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Autocratic Consent to International Law: the Case of the International Criminal Court's Jurisdiction, 1998–2017

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Abstract

This article contributes to an understanding of why autocrats have accepted the jurisdiction of the International Criminal Court. Leveraging their ability to obstruct their own prosecution, autocrats have traded off the risk of unwanted prosecutions against the deterrent threat that prosecutions pose to political rivals and patrons of their enemies conspiring to oust them. The risk of unwanted prosecutions and the court's deterrent threat both arise because ICC prosecutions credibly communicate guilt for international crimes to capital-disbursing democracies, which may, insofar as possible, use leader-specific economic statecraft to prevent the administration of foreign states by those whom the court signals are guilty of international crimes. Analysis using fixed effects and matching shows that a greater reliance on capital publicly financed by democracies increased the probability that a state accepted the court's jurisdiction only when it was an autocracy (1998–2017). ICC jurisdiction also lengthened the tenure of autocrats and reduced the severity of civil conflict in autocracies.

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Years before the International Criminal Court (ICC) reached its first conviction of Congolese militant, Thomas Lubanga, jurists and statesmen saw the court as an inflection in the rule of law over world politics. Acheson and Dulles quietly sponsored planning for the court until 1954. Kissinger feared that the final product had “virtually unlimited discretion in practice.”¹ The ICC emerged from six decades of negotiation in the League of Nations, United Nations, multilateral diplomacy, and the proof-of-concept shown by the UN tribunals. Its mandate to develop and enforce international criminal law globally is unique and unprecedented. It aims to prosecute primary perpetrators as well as planners and elites for typical horrors of modern warfare and repression—murder, rape, torture, child soldiering. It prosecutes at-will (*proprio motu*) in exercise of jurisdiction based on nationality and territoriality principles. It constrains the traditional sovereignty of member states through its exclusion of customary immunities and competence to override the primary jurisdiction of national courts—a principle magnified in the Rome Statute to limit the *ne bis in idem* rights of persons in its jurisdiction.² Its parties relinquish their rights to discontinue the court’s exercise of jurisdiction when exiting the Rome Statute, and they may only exit at long delay. The treaty’s non-reservability, non-derogability, and $\frac{7}{8}$ supermajoritarian amendment rule create an unusual uniformity and inflexibility in states’ obligations to the court.

Nevertheless, leaders of 123 states accepted the court’s jurisdiction between the Rome Statute’s 1998 opening and 2017. At some point in those two decades, 44% of the present parties were autocratic and 28% had experienced civil conflict. Leaders of Burundi and the Philippines are alone in formally seeking to exit the treaty—decisions made after the court began examinations of their actions.³ The gravity of obligations in the Rome Statute exceeds that undertaken by the few leaders who accept the International Court of Justice’s compulsory and unlimited jurisdiction. It is a puzzle why so many leaders consent to the jurisdiction of such a court, which may prosecute, detain, and convict them.

The ICC and prior criminal tribunals have overcome many obstacles in their missions. They prosecuted and incarcerated hundreds of former state officials of various high rank.⁴ The ICC has indicted dozens of African nationals and engaged in preliminary examinations of evidence on territories such as Afghanistan, Iraq, and Ukraine. Leaders have accepted the ICC’s jurisdiction only to find the court prosecuting regime insiders a few years later.⁵ Even when international criminal courts have little hope of arrest and surrender, politicians and states under their scrutiny have viewed them seriously and tried to delegitimize or rescind their authorities. The apparent power of international criminal courts leads some to propose that civil society and powerful states

¹Kissinger 2001, 95

²Jurisdiction is over war crimes, crimes against humanity, genocide, and aggression committed by party nationals and on party territory. Rome Statute Article 21 allows application of ICC practice, applicable *lex specialis*, and general international law. Parties have the right to “self-refer” proposed situations for preliminary examination. They accept the court’s right to arbitrarily extend jurisdiction when seized by a Security Council resolution. Schabas 2016.

³*New York Times*, 9 November 2017; *New York Times*, 14 March 2018

⁴*viz.* Jean Kambanda, Foday Sankoh, Charles Taylor, Slobodan Milošević, Radovan Karadžić, Momčilo Krajišnik and Biljana Plavšić, Ramush Haradinaj, Laurent Gbagbo, and Uhuru Kenyatta.

⁵*e.g.* Kenya’s Mwai Kibaki and Côte D’Ivoire’s Laurent Gbagbo accepted ICC jurisdiction. The court later indicted Kibaki’s successor (Kenyatta) and Gbagbo himself.

have pressured for acceptance of the ICC's jurisdiction. Others propose that leaders see the court as a device for credibly committing to more peaceful governance.

This article develops the argument that a primary motive for autocrats to accept the ICC's jurisdiction is to marginalize political competitors. Autocrats trade off the ICC's potential to harm their own careers in unwanted prosecutions against the similar threat that the court poses to contenders for office and the foreign patrons of domestic enemies of the state. The court's power to harm political careers stems from its ability to credibly communicate personal guilt for atrocities. This may trigger economic punishment by wealthy democracies that prefer to keep atrocity-doers out of office. Leaders may exploit their control over inculpatory evidence to preemptively sabotage investigations and prosecutions—to a degree determined by the institutionalization of electoral turnover. But the court's discoveries of guilt result from evidence made credible by court's resourceful specialization and analytical integrity. The reliance of incumbents on publicly financed foreign capital to gain and keep office is crucial to explaining the deterrent capacity of prosecutions resting on broad criminal liability theories. The risk of losing foreign capital raises the opportunity cost to domestic elites and foreign patrons of internal enemies of involving themselves with anti-regime violence.

Patterns consistent with this argument are documented below with 788 leaders from 171 states from 1998 to 2017.⁶ Publicly financed capital disbursed by democracies makes autocrats—who are relatively better at obstructing ICC prosecutions—more likely to accept the court's jurisdiction. This money has no effect on the choices of democrats, who face greater exposure to prosecution. The ICC's jurisdiction also reduces civil conflict in autocracies and enhances the political survival of autocrats.

THEORIZING CONSENT

Acceptance of the ICC's jurisdiction has generated much scholarly attention. At least five theories might explain why certain leaders make the choice. The *cultural* theory explains why consolidated and newer democracies rapidly accepted the court's jurisdiction, arguing that domestic political and legal cultures make states accept or reject the liberal and legal character of a court like the ICC. The first version claims that the liberal democratic and legalistic cultures of democracies determine the identities of their citizens and statesmen, who place high value on defending human rights and lawfully enforcing international rules.⁷ The second version models the court's design (1994–1998) as an interstate coordination game whose outcome determined expected legal costs under jurisdiction. The outcome left a set of likely losers and likely winners—states with European legal traditions. Winners would consent to jurisdiction.⁸

The *empty-promises* theory would argue illiberal states have consented to jurisdiction in search of praise for endorsing the symbolic values of modernity—costless genuflections to a court

⁶The panel the excludes 23 members (as of 2010) of the Organisation for Economic Co-Operation and Development's Development Assistance Committee (OECD DAC), whose motives for accepting jurisdiction are likely unrelated to those theorized in this article.

