0.00016 (0.00010)	0.00016 (0.00010)	0.000033 (0.000060)	0.000033 (0.000060)
Private Prosecutions (RS)	0.00036** (0.00013)	0.00036** (0.00013)	-0.000012 (0.000071)	-0.000012 (0.000071)	-0.000028 (0.000034)	-0.000028 (0.000034)
British Legal Origin (RS)	-0.00063** (0.00019)	-0.00063** (0.00019)	0.000091 (0.000071)	0.000091 (0.000071)	0.000025 (0.000033)	0.000025 (0.000033)
<b>Cost (RS)</b>						
GDPpc (RS)	0.000021 (0.000046)	0.000021 (0.000046)	0.000021 (0.000041)	0.000021 (0.000041)		
OECD (RS)	0.00019 (0.00010)	0.00019 (0.00010)	-0.000041 (0.000070)	-0.000041 (0.000070)		
Prior Initiation (RS)	0.070** (0.025)	0.070** (0.025)				
Prior Success (RS)	0.00011 (0.00012)	0.00011 (0.00012)				
<b>Cost (Dyadic)</b>						
GDPpc (SS)	0.00012** (0.000046)	0.00012** (0.000046)	-0.0000012 (0.000011)	-0.0000012 (0.000011)		
Trade	0.00028 (0.00016)	0.00028 (0.00016)	0.00012 (0.00011)	0.00012 (0.00011)		
Alliance	-0.00069 (0.00036)	-0.00069 (0.00036)	0.00026 (0.00020)	0.00026 (0.00020)		
Major Power (RS)	-0.0016 (0.00098)	-0.0016 (0.00098)	-0.00073 (0.00075)	-0.00073 (0.00075)		
Major Power (SS)	-0.00096** (0.00036)	-0.00096** (0.00036)	-0.00028* (0.00011)	-0.00028* (0.00011)		
Shared Language	0.0010* (0.00042)	0.0010* (0.00042)	-0.000043 (0.00010)	-0.000043 (0.00010)		
<b>Controls</b>						
NGOs (RS)	0.000011** (0.0000037)	0.000011** (0.0000037)	0.0000042 (0.0000042)	0.0000042 (0.0000042)	0.0000014 (0.0000016)	0.0000014 (0.0000016)
International Prosecutions (SS)	0.0061 (0.0034)	0.0061 (0.0034)	0.00047 (0.00061)	0.00047 (0.00061)	0.00014 (0.00016)	0.00014 (0.00016)
Amnesty (SS)	-0.00087* (0.00044)	-0.00087* (0.00044)	-0.00016* (0.000071)	-0.00016* (0.000071)	-0.000100** (0.000037)	-0.000100** (0.000037)
Former Colony (dyad)	0.0026 (0.0023)	0.0026 (0.0023)	0.00056 (0.00078)	0.00056 (0.00078)	0.00078 (0.00058)	0.00078 (0.00058)
Former Colony (any)	-0.00036 (0.00018)	-0.00036 (0.00018)	-0.00021* (0.000083)	-0.00021* (0.000083)	-0.00012* (0.000048)	-0.00012* (0.000048)
Distance	0.00022* (0.000096)	0.00022* (0.000096)	0.000099 (0.000056)	0.000099 (0.000056)	0.000072* (0.000032)	0.000072* (0.000032)
Cold War	-0.000021 (0.00010)	-0.000021 (0.00010)	0.000086 (0.000063)	0.000086 (0.000063)	0.000026 (0.000056)	0.000026 (0.000056)
War on Terror	-0.00017 (0.00030)	-0.00017 (0.00030)	-0.000020 (0.00014)	-0.000020 (0.00014)	-0.000027 (0.000093)	-0.000027 (0.000093)
Observations	161213	161213	141403	141403	270416	270416
R <sup>2</sup>	0.026	0.026	0.00092	0.00092	0.00058	0.00058

See text for details on variables. Robust standard errors clustered by dyad reported in parentheses. Data is available from 1976-2007 for models 1 & 2 and 1976-2012 for models 5 & 6. Shared border variables are included but not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

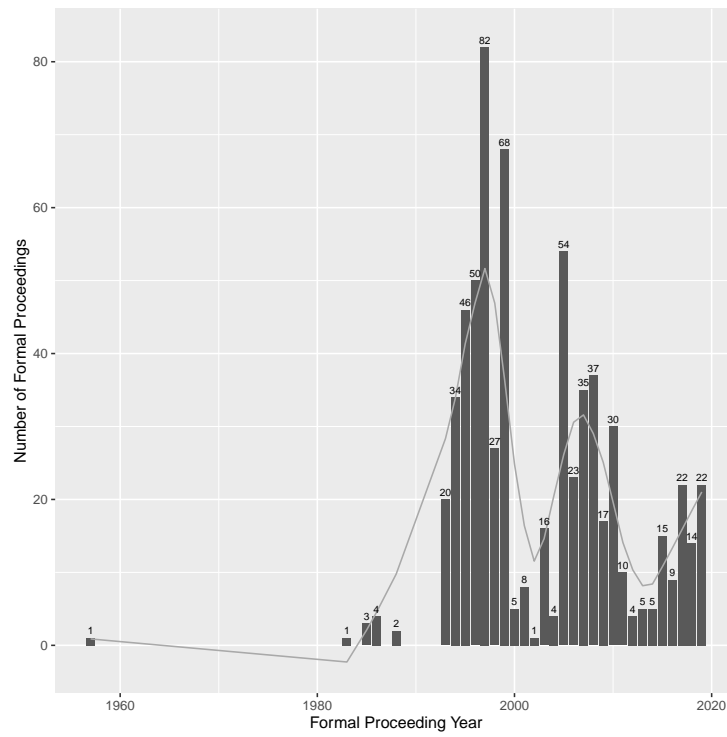
# Appendix: Additional Results

Figure A1: Universal Jurisdiction Investigations, 1957–2017



Note: This figure shows the distribution of INVESTIGATIONS, with a loess line to depict the general trend over time.