⁷Bass 2000, Kelley 2007; Simmons 2009, Ch. 3

⁸Mitchell and Powell 2011, Ch. 4

powerless to punish the guilty.⁹ The *diffuse-reciprocity* theory proposes that states made reciprocal “forward payments” to states with influence over them by emulating their positions toward the court.¹⁰ The *persuasion* theory proposes that international non-governmental organizations persuaded states to see existence under a strong ICC as complementary to their interests.¹¹ The *civil-peace* theory proposes that stability-seeking states see the court as a surrogate for domestic institutions failing to credibly commit governments to internationally lawful governance over internal enemies. Hands tied against repression and amnesties generate peaceful submission to the *status quo*.¹²

This understanding of the court’s expanding jurisdiction can be improved in two ways. First, these theories rely on the idea that the court intends to enforce compliance with international law through prosecution. Only one proposes how. The source of such compliance is an interdisciplinary focus that deserves close examination across substantive issues.¹³ Moving this direction, civil-peace theorists substantiated the claim that leaders tied hands in accepting the court’s jurisdiction by showing that the exercise thereof reduced anti-civilian violence.¹⁴ Yet, as this article argues, the court’s uneven enforcement capacity may also explain why states capable of evading its reach have accepted its jurisdiction, and why their territories might be subsequently more peaceful.

Second, none of the theories except the empty-promises theory implies a sovereign abuse of rights. States are not theorized to have undertaken bad-faith commitments incompatible with the ICC’s object and purpose—to end impunity for international crimes. They are not suspected of having engaged the court’s jurisdiction, prosecutor, and chambers for repressive ends. This presumption of benign or good intentions contrasts with important facts. François Bozizé, Joseph Kabila, Yoweri Museveni, and Alassane Ouattara preside over states accused of serious crimes. They accepted the court’s jurisdiction and referred investigations to it about internal enemies.¹⁵ The court acquitted Bozizé’s foe in a decision citing a litany of deficiencies in the case against him.¹⁶ Ouattara arranged for his predecessor’s indictment and surrender immediately upon entering office. Nine years later the court’s appeals chamber found the evidence against the defendant “exceptionally weak” and acquitted him in advance of defense pleadings.¹⁷ The majority of the court’s list of indictees are politicians, militants, or their operatives engaged in violent challenges

⁹Hafner-Burton and Tsutsui 2005

¹⁰Goodliffe and Hawkins 2009, Goodliffe et al. 2012

¹¹Deitelhoff 2009

¹²Simmons and Danner 2010; *cf.* Chapman and Chaudoin 2012

¹³Gilligan 2006, Simmons 2009, Alter 2014, Pelc 2016

¹⁴Simmons and Jo 2016 show that the stigma and risk of incarceration from ICC prosecutions deterred 101 states with histories of civil war and 264 rebel groups from one-sided civilian killings, 1998–2011.

¹⁵The junta leading Mali after March 2012 also self-referred a situation to the ICC in 2013 amid struggles against Al Qaeda in the Islamic Maghreb. Malian president, Alpha Oumar Konaré, accepted the court’s jurisdiction in 2000 while fighting Libya-supported insurgents.

¹⁶*Prosecutor v. Jean-Pierre Bemba Gombo* (ICC Judgment of the Appeals Chamber, 8 June 2018).

¹⁷*Prosecutor v. Laurent Gbagbo and Charles Blé Goudé* (ICC Delivery of Decision, 16 January 2019).

against states.¹⁸ The court has indicted only four incumbents.¹⁹ These facts better fit theories where states and their agents manipulate law and judicial processes for coercion and oppression.²⁰ The idea was important enough to motivate Hague Lectures by Politis in 1925 and Guggenheim in 1949.²¹ It is alive today in interstate litigation.²²

A THEORY OF SECURITY AND EXPOSURE

This article argues that autocrats accept the ICC's jurisdiction to marginalize threats to their political survival. They trade off self-exposure to an ICC prosecution against prosecutorial deterrence focused on their rivals and enemies—those capable and willing to conspire to violently oust them. The risk of self-exposure and this deterrent threat each exist because the court's prosecutions credibly communicate guilt for atrocities. This information may trigger leader-specific economic punishment by wealthy donor states preferring to keep the planners and perpetrators of atrocities out of office. The impact of prosecutions on political careers and the choices of leaders accept the court's jurisdiction are studied below. Three hypotheses are drawn.

Why ICC prosecutions create information harmful to the careers of politicians

Domestic elites—military elites, major landowners and businessmen, first families, local officials, and intermediaries who deliver votes—demand payment in pork, patronage, and club or public goods in exchange for putting or keeping politicians in leadership posts. Regardless of the sort of goods used to buy this support, and whether or not leaders are elected, leaders must cultivate reputations of resourcefulness to stay politically competitive.

A significant source of the capital that domestic elites expect leaders to raise comes is foreign. Aid provided by fewer than thirty wealthy democracies is known to prolong leaders' tenure.²³ Those donors make disbursements contingent on recipients' behavior, and may enforce the deal with the threat of leader-specific punishment as they do in other contexts.²⁴ They also disburse nonconcessional loans multilaterally, with explicit policy conditionality and preferential treatment for major aid recipients.²⁵ By leveraging their wealth, democracies subtly influence politics and

¹⁸ *e.g.* Lubanga, Bemba Gombo, Joseph Kony, and William Ruto.

¹⁹ *viz.* Omar Al-Bashir, Muammar Gaddafi, Saif al-Islam Gaddafi, Uhuru Kenyatta.

²⁰ Moe 2005, Hollyer and Rosendorff 2011, Hashimoto 2012

²¹ Byers 2002

²² *Immunities and Criminal Proceedings (Equatorial Guinea v. France), Judgment of 6 June 2018, ICJ Reports 2018* and its Dissenting Opinion of Judge Donoghue; *Certain Iranian Assets (Islamic Republic of Iran v. United States of America), Preliminary Objections of the United States of America on 1 May 2017, ICJ Reports 2017*. The preoccupation of jurists and states with abuse of rights motivates substantive and procedural requirements for states invoking countermeasures, necessity, and *force majeure* as circumstances precluding wrongfulness; as well as the “wave goodbye” to the *exceptio non adimpleti contractus* in the International Law Commission's ultimate codification of the law of state responsibility. *Application of the Interim Accord of 13 September 1995 (the former Yugoslav Republic of Macedonia v. Greece), Separate Opinion of Judge Simma, ICJ Reports 2011*. It is evident in references to “good faith” in the Vienna Convention on the Law of Treaties.

²³ Smith and Vreeland 2006, Bueno de Mesquita and Smith 2010, Ahmed 2012

²⁴ McGillivray and Smith 2008

²⁵ Stone 2008

political survival around the world.

Moral and economic objections to funding rulers with unclean hands using public money

Citizens in wealthy donor-democracies may not object to the use aid and loans as a tool of statecraft. A reasonable conjecture, however, is these citizens *do* object when aid helps ostensibly evil leaders stay in power. In particular, pivotal groups of citizens are likely to object to funding states led by people guilty of grave international crimes that have held sway in the public imagination since the end of colonialism, totalitarianism, and decolonization. There are moral and economic reasons for this objection. The moral reason is that many citizens support aid and lending because they wish to improve the welfare of foreigners.²⁶ Financing misrule and outright evil harms is contrary to the interests of the foreign public. The economic reason is that democracies finance giving and lending with taxation. Credible and public proof of a foreign leader's guilt of atrocities may prompt democracies to leverage disbursements toward deposing the leader. Promises of generosity can be used a carrot when domestic interests favor continued disbursements, and threats of austerity can be used as a stick when they do not.²⁷

The problem is that few of the citizens in wealthy democracies understand when foreign authorities have committed atrocities. This requires knowledge of definitions, elements, standards of proof, and liability theories native to general principles of criminal law and the *lex specialis* of international criminal law which few possess. Facts are elusive too. Constructing persuasive cases is not trivial, and there is a history of expert counsel and judges bungling them.²⁸ A key difficulty is the fact that planners delegate atrocities to subordinates and rarely document orders, intentions, motives, and relationships. Proving key objective, subjective, and contextual elements is hampered by conceptual and inferential challenges. Sovereign immunities render state organs disinterested in solving these challenges. International and nongovernmental organizations are unequipped for them.²⁹ Cases surely exist where educated observers can be nearly certain who is responsible for atrocities, even in a fairly precise legal sense. State-organized massacres in Libya and Syria since 2011 are examples.³⁰ There, key pieces of broadcast evidence established proof of criminal elements and additional evidence could be quickly accessed by investigators. But these

²⁶Lumsdaine 1993, Milner and Tingley 2010, 2011

²⁷Hashimoto 2012 finds that international criminal prosecutions generated large, sustained aid bonuses for successors of indicted incumbents. Trade and investment sanctions may be less likely tools of turnover, as they are often breaches of treaty and customary obligations when performed without SC authorization. Concentrated domestic interests are harmed by such sanctions, and targeted states may instead engage the commerce of autocracies. Binding international regulation of capital disbursements for political purposes is inchoate; *e.g.* the 2011 Maastricht Principles. The influence of concentrated domestic interests (*i.e.* Broz and Hawes 2006) loses bite because capital may be operated as a carrot or stick. The magnitude of disbursements by democracies remains unparalleled despite China's and Russia's recently increased activity.

²⁸Boister and Cryer 2008.

²⁹Human rights NGOs tend focus on fact-collecting—even then, without necessarily an eye to all dispositive facts needed for conviction. An example is the Amnesty International 2000 inquiry into NATO's bombing in Serbia. Ostensibly in reaction to Amnesty, the ICTY's prosecutor argued against indicting NATO member officials for war crimes in light of the unavailability of evidence and infeasibility of prosecution.

³⁰Kassab 2018

are exceptions to the rule. It is typically far harder to prove criminal liability for atrocities.

The credibility of international criminal prosecutions

The ICC is a specialist in providing this information. It assigns personal guilt for atrocities in a legitimized conceptual and procedural framework, credibly communicating that its indictees and convicts are *a posteriori* more likely guilty than others are. The court's legitimacy, investment, and reputational qualities explain its credibility.

First, there is much sovereign consent to the solutions to the problems of defining crimes and assigning liability that are encoded in the Rome Statute and practice of the ICC. It was state practice that polished and crystallized the views of jurists and the public toward these solutions—starting in dialogue over a permanent court in the interwar period, accelerating after the Nuremburg and Tokyo precedents, and gestating through the Cold War. The Rome Statute and the court's subsidiary rules formed significant legislation and restatement by jurists and states of the law of armed conflict, human rights, genocide, crimes against humanity, jurisdiction, admissibility, procedure, evidence, and criminal liability. The Rome Statute is a paragon of an incomplete multilateral contract. It awaits dialectic interpretation by jurists and states performing the art of treaty interpretation in trial and in print.³¹ If legitimacy is derived from rigidity, then the statute's malleability lacks the legitimacy of criminal codes in mature municipal fora. But the better view is that the statute's flexibility is a source of legitimacy, as it allows sovereign interests—and hence public interests, indirectly—to determine its evolution. Other actors seeking to blame natural persons for atrocities before an international audience are far more likely employ idiosyncratic standards untethered to this consensual and democratic process.

Second, the ICC has an unrivaled ability to invest in collecting evidence, interpreting atrocity law, and evaluating questions of jurisdiction and merits. Its budget has exceeded \$100M annually since 2009, and was proposed at nearly \$170M for 2019.³² The court's investment does two things. It lowers both the rate of mistaken non-indictments and non-convictions (false negatives) and the rate of mistaken indictments and convictions (false positives). It also sends a costly signal to observers that the court has in fact taken steps to lower false-inference rates. The court may reach an inflexible lower bound on the probability of a false negative ruling due to the indecisiveness of evidence and judicial disagreement on inchoate or poorly defined rules. But its investments help it push the probability of false positive rulings toward zero. Other interested actors are unlikely to concentrate such resources on particular individuals. Their active commitments are more numerous and spread more broadly. Their research expenditures rarely exceed the ICC's.³³

Third, ICC judges evaluating a case stake their reputations in the legal community for

³¹Sadat and Jolly 2014

³²The resolutions of the ICC's Assembly of States Parties hosted at the court's website publishes budgets. In the 2019 proposal the Judiciary and Office of the Prosecutor are allocated roughly 40%. The court's expenditures track its budgets closely.

³³www.ngo-monitor.org reports that AI's expenditures grew from \$60M in 2007 to \$329M in 2017 (of which less than 50% was related to research and advocacy). HRW's expenditures grew from \$30M in 2005 to \$76.4 million in 2017. Oxfam, CARE, and World Vision have budgets larger than the ICC's. Yet each expends on a range of activities unrelated to atrocities.

professional competence—for writing decisions and opinions of high quality and with a low rate of false-positives. This sort of reputation, in turn, influences their chances of re-selection for prestigious judgeships, lucrative appointments to arbitral tribunals, and seats in universities and international organizations. Whether ICC judges apply the science of international law impartially, independently, and entirely competently is known to them alone.³⁴ But their desire to acquire reputations for quality is the overriding concern. By contrast, judges prosecuting the rivals and enemies of their political masters do not care about cultivating reputations for high-quality reasoning, or for writing judgments and opinions with longevity. Outside of the consolidated democracies, leaders often use judges and courts as instruments to coordinate and legitimize repression, building cases against regime enemies through flawed uses of law and fact.³⁵ Finally, there is little reason to believe that fact-finders working for international NGOs would seek reputations for judicial credibility. The competitive marketplace for non-profit funding has been found to pull them toward dramatic results at the expense of credibility.³⁶

In summary, the ICC's power to damage politicians' careers arises from the interplay of clientelism, the growth of international flows of publicly financed capital, and the credibility of information revealed by a specialized court. The court's public disclosures of politicians' behavior can disqualify them from the fruits of foreign capital, closing off futures as viable candidates for leadership. Likewise, aspirant incumbents in autocracies should be affected by these disclosures, an implication important for understanding the expansion of the court's jurisdiction.