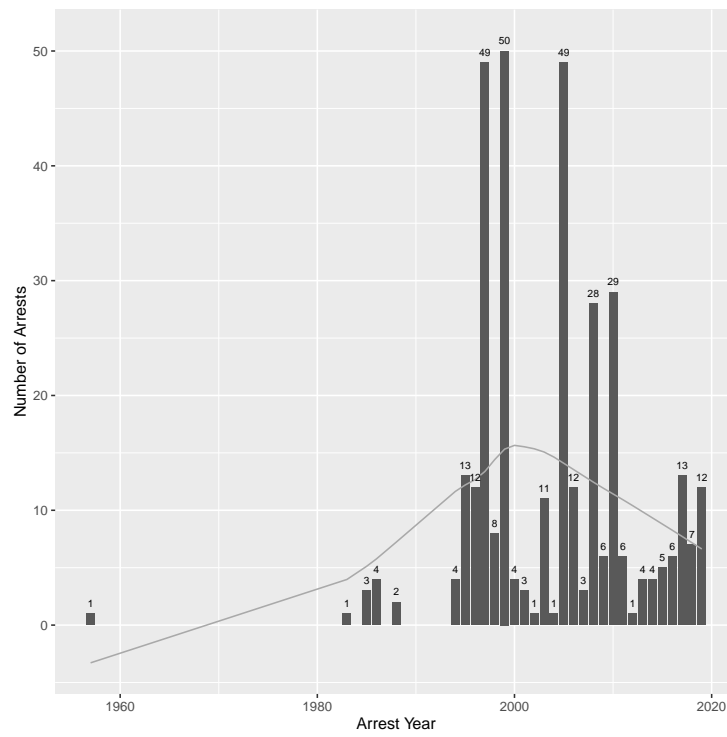
Figure A2: Universal Jurisdiction Formal Proceedings, 1957–2017



Note: This figure shows the distribution of FORMAL PROCEEDINGS, with a loess line to depict the general trend over time.