Why autocrats accept the ICC's jurisdiction

Beyond consolidated democracies where institutionalized mechanisms of peaceful leadership turnover operate, leaders face a variety of violent threats to their political tenure. Political rivals from within and without the regime aspire to oust them, undermine their campaigns to expand executive power, and organize anti-regime violence in the form of coups, rebellions, riots, and protests. Terrorists and organized criminals undermine their rule indirectly by burdening the state's coffers and revealing the regime's incompetence. Meddlesome foreign incumbents patronize these actors, supplying them with ideological motives, sanctuary, money, labor, weapons, and intelligence. Autocrats may co-opt or repress these threats—each option a double-edged sword. Co-optation is costly and may prove impossible, while relying on security forces for repression increases their power to induce leadership turnover.³⁷ The ICC's jurisdiction presents autocrats with a modest alternative. The key consideration they consider when deciding whether to accept its jurisdiction is the trade-off between a *security effect* and an *exposure effect*. The former arises because jurisdiction protects leaders from those who threaten their rule, while the latter arises because jurisdiction expose leaders and their protegés to prosecution. Leaders accept jurisdiction under a favorable trade-off.

³⁴*c.f.* Voeten 2008

³⁵Pereira 2005

³⁶Cohen and Green 2012

³⁷Svolik 2012

The security effect

The court's jurisdiction provides leaders with domestic security via two mechanisms. Jurisdiction deters their direct political rivals from cooperating with domestic elites to challenge the regime. It also deters foreign patrons of rivals and internal enemies (who may be uninterested in public office) from continuing to support their clients.

Autocrats who wish to marginalize direct rivals for office exploit three facts. First, rivals must credibly promise a future flow of resources to domestic elites whose support rivals need to enter and remain in office. Bilateral and multilateral sovereign finance is a significant source of this money. Second, leaders have unique control over the supply of evidence that could inculpate enemies engaged in anti-regime violence by virtue of control over state security and intelligence agencies. Leaders have exercised their capacity to prepare such evidence for the ICC. Third, the credibility of the promise comes into question if the court has jurisdiction to prosecute strategies of violence arising from conflict between loyalists and dissentients. Leaders, rivals, and elites understand that foreign donor democracies view ICC prosecutorial action as a credible signal of guilt and may act upon them with leader-specific punishment. Rivals and elites take this into account *ex ante* when deciding whether to conspire against incumbents. The court's jurisdiction thus protects leaders from rivals by raising the opportunity cost to elites of organizing anti-regime action—and therefore, it limits the resources available to highly resolved challengers.

The security effect's second mechanism enters when leaders face threats from terrorist, irredentist, or criminal organizations. The rulers of these internal threats are unlikely to care about international prosecutions and their effect on the foreign policy of donor-democracies. But they typically rely on foreign patrons in positions of state authority, who do care. Leaders can exploit their fear of ICC prosecutions, accepting jurisdiction to deter patron-client relationships.

By the mid-1990s, leaders or their counsel would have recognized this security effect in relation to enemies large and small. By then, jurists had abandoned the "major-criminal" admissibility rule written in the 1945 Nuremberg Charter in favor of "gravity" requirement focused on the seriousness of the crime. The 1993-1994 statutes of the ICTY and ICTR, and the Rome Statute itself speak of "gravity" and "seriousness" rather than "major offenders." Tribunal practice confirmed this evolution with immediacy. The ICC's Appeals Chamber all but closed the door on the "major-criminal" rule in 2006, reversing the Pre-Trial Chamber's attempt to apply a "central-figure" rule.³⁸

The legal pursuit of foreign patrons

International criminal courts have pursued foreign patrons using several accepted theories of liability: individual criminal responsibility, individual criminal responsibility in the context of a co-perpetration or a joint criminal enterprise, and command responsibility. The Rome Statute considers a defendant liable for crimes as either a principal or secondary co-perpetrator under the theory of individual criminal responsibility if they organize, mastermind, control, order, solicit,

³⁸*Prosecutor v. Ntaganda*, Case No. ICC-01/04-02/06, Judgement on the Prosecutor's Appeal against the Decision of Pre-Trial Chamber I Entitled 'Decision on the Prosecutor's Application for a Warrant of Arrest, Article 58' 13 July 2006, para. 77. See also Sadat 2013, 202-203.

induce, aid, abet, or similarly assist the commission of the crime with the intention of doing so.³⁹ Famously, international criminal courts impute forms of “guilt by association” when defendants partook in plans to commit crimes or having predictable consequences of criminal effect. The ICC employs the narrow version of this theory, known as co-perpetration. The broader customary theory known as joint criminal enterprise was excluded from the Rome Statute at the drafting stage. The degree to which it nevertheless influences the reasoning of ICC judges is a point of debate.⁴⁰ Defendants may also be held separately liable for actions of subordinates under the theory of command responsibility. Here, a superior-subordinate relationship need not be military, hierarchical, or lawful. The crucial elements of proof are the existence of a person’s “effective control” over primary perpetrators lending him power to prevent or punish their behavior, and his knowledge of—or negligent ignorance of—the actions of the primary perpetrators.⁴¹

The precise elements of theories of liability are still to be resolved by interpretation of Rome Statute.⁴² But the ICC and other tribunals have some combination of the theories above to prosecute foreign patrons.⁴³ For example, the Special Court for Sierra Leone indicted Charles Taylor, alleging that the former Liberian president was individually responsible for atrocities committed by three rebel groups fighting in Sierra Leone (1996–2002). ICTY prosecutors argued that Slobodan Milošević partook in a joint criminal enterprise with Serbian paramilitaries to commit atrocities in Croatia and in Bosnia and Herzegovina (1991–1995). The ICC has thus far not applied the theory of co-perpetration to a foreign patron, but it will be an option in Georgia and, if the court chooses to proceed with prosecution, in Ukraine. The ICC’s case against Bemba Gombo applied the command-responsibility theory to his alleged interaction with subordinates operating in the Central African Republic.

The exposure effect

Accepting the court’s jurisdiction exposes leaders and protégés to the court’s *proprio motu* discretion, of course. Insiders, foreign spies, and civil society may give the court *prima facie* evidence leading to undesired indictments and impaired political survival. The aftermath of interethnic violence surrounding Kenya’s 2007–2008 election illustrates this danger. Kenya’s national commission to investigate atrocities had given Kofi Annan, the African Union mediator, a list of suspected perpetrators and a box of evidence soon after the crisis. When the Kenyan

³⁹Rome Statute, Article 25.

⁴⁰The Rome Statute codifies co-perpetration in Article 25(3)(a). Wirth 2012, Sadat and Jolly 2014, and Lachezar 2018 examine the theory as applied at the ICC. The ICTY crystallized joint criminal enterprise in *Prosecutor v. Delalić et al.*, Judgment, Case No. IT-96-21, Trial Chamber, 16 November 1998 and *Prosecutor v. Tadić*, Judgment, Case No. IT-94-1, Appeals Chamber, 15 July 1999.

⁴¹Rome Statute Article 28; Danner and Martinez 2005

⁴²Sadat and Jolly 2014.

⁴³*e.g.* *Prosecutor v. Taylor*, Indictment, SCSL-03-01-I, 7 March 2003; *Prosecutor v. Milošević*, Second Amended Indictment, Case No. IT-02-54-T, 24 July 2004; *Prosecutor v. Milošević* Amended Indictment, Case No. IT-02-54-T, 23 November 2002; Mehlis, D.E. “Report of the International Independent Investigation Commission Established Pursuant to SC Resolution 1595 (2005),” S/2005/662, 20 October 2005; *Prosecutor v. Bemba*, Decision Pursuant to Article 61(7)(a) and (b) of the Rome Statute on the Charges of the Prosecutor Against Jean-Pierre Bemba Gombo, Case No. ICC-01/05-01/08, 15 June 2009.

parliament struck down legislation for a hybrid special tribunal to Annan's dissatisfaction, he gave evidence within to the ICC. The court promptly indicted Kenyan officials with deep ties to the president, Mwai Kibaki. It also indicted Kibaki-ally Uhuru Kenyatta for funding gangs to perform discriminatory violence. When Kenyatta emerged as the frontrunner of Kenya's 2013 general election, the United States and other Kenyan donors warned that there would be consequences if Kenyatta took office under indictment.⁴⁴

Autocratic obstruction of justice

To a greater or lesser extent, however, leaders can engage in obstructionism to shield themselves and their allies in government from prosecution. The ICC's ability to build a credible case for indictment or conviction is so dependent on state cooperation that Part 9 of the Rome Statute contains 17 articles concerning it. Leaders may be happy to submit evidence against their enemies, but will seek ways to suppress evidence inculcating them or their allies. Autocrats can use security forces or other agents to destroy or hide physical evidence—documents, weapons debris, graves—and to co-opt, intimidate, or kill witnesses. They may invoke the ICC's complementarity principle, which grants able-and-willing national courts priority to prosecute atrocities, to justify restraining ICC investigators from working *in situ*.

Histories of international criminal tribunals and ICC reveal grand obstructions of justice. It is well known that states often bury and rebury the massacred in secrecy to hide evidence of atrocity crimes. Serbian and Syrian security forces have been caught in the act.⁴⁵ Remote sensing techniques offer some hope of finding mass graves. But mere evidence of a mass grave's existence is thin evidence in an international criminal prosecution. Only the downfall of a regime reveals unambiguous evidence of the scope of the killings, the identities of the dead, and their manner of death.⁴⁶ Documentation of a plan for atrocities, when it exists, is easier to erase. The most striking example may be Imperial Japan's subterfuge during the two-week period between its 1945 surrender and the start of the Allied occupation and Tokyo War Crimes Tribunal. Gary Bass reminds us that "even in the best of circumstances, it is hard to prevent war criminals from destroying the evidence...Japanese militarists set off bonfires, destroying records of the secret police and military, transcripts of imperial conferences, cabinet deliberations, and records on prisoners of war and on campaigns in China."⁴⁷ The heavy dissents of tribunal judges Bernard V.A. Röling, Heri Bernard, and Radhabinod Pal on evidentiary and procedural defects, and Pal's insistence on mass acquittal must be viewed in light of this organized obfuscation.

With or without physical evidence, international criminal prosecutions rely crucially on witness testimony to prove the mental elements of atrocity crimes. Herein lies a serious weakness of international criminal courts. Political authorities co-opt, intimidate, and murder witnesses and their families. Court officials may be able to weed out false allegations against high-level indictees in cross-examination, but they can do little with witnesses who remain silent, make plausible

⁴⁴*The Star*, 8 February 2013.

⁴⁵*New York Times*, 31 July 2001; *The Guardian*, 5 March 1999; *New York Times*, 8 June 2012.

⁴⁶Stover, Haglund and Margaret 2003

⁴⁷Bass 2000, 303. Thousands of potential defendants or witnesses committed suicide.

denials, give inconsistent testimony, or refuse to take the stand. Surveys completed near the start of three ICC situations suggested a lack of confidence in the court's ability to protect its collaborators. Just 21% of Kenyan respondents thought that people cooperating with the court in April 2010 were "very safe," while a plurality of 37% thought that cooperators were "unsafe." In Uganda just 2% admitted knowing how to contact the court in 2007. In Democratic Republic of the Congo, just 12% admitted knowing how to contact the court in 2008.⁴⁸ Prior to vacating charges against high-ranking Kenyan defendants—including the president—because of Kenya's obstructionism, the ICC tried to safeguard Kenyan witnesses by helping them flee the country, offering them lifetime protection, and condemning sources of intimidation.⁴⁹ Yet witness protection programs at international criminal courts face are imperfect, as the Kenyan situation proves.⁵⁰

Finally, international criminal courts have engaged in battle over questions of jurisdiction and admissibility with openly uncooperative states, delaying investigations and trials for years in some cases. Serbia, Croatia, Lebanon, Rwanda, Democratic Republic of the Congo, and Uganda have refused to arrest or surrender suspects and witnesses, granting them domestic trials or amnesties instead. So has Libya, invoking the principle of complementarity in regard to revolution-related crimes and the ICC warrant for Saif al-Islam Gaddafi, and going as far as detaining ICC investigators seeking interviews with Libyans.⁵¹ The court faces similar problems with Uganda, which created a special branch of its judiciary to shield state authorities from prosecution, and with Kenya, which foreshadowed its present denial of an official hand in obstructionism with the view that the ICC should defer to Kenyan courts, tribunals, and commissions.⁵² These deadlocks matter. Stalling prosecutions does not merely postpone indictments and convictions. It may prevent them from ever occurring. The evidence sought by the international criminal courts decays. Potential targets of investigation gain time to destroy residual physical evidence, and the credibility of witness testimony reaches a vanishing point as delays mount.⁵³

Electoral turnover and exposure to prosecution

Obstruction of international criminal investigations is not always possible or sustainable. Leaders likely to be replaced in elections by competitors who are not political allies will be unable to insulate themselves and their accomplices for long.⁵⁴ The exposure effect of accepting the court's jurisdiction should be greater in states with institutionalized and peaceful electoral turnover between competing factions at the highest level of office for two reasons. First, an arbitrary set of

⁴⁸2007/8 surveys of the Human Rights Center at University of California, Berkeley; *Synovate Pan-Africa*, April 2010.

⁴⁹*Citizen News*, 17 February 2013; *The Star*, 11 July 2011.

⁵⁰Intimidation and murder of witnesses in the trial of former Kosovo Liberation Army commander and prime minister of Kosovo, Ramush Haradinaj may have produced his acquittal at the ICTY. *New York Times*, 21 July 2010; *New York Times*, 29 November 2012. The Hague courtroom of the Special Tribunal of Lebanon is windowless to prevent missile attacks. Several of the court's witnesses and one investigator have been murdered. *New York Times Magazine*, Feb. 10, 2015.

⁵¹*New York Times*, 12 April 2012; *New York Times*, 21 March 2012; *New York Times*, 9 June 2012.

⁵²Sriram and Brown 2012

⁵³Combs 2010, 14–16

⁵⁴This view of democracy is closer to that of Dahl 1971 than to more minimalist conceptions emphasizing *de facto* leadership turnover under stable electoral laws.

voters and civil society organizations will always prefer to unseat obstructionists. Obstructionism perpetuates the grievances of multiple groups: those aggrieved by atrocities, those aggrieved because they value the rule of law, and those aggrieved because they prefer their government to comply with international law. If these groups are large and influential, and if the political opposition can turn their support into elections victories, then obstructionism will quickly come to an end. Second, exogenous factors matter. Errors of policy, term limits, recessions, and scandals are more likely to force an incumbent from office in a state that at minimum operates as an electoral democracy than elsewhere. Regardless of whether obstructionism itself causes their downfall, obstructionist leaders should be more prone to exit office in such states. When they do exit, they lose control of the effective means of obstruction—and thus, an insurance policy on their viability as future heads of state.