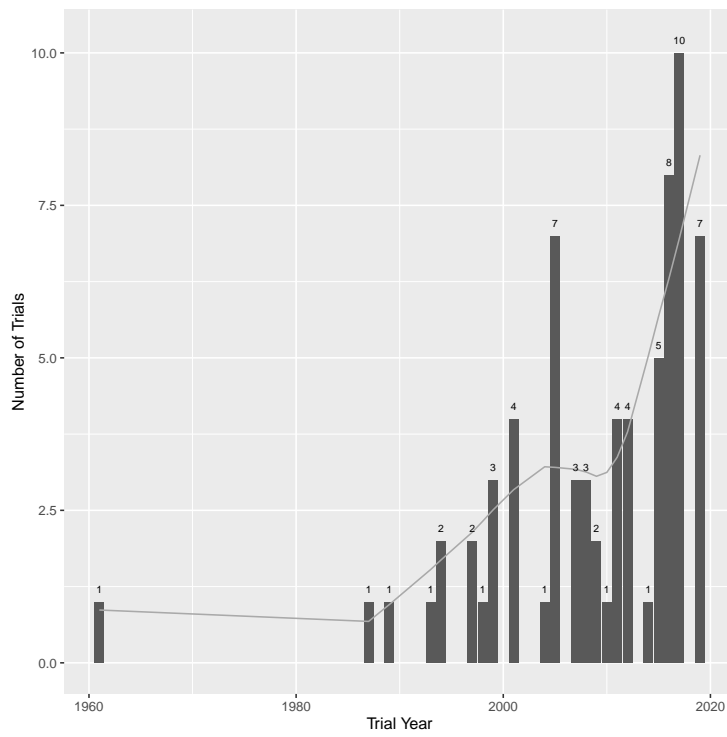


Figure A3: Universal Jurisdiction Arrests, 1957–2017



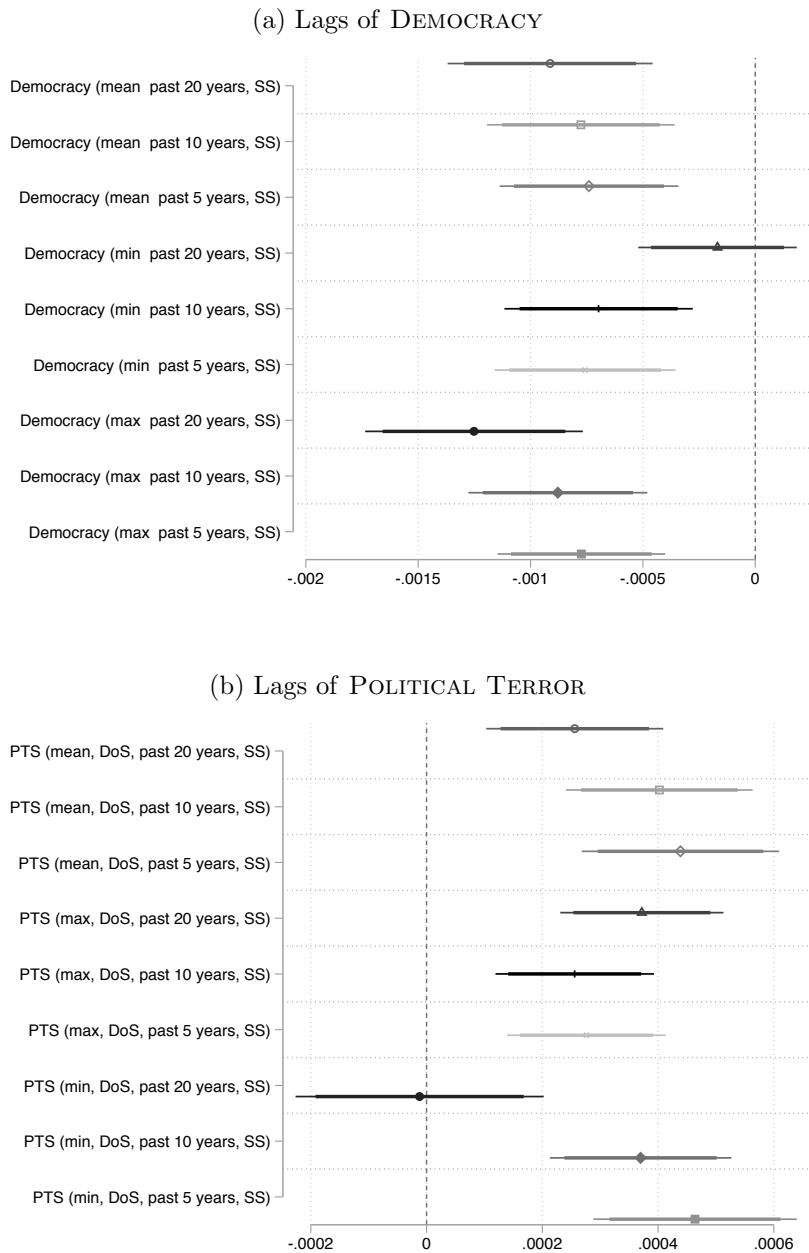
Note: This figure shows the distribution of ARRESTS, with a loess line to depict the general trend over time.

Figure A4: Universal Jurisdiction Trials, 1957–2017



Note: This figure shows the distribution of TRIALS, with a loess line to depict the general trend over time.

Figure A5: Comparing the Impact of Alternative Lag Structures



This graph plots the coefficients on the different measures of democracy and political terror, respectively. The coefficients come from regressions that replicate model 1 but replace the atrocity variables for both democracy and political terror. Again, the results are very robust to using these alternative measures of atrocities.

Table A1: Regressions of steps in UJ cases on migrant stocks

	Initiation	Investigations	Formal Proceedings	Arrests	Trials					
<b>Migration (SS → RS)</b>										
Migrant Stocks	0.00013**	(0.000048)	0.000030	(0.000029)	0.0000068	(0.000025)	-0.0000025	(0.000020)	0.0000048	(0.000013)
<b>Atrocities (SS)</b>										
PTS (mean, DoS, past 10 years, SS)	0.00040***	(0.000082)	0.00012*	(0.000053)	0.000061	(0.000044)	0.000027	(0.000032)	0.0000025	(0.000024)
Democracy (mean past 10 years, SS)	-0.00078***	(0.00021)	-0.000079	(0.00011)	0.0000077	(0.000093)	0.00010	(0.000058)	0.0000028	(0.000032)
<b>Responsiveness (RS)</b>										
Democracy (RS)	-0.00039**	(0.00013)	-0.000043	(0.000087)	-0.0000035	(0.000074)	-0.000071	(0.000072)	-0.0000047	(0.000044)
Population (RS)	-0.00023***	(0.000065)	-0.000022	(0.000043)	0.0000094	(0.000034)	-0.0000027	(0.000030)	-0.0000019	(0.000024)
Center (RS)	-0.000015	(0.00014)	-0.000023	(0.00010)	-0.000048	(0.000087)	-0.000030	(0.000087)	-0.0000060	(0.000047)
Left (RS)	-0.000018	(0.00013)	0.000059	(0.000095)	0.00011	(0.000090)	0.000018	(0.000076)	-0.0000052	(0.000048)
Rome Statute (RS)	0.00069**	(0.00022)	0.00024	(0.00016)	0.00013	(0.00015)	0.000012	(0.00011)	0.0000034	(0.000063)
Private Prosecutions (RS)	0.00036**	(0.00013)	-0.000042	(0.000091)	-0.000064	(0.000071)	-0.000092	(0.000068)	-0.0000048	(0.000044)
British Legal Origin (RS)	-0.00063**	(0.00019)	-0.00012	(0.00013)	-0.000041	(0.000098)	0.000013	(0.000091)	0.0000053	(0.000069)
<b>Cost (RS)</b>										
GDPpc (RS)	0.000021	(0.000046)	0.000040	(0.000033)	0.000064*	(0.000027)	0.000061*	(0.000025)	0.0000086	(0.000017)
OECD (RS)	0.00019	(0.00010)	0.00013*	(0.000064)	0.00012*	(0.000054)	0.00011*	(0.000052)	0.0000038	(0.000028)
Prior Initiation (RS)	0.070**	(0.025)	0.092***	(0.020)	0.084***	(0.016)	0.060***	(0.014)	0.033**	(0.011)
Prior Success (RS)	0.00011	(0.0012)	-0.0014*	(0.00071)	-0.0014*	(0.00063)	-0.0014**	(0.00050)	-0.00068	(0.00037)
<b>Cost (Dyadic)</b>										
GDPpc (SS)	0.00012**	(0.000046)	0.000013	(0.000027)	-0.0000079	(0.000023)	-0.000022	(0.000021)	-0.0000023	(0.000015)
Trade	0.00028	(0.00016)	0.00017	(0.00013)	0.00013	(0.00012)	0.00012	(0.00012)	0.0000038	(0.000063)
Alliance	-0.00069	(0.00036)	-0.000097	(0.00029)	0.000095	(0.00022)	0.00031	(0.00022)	0.00012	(0.00013)
Major Power (RS)	-0.0016	(0.00098)	0.00000081	(0.00059)	0.00033	(0.00056)	0.00039	(0.00051)	-0.00028	(0.00028)
Major Power (SS)	-0.00096**	(0.00036)	-0.00077	(0.00039)	0.00035	(0.00042)	0.00056	(0.00048)	0.000096	(0.00025)
Shared language	0.0010*	(0.00042)	0.0000031	(0.00019)	-0.000027	(0.00017)	-0.00012	(0.00015)	-0.00017	(0.00011)
<b>Controls</b>										
NGOs (RS)	0.000011**	(0.0000037)	-0.00000040	(0.0000021)	-0.0000016	(0.0000018)	-0.0000011	(0.0000014)	0.000000077	(0.0000011)
International Prosecutions (SS)	0.0061	(0.0034)	0.0035	(0.0018)	0.0025	(0.0015)	0.0019	(0.0011)	0.0013	(0.0010)
Amnesty (SS)	-0.00087*	(0.00044)	-0.00053*	(0.00025)	-0.00030	(0.00021)	-0.00029	(0.00017)	-0.00024	(0.00015)
Former Colony (dyad)	0.0026	(0.0023)	0.0019	(0.0013)	0.0018	(0.0011)	0.00099	(0.00095)	0.0012	(0.00094)
Former Colony (any)	-0.00036	(0.00018)	0.000080	(0.00011)	0.000092	(0.000083)	0.00010	(0.000066)	0.000066	(0.000047)
Distance	0.00022*	(0.000096)	0.000065	(0.000056)	0.000045	(0.000048)	0.000042	(0.000038)	0.000019	(0.000023)
Cold War	-0.000021	(0.00010)	-0.000056	(0.000064)	-0.00011	(0.000060)	-0.000098	(0.000058)	0.0000059	(0.000022)
War on Terror	-0.00017	(0.00030)	-0.00023	(0.00019)	-0.00022	(0.00017)	-0.000094	(0.00012)	-0.0000015	(0.000064)
Observations	161213		161213		161213		161213		161213	
R <sup>2</sup>	0.026		0.068		0.066		0.051		0.034	