Lessons of obstructionism and its defeat by electoral turnover in Serbia

The downfall of Vojislav Koštunica illustrates how democratic turnover can expose a leader's protégés to international prosecution.⁵⁵ As Serbia's president and then its prime minister, Koštunica fought jurisdictional battles with the ICTY, a court whose need for state cooperation and vulnerability to obstructionism matches the ICC's, but which SC Resolution 827 made inescapable for Serbia.⁵⁶ He also cooperated with the military to hide several high-level indictees with intimate knowledge of who in Serbia had organized brutal massacres in Croatia and Bosnia and Herzegovina. The most prominent of these figures were Radovan Karadžić, Ratko Mladić, and Goran Hadžić.

Koštunica suffered a series of setbacks beginning in 2006. First, the European Union (EU) halted accession talks in frustration with Serbia's obstructionism. The Serbian Justice Ministry also prosecuted eleven of Mladić's handlers, who publicly revealed where they took their orders from. Second, Koštunica made a fateful decision to oppose Europe's support for Kosovo's independence, vowing to withdraw Serbia's bid for EU membership and develop closer relations with Russia instead.

These developments precipitated a crisis in 2008. Serbia's president, Boris Tadić and his pro-EU Democratic Party dissolved a governing coalition with Koštunica's Democratic Party of Serbia and scheduled a parliamentary election. Koštunica's party lost 13 seats in the election, but its alliance with the Serbian Radical Party meant that a second alliance with the pivotal Socialist Party could give the coalition a parliamentary majority. The socialists had abandoned their party's initial hostility toward the ICTY and Europe by the middle of the decade. They sided with Tadić after his party won the plurality of seats with nearly 39% of the popular vote. This put Koštunica's party and the radicals in the minority, and Koštunica out of a job.

⁵⁵Koštunica was president (2000–2003) and prime minister (2004–2008). Peskin 2008; *New York Times*, 26 December 2006; *New York Times*, 8 June 2007; *New York Times*, 7 February 2008; *New York Times*, 23 July 2008; *The Economist*, 12 May 2008; *New York Times*, 21 October 2010; *New York Times*, 20 July 2011.

⁵⁶The ICTY and ICC have distinct legal personalities, origins, jurisdiction, and liability theories. Yet each is established in hierarchically equivalent sources of law pursuant to Article 38 of the ICJ Statute. While great powers were more intent on establishing the ICTY than the ICC, they have been equally equivocal in enforcing full compliance with state-court obligations.

Koštunica thus alienated the EU for multiple reasons—some related to his government’s obstruction of justice, some arguably not—destroying the coalition that kept him in office. His career in high office ended amid the growing demand by centrist and leftist Serbian politicians for compliance with the ICTY, which Europe had made a prerequisite for cooperation on trade and immigration. Serbia’s new government began dismantling the state’s obstructionist agenda upon entering office. It rapidly captured Karadžić hiding in Belgrade in July of 2008 and surrendered him to the ICTY. It next hunted down Mladić and Hadžić, surrendering them by 2011.

Neither this sequence of events, nor Serbia’s prior non-cooperation with the tribunal would have gone ignored by incumbent and aspirant leaders between 2000 and 2008—a period spanning the birth and first steps of the ICC. Autocrats would have first noticed that even Serbia’s leaders, reliant on re-election, were able to protect the higher echelons of international criminals on its territory for a while. They would have recognized the ease with which they could replicate this. Autocrats and democrats alike would have observed the fragmentation of this practice and delivery of justice as elections replaced the Serbian leadership. They all would have been able to contrast it with the not-so-puzzling protection of regime insiders that Rwanda’s Paul Kagame was able to afford while the Arusha tribunal labored. And they would have seen the focus put by wealthy democracies on cooperation with at least one of the tribunals. It must have been a moment for understanding both the circumstances that can influence personal exposure to an international court, and the concern that democratic states had for punishing international criminals.

Hypotheses

A theory in which leaders trade off the security and exposure effects of the court’s jurisdiction when choosing whether to accept it has three straightforward predictions. First, a greater receipt of publicly financed aid and loans from wealthy democracies increases both the security and exposure effects in democracies with effective electoral turnover, where leaders are less likely to be able to obstruct prosecutions. Both effects increase, because a potential ICC prosecution is more costly to leaders and their enemies alike when eligibility for foreign money is a prerequisite for gaining and maintaining a position of leadership. Autocrats, by contrast, will be more likely to succeed at obstructing unwanted prosecution. A greater receipt of foreign money will therefore increase the security effect relative to the exposure effect for autocrats, but not for democrats. The implications are that only autocrats should become more likely to accept the court’s jurisdiction as disbursements increase, and that the security effect should manifest itself for autocrats but not for democrats in the form of longer tenures and less anti-regime violence.⁵⁷

HYPOTHESIS 1: Publicly funded aid and loans from wealthy donor democracies increase the probability that an autocrat accepts ICC jurisdiction, but has no such effect on a democrat.

HYPOTHESIS 2: The ICC’s jurisdiction reduces the probability that an autocrat loses office, but has no such effect on a democrat.

⁵⁷The exposure/security trade-off may have determined the choice in some democracies, too—*e.g.* where democrats faced violent domestic enemies but had kept “clean hands,” and therefore faced a low exposure to prosecution.

HYPOTHESIS 3: The ICC’s jurisdiction reduces the anti-regime violence experienced by an autocrat but not by a democrat.

EMPIRICAL ANALYSIS

The hypotheses are tested on a panel of 788 leaders in 171 states from 1998 through 2017.⁵⁸ The data set excludes the wealthy donors of the OECD DAC, whose motives in accepting the ICC’s jurisdiction are beyond article’s scope. It includes small states sometimes omitted from cross-national studies for lack of data. Missing data on all covariates are multiply imputed.⁵⁹ The Supplementary Appendix contains over-imputation plots demonstrating the model’s close fit to the observed data. The inference is that, in this context, imputation will reduce bias and increase precision over listwise deletion, insofar as the pattern of missingness is independent of the values of missing observations. Findings based on listwise deletion are broadly consistent with those based on imputation. The details, data sources, and imputation procedure are described in the appendix.

Dependent and independent variables

Tests of Hypotheses 1–3 rely on three dependent variables. The first measures a leader’s decision to accept ICC jurisdiction through ratification, accession, or (rarely) indefinite acceptance by declaration (*Leader accepts*). The second indicates whether a leader exits office (*Leader exits office*) in any manner. The third measures anti-regime violence. Following other studies of the impact of international institutions on anti-regime violence, this is operationalized with the log number of battle deaths resulting from conflicts of a civil nature, and it includes deaths in civil conflicts having an international dimension (*Battle deaths*).

The theory’s procedural concept of democracy is operationalized as a dichotomous measure of whether a state has *de jure* and *de facto* multiparty elections for its executive and legislative branches, as well as moderately high freedoms of suffrage, association, and expression theorized to make elections meaningful. This binary measure (*Democracy*) codes “closed and electoral autocracies” as zero in contrast to “electoral and liberal democracies,” relying on the methodological framework of Varieties of Democracy Version 8.⁶⁰

A state’s reliance on capital publicly financed by wealthy democracies is measured in four ways: concessional aid disbursed by the democratic members of the DAC and European Commission (*DAC-EC Aid*), multilateral concessional aid disbursements (*Multilateral aid*), multilateral nonconcessional debt outstanding and disbursed (*Multilateral loans*) from international banks where they enjoy large voting blocs and *de facto* influence, and the sum of the above (*Total aid and loans*).⁶¹ Each measure of capital flows is measured annually, divided by a state’s gross

⁵⁸DAC members as of 2010 were Australia, Finland, Italy, South Korea, Austria, France, Luxembourg, Spain, Belgium, Germany, Netherlands, Sweden, Canada, Greece, New Zealand, Switzerland, Denmark, Ireland, Norway, United Kingdom, Japan, Portugal, and the United States.

⁵⁹Honaker and King 2010

⁶⁰Lührmann, Tannenber and Lindberg 2018

⁶¹The OECD labels concessional aid as “official development assistance” and all other nonconcessional disbursements as “other official flows.” Only measures of the former are consistently collected and published by the OECD at a fine-grained level.

domestic production (*GDP*), and scaled by the inverse hyperbolic sine transform.⁶²

Wealthy democracies channel a large sum of development capital bilaterally or through organizations under their exclusive control such as the European Commission. They also channel aid through multilateral agencies where they have majority power: the International Development Agency (IDA) of the World Bank, the Poverty Reduction and Growth Facility (PRGF) of the International Monetary Fund (IMF), and regional entities: the Council of Europe Development Bank (CEB), Inter-American Development Bank (IaDB), African Development Bank (AfDB), and Asian Development Fund (AIDF). Each agency finances operating costs and disbursements with member-guaranteed bonds and member contributions, which determine voting power.⁶³ The sum of gross aid from these sources yields a single measure of aid from banks under the substantial control of wealthy democracies (*Multilateral aid*).

These same states also control a large amount of nonconcessional credits and loans to promote development, structural adjustment, and the resolution of debt and currency crises. The concentration of this function in the Bretton Woods organizations is one of the most recognizable developments of politics since 1945. Loans and credits are publicly financed, like aid. However, they are repayable and typically bear interest in the vicinity of market rates of debt issued by private entities. *Multilateral loans* records such “debt outstanding and disbursed” by the IMF and the relevant World Bank vehicles: the IDA and International Bank for Reconstruction and Development (IBRD).⁶⁴

The binary independent variable used to test Hypotheses 2–3 is *ICC jurisdiction*, indicating the state’s continuing acceptance of the court’s jurisdiction.

Possible confounding relationships

Hypothesis 1

The relationship between publicly financed money and consent to the court’s jurisdiction may be confounded in a number of ways. Aid has been channeled to lesser-developed countries since the “McNamara Revolution” of the 1970s,⁶⁵ so controlling for development would be crucial if such states accept the ICC’s jurisdiction to tie their hands—a reasonable conclusion given the weakness of courts in poor states. It has also been posited that states with a strong rule of law, a peaceful history in the decade preceding the ICC, and European legal traditions were the quickest to accept the ICC’s jurisdiction.⁶⁶ Since the end of the Cold War, however, donors reoriented disbursements toward promoting good governance, including democracy, the rule of law, and post-

⁶²This scaling attenuates right-skew, which can exacerbate rare-data leverage and mask detection of non-linear relationships.

⁶³Eighteen of the AfDB’s 77 members are DAC members (as of 2010), holding 43.8% of the AfDB’s 2011 vote share. Twenty of the AsDB’s 67 members are, holding 56.5% of organization’s 2011 vote share. The same democracies control voting blocs through command of voting shares or participating certificates at The World Bank, IMF, IaDB, and CEB. Vote shares are available at the banks’ websites.

⁶⁴States and regional development banks also disburse nonconcessional funds, but these data are poorly recorded and unpublished at the recipient level.

⁶⁵Easterly and Pfutze 2008, 42

⁶⁶Kelley 2007, Mitchell and Powell 2011, Chapman and Chaudoin 2012

conflict stability.⁶⁷ Donors indeed withhold aid from highly repressive states,⁶⁸ but states are also theorized to select into human rights treaties based on their propensities for repression.⁶⁹

Tests of Hypothesis 1 control for these confounding factors with binary indicators of legal tradition (*Common*, *Islamic*, and *Mixed*), V-Dem's *Rule of law* measure, a dummy indicating a state's participation of an armed civil conflict from 1988 to 1998 resulting in at least 25 battle deaths (*Prior conflict*), and a state's mean score on the Political Terror Scale from 1988 to 1998 (*Prior repression*). Two proxies for economic development are used: log *GDP* and a state's median infant mortality rate between 1945 and 1998 (*Prior infant mortality*). 14 regional and 20 yearly fixed effects proxy for certain unobserved confounds.

Hypotheses 2–3

It is possible that autocrats with the greatest expected tenures and capacities to deter anti-regime violence (secure autocrats) accept the court's jurisdiction in part to improve their reputations or gain material rewards, knowing that their regime stability limits their exposure to prosecution. To limit possible selection biases, one could control for country-specific fixed effects capturing unobserved sources of a leader's security. This is one approach taken in tests of Hypotheses 2 and 3. Covariates from tests of Hypothesis 1 are also controlled for. These may influence political survival and anti-regime violence, and in any event, are causally prior to the establishment of jurisdiction. Extra variables theorized to influence political survival or anti-regime violence are controlled for, too. Data sources are in the appendix.

Turning to Hypothesis 2, leaders who are more secure in office are distinguishable in several ways. The ability to fund public and private goods through a sound macroeconomy and the “unearned income” of resource rents and foreign capital is posited to promote a leader's tenure.⁷⁰ A history of civil conflict and repression may generate grievances and collective action within dissident communities, parties, and the bureaucracy that destabilize leaders.⁷¹ Ongoing civil and interstate wars, which are costly bargaining failures and risks to regime survival, imperil the survival of autocrats and democrats, albeit in different ways.⁷² The resolution of credible commitment problems involving elites and the public—arguably evident in perceptions of a strong rule of law—are theorized to promote autocratic survival.⁷³ In testing Hypothesis 2 a reasonable number of operationalizations of these confounds are controlled for: *GDP growth*, *GDP*, *Prior infant mortality*, *Unemployment*, log *Oil rents*, *Total aid and loans*, *Prior repression*, *Prior conflict*, *Civil conflict*, *Interstate war*, and *Rule of Law*.

With regard to Hypothesis 3, the relevant literature is framed around the idea that grievances and opportunity fuel anti-regime violence. Among the many sources of grievances are poverty,

⁶⁷Dunning 2004

⁶⁸Nielsen and Simmons 2013

⁶⁹Simmons 2009, Hollyer and Rosendorff 2011

⁷⁰Morrison 2009, Ahmed 2012, Burke 2012, Ross 2015. Economic development and growth may also be consequences of autocratic stability. Magaloni and Kricheli 2010, 130.