See text for details on variables. Robust standard errors clustered by dyad reported in parentheses. Data is available from 1976-2007 for models 1 & 2 and 1976-2012 for models 5 & 6. Contiguity variables included but not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Table A2: Regressions of steps in UJ cases on refugee stocks

	Initiation	Investigations	Formal Proceedings	Arrests	Trials
<b>Migration (SS → RS)</b>					
Refugee stocks	0.00039 (0.00022)	0.00021 (0.00018)	0.00014 (0.00015)	0.00018 (0.00011)	0.00015 (0.000086)
<b>Atrocities (SS)</b>					
PTS (mean, DoS, past 10 years, SS)	0.0019*** (0.00051)	0.00061 (0.00032)	0.00040 (0.00028)	0.00019 (0.00019)	-0.000014 (0.00014)
Democracy (mean past 10 years, SS)	-0.0054*** (0.0016)	-0.00066 (0.00076)	-0.00014 (0.00068)	0.00055 (0.00040)	0.00024 (0.00028)
<b>Responsiveness (RS)</b>					
Democracy (RS)	0.0049 (0.0027)	0.0026 (0.0018)	0.0025 (0.0016)	0.000073 (0.0010)	-0.00016 (0.00055)
Population (RS)	-0.0012* (0.00054)	-0.00023 (0.00032)	-0.000091 (0.00027)	-0.00022 (0.00013)	-0.00013 (0.00021)
Center (RS)	0.0015 (0.00093)	0.00075 (0.00068)	0.00055 (0.00060)	0.00064 (0.00055)	-0.00034 (0.00027)
Left (RS)	0.0012 (0.00082)	0.00085 (0.00065)	0.0010 (0.00062)	0.00060 (0.00041)	-0.00042 (0.00031)
Rome Statute (RS)	0.0024* (0.0010)	0.00075 (0.00078)	0.00049 (0.00067)	0.00048 (0.00049)	-0.00014 (0.00013)
Private Prosecutions (RS)	0.0021 (0.0013)	-0.00021 (0.00084)	-0.00035 (0.00066)	-0.00046 (0.00059)	-0.00045 (0.00055)
British Legal Origin (RS)	-0.0039* (0.0017)	-0.0017 (0.0010)	-0.0016 (0.00089)	-0.0014 (0.00074)	-0.00025 (0.00075)
<b>Cost (RS)</b>					
GDPpc (RS)	-0.00024 (0.00064)	0.000018 (0.00036)	0.000031 (0.00036)	0.00023 (0.00025)	-0.000012 (0.00016)
OECD (RS)	0.0013 (0.00082)	0.000092 (0.00044)	-0.00011 (0.00039)	-0.00026 (0.00023)	-0.00016 (0.00016)
Prior Initiation (RS)	0.057* (0.023)	0.082*** (0.018)	0.067*** (0.015)	0.043*** (0.012)	0.027* (0.011)
Prior Success (RS)	-0.0025 (0.0023)	-0.0024 (0.0017)	-0.0013 (0.0014)	-0.0015 (0.0011)	-0.00097 (0.00071)
<b>Cost (Dyadic)</b>					
GDPpc (SS)	0.0012*** (0.00030)	0.000063 (0.00021)	-0.000015 (0.00018)	-0.00018 (0.00016)	-0.00019 (0.00012)
Trade	-0.0058 (0.0034)	-0.0055* (0.0028)	-0.0055* (0.0025)	-0.0036* (0.0017)	-0.0027 (0.0016)
Alliance	0.00010 (0.0012)	-0.00012 (0.00071)	-0.00035 (0.00065)	0.00014 (0.00049)	0.00011 (0.00025)
Major Power (RS)	-0.0014 (0.0024)	0.00094 (0.0019)	0.0016 (0.0018)	0.0018 (0.0016)	-0.00091 (0.00066)
Major Power (SS)	-0.0036* (0.0017)	-0.0019 (0.0017)	-0.0013 (0.0014)	-0.00026 (0.00088)	0.00016 (0.00059)
Shared language	0.0063*** (0.0021)	0.00015 (0.0011)	0.00048 (0.0010)	-0.00012 (0.00081)	-0.00086 (0.00065)
<b>Controls</b>					
NGOs (RS)	0.000026** (0.000088)	0.0000010 (0.0000051)	-0.00000094 (0.0000045)	0.00000045 (0.0000025)	0.00000024 (0.0000031)
International Prosecutions (SS)	0.0076 (0.0047)	0.0052 (0.0030)	0.0035 (0.0025)	0.0030 (0.0018)	0.0025 (0.0020)
Amnesty (SS)	-0.0043** (0.0015)	-0.0024* (0.00095)	-0.0015 (0.00078)	-0.0013* (0.00060)	-0.0010 (0.00060)
Former Colony (dyad)	0.0056 (0.0055)	0.0053 (0.0035)	0.0051 (0.0030)	0.0028 (0.0025)	0.0043 (0.0030)
Former Colony (any)	-0.0016 (0.0013)	0.00063 (0.00075)	0.00085 (0.00064)	0.0011** (0.00037)	0.00058* (0.00026)
Distance	0.0022*** (0.00065)	0.00087* (0.00044)	0.00062 (0.00036)	0.00046 (0.00024)	0.00027 (0.00022)
Cold War	-0.0013* (0.00065)	-0.00100 (0.00054)	-0.00078 (0.00053)	-0.00041 (0.00039)	-0.00028 (0.00021)
War on Terror	0.000072 (0.0011)	-0.000089 (0.00076)	0.00022 (0.00060)	0.00039 (0.00039)	0.00032 (0.00018)
Observations	21856	21856	21856	21856	21856
R <sup>2</sup>	0.029	0.062	0.052	0.036	0.031

See text for details on variables. Robust standard errors clustered by dyad reported in parentheses. Data is available from 1976-2007 for models 1 & 2 and 1976-2012 for models 5 & 6. Contiguity variables included but not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Table A3: Regressions of first instance of each step in UJ cases on migrant stocks

	Initiation	Investigations	Formal Proceedings	Arrests	Trials
<b>Migration (SS → RS)</b>					
Migrant Stocks	0.000048* (0.000022)	-0.000016 (0.000016)	0.000022* (0.000010)	0.000015 (0.0000084)	0.000016 (0.0000083)
<b>Atrocities (SS)</b>					
PTS (mean, DoS, past 10 years, SS)	0.000027 (0.000023)	0.0000099 (0.000022)	0.000026 (0.000016)	0.0000082 (0.000012)	0.0000021 (0.000010)
Democracy (mean past 10 years, SS)	-0.00020 (0.00011)	0.000087 (0.000082)	-0.000030 (0.000031)	-0.0000048 (0.000020)	-0.000027 (0.000020)
<b>Responsiveness (RS)</b>					
Democracy (RS)	-0.00014 (0.000099)	0.000031 (0.000083)	-0.000052 (0.000048)	-0.000065 (0.000044)	-0.000034 (0.000026)
Population (RS)	-0.000034 (0.000030)	0.000021 (0.000024)	-0.000026* (0.000013)	-0.000023 (0.000012)	-0.000017 (0.0000095)
Center (RS)	0.00010 (0.00010)	0.000014 (0.00011)	0.000014 (0.000037)	0.0000096 (0.000038)	0.000011 (0.000036)
Left (RS)	-0.0000019 (0.000047)	0.0000095 (0.000037)	0.000032 (0.000031)	-0.000010 (0.000027)	-0.0000040 (0.000025)
Rome Statute (RS)	0.00016 (0.00010)	0.00012 (0.00010)	0.000012 (0.000062)	-0.000031 (0.000049)	0.000034 (0.000040)
Private Prosecutions (RS)	-0.000012 (0.000071)	-0.000039 (0.000059)	-0.0000089 (0.000028)	-0.000014 (0.000026)	-0.0000032 (0.000025)
British Legal Origin (RS)	0.000091 (0.000071)	0.000052 (0.000072)	-0.000028 (0.000026)	-0.0000085 (0.000023)	-0.000013 (0.000023)
<b>Cost (RS)</b>					
GDPpc (RS)	0.000021 (0.000041)	-0.000054 (0.000036)			
OECD (RS)	-0.000041 (0.000070)	0.000092 (0.000066)			
<b>Cost (Dyadic)</b>					
GDPpc (SS)	-0.0000012 (0.000011)	-0.0000033 (0.000070)			
Trade	0.00012 (0.00011)	0.000061 (0.000067)			
Alliance	0.00026 (0.00020)	0.00022 (0.00016)			
Major Power (RS)	-0.00073 (0.00075)	-0.00048 (0.00054)			
Major Power (SS)	-0.00028* (0.00011)	-0.0000014 (0.000068)			
Shared language	-0.000043 (0.00010)	0.000025 (0.00011)			
<b>Controls</b>					
NGOs (RS)	0.0000042 (0.0000042)	0.0000041 (0.0000032)	0.0000027 (0.0000016)	0.0000026 (0.0000016)	0.0000050 (0.00000054)
International Prosecutions (SS)	0.00047 (0.00061)	0.0012 (0.00061)	0.00041 (0.00024)	0.00042 (0.00024)	0.00038 (0.00022)
Amnesty (SS)	-0.00016* (0.000071)	-0.000076 (0.000066)	-0.000080 (0.000048)	-0.000039 (0.000042)	-0.000044 (0.000036)
Former Colony (dyad)	0.00056 (0.00078)	0.00038 (0.00051)	0.00033 (0.00041)	0.00038 (0.00041)	0.00063 (0.00042)
Former Colony (any)	-0.00021* (0.000083)	0.000023 (0.000062)	-0.000030 (0.000033)	-0.000020 (0.000027)	-0.000022 (0.000025)
Distance	0.000099 (0.000056)	-0.000012 (0.000044)	0.000045 (0.000025)	0.000033 (0.000017)	0.000030 (0.000019)
Cold War	0.000086 (0.000063)	0.00010 (0.000059)	0.000078 (0.000062)	0.000058 (0.000061)	0.000014 (0.000054)
War on Terror	-0.000020 (0.00014)	-0.000082 (0.00013)	-0.00010 (0.000088)	-0.000073 (0.000063)	0.000031 (0.000063)
Observations	141403	143555	281818	283668	294453
R <sup>2</sup>	0.00092	0.19	0.00058	0.00064	0.00057

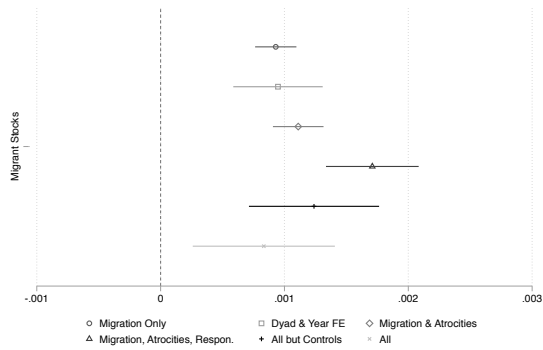
See text for details on variables. Robust standard errors clustered by dyad reported in parentheses. Data is available from 1976-2007 for models 1 & 2 and 1976-2012 for models 5 & 6. Contiguity variables included but not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Table A4: Regressions of first instance of each step in UJ cases on refugee stocks

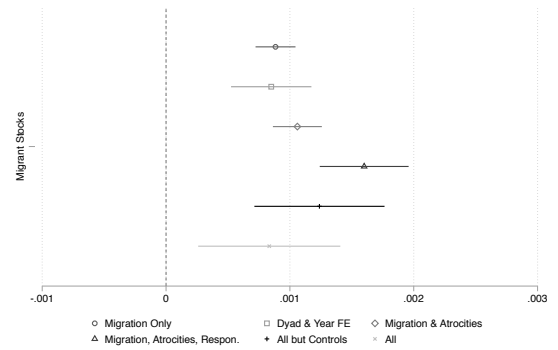
	Initiation	Investigations	Formal Proceedings	Arrests	Trials
<b>Migration (SS → RS)</b>					
Refugee stocks	0.00013 (0.000071)	0.00015 (0.000081)	0.00018* (0.000085)	0.000052 (0.000028)	0.000094 (0.000050)
<b>Atrocities (SS)</b>					
PTS (mean, DoS, past 10 years, SS)	0.00027 (0.00018)	-0.000072 (0.00025)	0.00033 (0.00018)	0.00016 (0.00011)	0.000025 (0.00013)
Democracy (mean past 10 years, SS)	-0.0013 (0.00098)	0.00010 (0.00079)	-0.000083 (0.00025)	-0.00022 (0.00017)	-0.000066 (0.00024)
<b>Responsiveness (RS)</b>					
Democracy (RS)	0.0021 (0.0025)	0.0027 (0.0024)	0.00050 (0.00056)	0.00016 (0.00037)	0.00016 (0.00037)
Population (RS)	-0.00039 (0.00021)	-0.00017 (0.00017)	-0.00020* (0.000084)	-0.00014 (0.000087)	-0.00014 (0.000087)
Center (RS)	0.0012 (0.00084)	0.00032 (0.00059)	0.00038 (0.00031)	0.00025 (0.00029)	0.00025 (0.00029)
Left (RS)	0.00038 (0.00026)	0.00037 (0.00032)	0.00026 (0.00029)	-0.000050 (0.00018)	-0.000050 (0.00018)
Rome Statute (RS)	0.00056 (0.00081)	0.00062 (0.00089)	0.00030 (0.00056)	-0.00019 (0.00012)	-0.00019 (0.00012)
Private Prosecutions (RS)	0.00057 (0.00079)	-0.000075 (0.00066)	0.00029 (0.00020)	0.000078 (0.00017)	0.000078 (0.00017)
British Legal Origin (RS)	0.00078 (0.00092)	-0.00016 (0.0012)	-0.00070* (0.00027)	-0.00054* (0.00024)	-0.00054* (0.00024)
<b>Cost (RS)</b>					
GDPpc (RS)	-0.00069 (0.00065)	-0.00058 (0.00064)			
OECD (RS)	0.00032 (0.00031)	0.00029 (0.00042)			
<b>Cost (Dyadic)</b>					
GDPpc (SS)	0.00014 (0.00013)	-0.000027 (0.00014)			
Trade	-0.00014 (0.00022)	-0.00078 (0.0017)			
Alliance	0.0012 (0.0013)	0.00052 (0.0012)			
Major Power (RS)	-0.0049 (0.0044)	-0.0018 (0.0061)			
Major Power (SS)	-0.00082 (0.00057)	0.00032 (0.00061)			
Shared language	0.00052 (0.0011)	0.00099 (0.0012)			
<b>Controls</b>					
NGOs (RS)	0.000021 (0.000017)	0.0000094 (0.000020)	0.0000082 (0.0000046)	0.0000063 (0.0000033)	0.0000016 (0.0000017)
International Prosecutions (SS)	-0.00057 (0.00038)	0.0022 (0.0013)	0.0014 (0.00082)	0.0012* (0.00047)	0.0014* (0.00067)
Amnesty (SS)	-0.00076 (0.00042)	-0.00067 (0.00040)	-0.00070* (0.00032)	-0.00026 (0.00015)	-0.00039 (0.00021)
Former Colony (dyad)	-0.00072 (0.00063)	-0.00098 (0.0015)	-0.00077* (0.00039)	-0.00075* (0.00034)	0.0036 (0.0021)
Former Colony (any)	-0.00080 (0.00065)	0.00069 (0.00073)	0.00018 (0.00029)	-0.00014 (0.00019)	0.00013 (0.00017)
Distance	0.00045 (0.00032)	0.00024 (0.00038)	-0.000076 (0.00013)	-0.000073 (0.000083)	0.000076 (0.00012)
Cold War	-0.0015 (0.00077)	-0.00049 (0.00082)	-0.00015 (0.00033)	-0.00056 (0.00039)	-0.00023 (0.00033)
War on Terror	-0.0011 (0.00098)	-0.0013 (0.00097)	-0.000046 (0.00066)	0.000087 (0.00023)	0.00053 (0.00036)
Observations	10668	11640	25036	49219	32063
R <sup>2</sup>	0.0032	0.13	0.0027	0.0018	0.0033

See text for details on variables. Robust standard errors clustered by dyad reported in parentheses. Data is available from 1976-2007 for models 1 & 2 and 1976-2012 for models 5 & 6. Contiguity variables included but not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

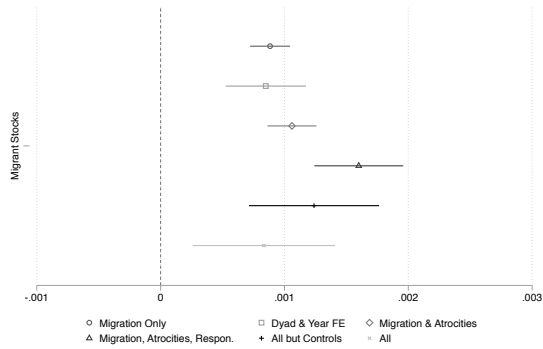
Figure A6: The effect of migration on initiation of UJ cases sequentially adding additional controls; only for data at the decade mark



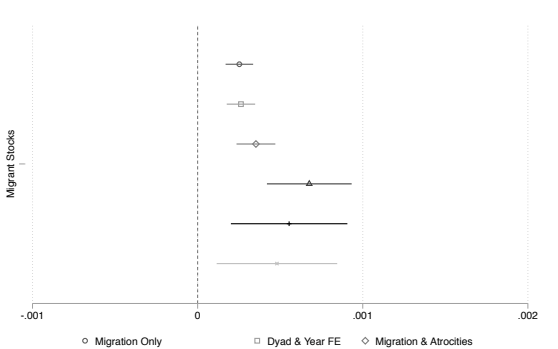
(a) ANY INITIATION INCLUDING JOE DOE CASES



(b) ANY INITIATION EXCLUDING JOE DOE CASES



(c) FIRST INITIATION INCLUDING JOE DOE CASES



(d) FIRST INITIATION EXCLUDING JOE DOE CASES

This figure plots the coefficient on MIGRANT STOCK from separate regressions. Each regresses any or first initiation on migration but sequentially adds in additional variables; all regressions with the first initiation drop all dyad-year observation after the first ignition and include years since 1957, its square, and cube. Dots represent the point estimates and thicker bars represent the 95% confidence interval. The dotted vertical line is at 0; coefficients whose confidence interval does not cross the 0-line are statistically significant at conventional levels.



Table A5: Regressions of Initiations on Explanatory Variables at the decade

	Any			FirstRS		
	All Model 1	No J. Doe Model 2	All Model 3	No J. Doe Model 4	All Model 3	No J. Doe Model 4
<b>Migration (SS → RS)</b>						
Migrant Stocks	0.00083** (0.00029)	0.00083** (0.00029)	0.00048** (0.00019)	0.00062*** (0.00016)		
<b>Atrocities (SS)</b>						
PTS (mean, DoS, past 10 years, SS)	0.0037*** (0.00066)	0.0037*** (0.00066)	0.00058 (0.00037)	0.00096*** (0.00026)		
Democracy (mean past 10 years, SS)	-0.0058*** (0.0016)	-0.0058*** (0.0016)	-0.0027* (0.0011)	-0.0018* (0.00073)		
<b>Responsiveness (RS)</b>						
Democracy (RS)	-0.0039** (0.0012)	-0.0039** (0.0012)	-0.0014 (0.0011)	0.00064 (0.00072)		
Population (RS)	-0.0018*** (0.00047)	-0.0018*** (0.00047)	-0.00074* (0.00034)	-0.00041 (0.00023)		
Center (RS)	-0.00078 (0.0014)	-0.00078 (0.0014)	-0.00028 (0.0011)	-0.00081 (0.00063)		
Left (RS)	-0.00021 (0.0013)	-0.00021 (0.0013)	-0.00030 (0.00076)	0.00027 (0.00056)		
Rome Statute (RS)	0.0044*** (0.0011)	0.0044*** (0.0011)	0.0012 (0.00069)	0.00076* (0.00035)		
Private Prosecutions (RS)	0.0042*** (0.0011)	0.0042*** (0.0011)	0.00087 (0.00075)	0.00031 (0.00035)		
British Legal Origin (RS)	-0.0042** (0.0014)	-0.0042** (0.0014)	-0.00075 (0.00098)	-0.0011** (0.00042)		
<b>Cost (RS)</b>						
GDPpc (RS)	0.00046 (0.00057)	0.00046 (0.00057)	0.00039 (0.00047)			
OECD (RS)	0.0026* (0.0010)	0.0026* (0.0010)	0.0011 (0.00079)			
Prior Initiation (RS)	0.17 (0.11)	0.17 (0.11)				
Prior Success (RS)	0.016 (0.011)	0.016 (0.011)				
<b>Cost (Dyadic)</b>						
GDPpc (SS)	0.00056 (0.00042)	0.00056 (0.00042)	-0.00029 (0.00025)			
Trade	0.0025* (0.0011)	0.0025* (0.0011)	0.0011 (0.0010)			
Alliance	-0.0047 (0.0027)	-0.0047 (0.0027)	0.00023 (0.0022)			
Major Power (RS)	-0.026* (0.010)	-0.026* (0.010)	-0.0069 (0.0094)			
Major Power (SS)	0.035*** (0.0090)	0.035*** (0.0090)	0.011* (0.0057)			
Shared Language	0.0072** (0.0026)	0.0072** (0.0026)	0.0018 (0.0018)			
<b>Controls</b>						
NGOs (RS)	0.00018*** (0.000049)	0.00018*** (0.000049)	0.000050 (0.000039)	0.00026 (0.000015)		
International Prosecutions (SS)	0.038* (0.015)	0.038* (0.015)	0.012 (0.010)	0.0019 (0.0019)		
Amnesty (SS)	-0.0032 (0.0027)	-0.0032 (0.0027)	-0.0011 (0.0014)	-0.0011 (0.00074)		
Former Colony (dyad)	0.028* (0.013)	0.028* (0.013)	0.014 (0.012)	0.021* (0.0089)		
Former Colony (any)	-0.0016 (0.0012)	-0.0016 (0.0012)	-0.0010 (0.00078)	-0.00083 (0.00051)		
Distance	0.0000064 (0.00076)	0.0000064 (0.00076)	0.000091 (0.00048)	0.00070* (0.00034)		
Cold War	0.00011 (0.0011)	0.00011 (0.0011)		0.0027** (0.0010)		
War on Terror						
Observations	16604	16604	15146	30323		
R <sup>2</sup>	0.049	0.049	0.012	0.011		

See text for details on variables. Robust standard errors clustered by dyad reported in parentheses. Data is available from 1976-2007 for models 1 & 2 and 1976-2012 for models 5 & 6. Contiguity variables included but not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Table A6: Sensitivity Analysis

Outcome: ANY CASE					
Treatment:	Est.	S.E.	t(H0=0)	$R_{Y \sim D   \mathbf{X}}^2$	$RV_{q=1}$
MIGRANT STOCK	0.0001	0.0000	4.59	.01%	1.14 %
df = 161530	BOUND 1X DEMOCRACY (SS): $R_{Y \sim Z   \mathbf{X}, D}^2 = .01\%$ , $R_{D \sim Z   \mathbf{X}}^2 = 8.5\%$				
	BOUND 2X DEMOCRACY (SS): $R_{Y \sim Z   \mathbf{X}, D}^2 = .02\%$ , $R_{D \sim Z   \mathbf{X}}^2 = 16.99\%$				
	BOUND 3X DEMOCRACY (SS): $R_{Y \sim Z   \mathbf{X}, D}^2 = .03\%$ , $R_{D \sim Z   \mathbf{X}}^2 = 25.49\%$				
Outcome: FIRST CASE					
Treatment:	Est.	S.E.	t(H0=0)	$R_{Y \sim D   \mathbf{X}}^2$	$RV_{q=1}$
MIGRANT STOCK	0.0001	0.0000	4.22	.01%	1.12 %
df = 141768	BOUND (1X DEMOCRACY (SS)): $R_{Y \sim Z   \mathbf{X}, D}^2 = .01\%$ , $R_{D \sim Z   \mathbf{X}}^2 = 8.29\%$				
	BOUND (2X DEMOCRACY (SS)): $R_{Y \sim Z   \mathbf{X}, D}^2 = .01\%$ , $R_{D \sim Z   \mathbf{X}}^2 = 16.59\%$				
	BOUND (3X DEMOCRACY (SS)): $R_{Y \sim Z   \mathbf{X}, D}^2 = .02\%$ , $R_{D \sim Z   \mathbf{X}}^2 = 24.88\%$				

This table displays the results of the sensitivity analysis. The quantity  $R_{Y \sim D | \mathbf{X}}^2$  shows what happens in an extreme case in which a confounder that is orthogonal to all the covariates that explains 100% of the residual variance of ANY INITIATION and FIRST INITIATION (RS). In this extreme case, the unobserved confounder would have to explain 0.03% and 0.02%, respectively, of the residual variance of MIGRANT STOCK. The rest of the covariates already explain 62% of the variation on MIGRANT STOCK. Perhaps, it is possible that such an extreme confounder exists.

The quantity  $RV_{q=1}$  is the robustness value and shows that unobserved confounders (orthogonal to the covariates) that explain more than 1.57% for ANY INITIATION and 1.26% for FIRST INITIATION (RS) of the residual variance of both the treatment and the outcome are strong enough to bring the point estimate to 0 (a bias of 100 percent of the original estimate). This may not seem like a lot but it is very difficult to predict these cases because they are so rare: the  $R^2$  of the regression on ANY INITIATION is 0.026 and on FIRST INITIATION (RS) is 0.0007. Thus this confounder would have to explain more than half the variation of ANY INITIATION and 26 times more variation of FIRST INITIATION (RS) than is currently explained.