⁷¹Svolik 2012

⁷²Chiozza and Goemans 2011

⁷³Gehlbach, Sonin and Svolik 2016, 572-75

the unhealed wounds and residual mobilization structures of prior repression and civil conflict, insecurity of person and property arising from judicial and bureaucratic corruption, and polarization between ethnic or religious groups.⁷⁴ The opportunities for rebellion depend on the balance of power between the state and its enemies. Major factors influencing this balance are the state's military and economic strength, the ability of rebels to mobilize soldiers and supporters among aggrieved populations, and the suitability of local geography for insurgency.⁷⁵ A reasonable number of operationalizations of these confounds are controlled for in tests of Hypothesis 3: *GDP growth*, *GDP*, *Prior infant mortality*, *Prior conflict*, *Prior repression*, *Rule of Law*, *Ethnic polarization*, *Religious polarization*, log *Military expenditures* as a percent of GDP, log *Military personnel* as a percent of the labor force, log *Oil rents*, *Total aid and loans*, *Male youth unemployment*, the log *Refugees* in the state, the state's log *Population*, and terrain *Ruggedness*.

Testing Hypothesis 1

Hypotheses 1 is tested by event history logit as follows.⁷⁶

$$Leader\ accepts \sim \text{Bernoulli}(\text{logit}^{-1}[\alpha_r + \gamma_t + d_{kt} + X\beta + Z\delta]) \quad (1)$$

A leader's choice to accept ICC jurisdiction in a given quarter is a function of independent variables X , controls Z , and a duration dependence term d_{kt} for t quarters of incumbency in country k since the opening of the Rome Statute. Fixed intercepts for 14 regions and 20 years are specified as α_r and γ_t . The term $Z\delta$ is a linear additive function of the control variables *Common*, *Mixed*, *Islamic*, *Rule of law*, *GDP*, *Prior conflict*, *Prior repression*, and *Prior infant mortality*. The term $X\beta$ includes an interaction between *Democracy* and one of four measures of capital receipts from rich democracies. Estimates are corrected for biases arising in models with limited dependent variables measuring rare events.⁷⁷

The motivation for regional and annual intercepts is to account for the heterogeneity common in cross-national data. Regional phenomena such as diplomatic conferences, development programs, economic conditions, and conflicts may result in correlations in leaders' propensities for accepting ICC jurisdiction. So may global phenomena like the entry into force of the Rome Statute, activities of the ICC prosecutor and chambers, and attitudes of influential states toward the court. These unobserved effects may bias parameter estimates.⁷⁸

Two kinds of duration dependence terms are specified. One set of models specifies a shared term across all countries (a shared baseline hazard) modeled by cubic polynomials. This is conventional in event history logits. The second set of models specifies country-specific terms (country-specific hazards) using cubic polynomials. The diverse dispositions of leaders,

⁷⁴Regan and Norton 2005, Montalvo and Reynal-Querol 2005, Lacina 2006, Blattman and Miguel 2010

⁷⁵Miguel, Satyanath and Sergenti 2004, Gleditsch and Salehyan 2006, Lacina 2006, Sobek 2010, Thies 2010, Blattman and Miguel 2010

⁷⁶Observations are dropped after *Leader accepts* = 1; models are equivalent to multiple-record Cox regression.

⁷⁷Kosmidis and Firth 2009

⁷⁸Unobserved heterogeneity may also generate violations of the proportional-hazards assumption in event history models. Box-Steffensmeier and Jones 2004, 141–142.

legislators, bureaucrats, and civil society toward international law, courts, and transnational activism surrounding the court may generate disparate rates of consent to jurisdiction. Country-specific hazards help account for this heterogeneity, also addressing concern about the proportional-hazards assumptions.

If Hypothesis 1 is correct, then the slope coefficient on each variable measuring development capital should be positive for autocracies, while no important effect should emerge for democracies. The first four rows of Table 1 display estimates from Equation 1 using the various measures of development capital. The estimates generally support the hypothesis. For autocracies, coefficient estimates are all positive and large. They are significant at the 10% level in seven out of eight models, including every model with country-specific hazard rates.⁷⁹

For democracies, by contrast, no clear pattern emerges. The effects of development capital are unstable. Estimates are negative and significant in three of four models estimated with a shared baseline hazard. But in models with country-specific hazards, they range from an insignificant -2.61 to a significant 4.66 . The estimate of *Total aid and loans* for democracies is insignificant when country-specific heterogeneity is modeled via the baseline hazard.

Figure 1 illustrates these findings using *Total aid and loans* as the independent variable. It graphs the expected probability that autocrats and democrats accept ICC jurisdiction across the variable's inner 99%-ile range.⁸⁰ The rising expected probability that an autocrat accepts ICC jurisdiction exceeds that of a democracy *Total aid and loans* reaches about 55% of GDP, a level of receipts experienced by 107 states coded as autocracies in certain years from 1998 to 2017.⁸¹ Autocrats with this level of aid have about a 1% expected probability of accepting jurisdiction per quarter. This small probability is nonetheless an increase over the predicted probability that an autocrat accepts ICC jurisdiction when receives zero aid or loans: essentially zero.

Robustness checks

Multiple regression with control variables is meant to rule out endogeneity as the source of an independent variable's posited causal effect. But the method can fail if moments and support of control variable distributions are markedly different for observations in different strata of the independent variable. In this scenario of imbalance or lack of overlap, inferences about effects can be highly sensitive to arbitrary modeling choices such as the functional form of the regressors and distributional assumptions in the estimator. A skeptical reader might object, for example, to modeling the controls linearly and assuming homogenous effects for states with and without histories of violent conflict.⁸² One can imagine any number of such objections.

Matching is a useful way to mitigate arbitrary concerns about model dependence, and it has

⁷⁹AIC statistics show that country-specific hazards produce better fit. One-tailed tests are applied to coefficients of capital variables for autocracies since a positive effect is hypothesized. Two-tailed tests are applied for democracies, since a null effect is hypothesized.

⁸⁰The model is based on Equation 1 estimated with a shared baseline hazard and shared intercept. Continuous (ordinal and nominal) controls are held at their means (modes).

⁸¹*e.g.* Afghanistan, Bosnia, Burkina Faso, Burundi, Chad, Honduras, Jordan, Gambia, Georgia, Côte D'Ivoire, Kenya, Liberia, Madagascar, Nigeria, Tajikistan, Tanzania, Tunisia, Uganda, Ukraine, Serbia, Venezuela, Zambia.

⁸²*e.g.* Simmons and Danner 2010

become common in the literature evaluating the effects of treaties on state behavior.⁸³ It works by first applying an algorithm to select a subset of all observations such that distributions of control variables are approximately balanced across strata of a binary treatment variable. One then proceeds with analysis as usual. Coarsened exact matching is the method applied here.⁸⁴ It forms a contingency table defined by all values of the coarsened control variables, discards observations in strata without variation in the treatment. Its advantage over related matching algorithms is to improve in-sample (rather than in-expectation) balance on the control variables as well as their multivariate nonlinearities, interactions, moments, quantiles, and co-moments.

Coarsened exact matching is applied to two data sets: one composed entirely of autocracies, the other entirely of democracies. The treatment on which observations are matched in each data set is *High aid* equaling one when *Total aid and loans* exceeds the sample median.⁸⁵ Matching dramatically improves overall balance to each data set, as shown in the appendix. Returning to Table 1, the bottom two rows report the results of Equation 1 fit on each matched data set. Consistent with Hypothesis 1, *High aid* has a positive and significant effect on the probability that autocrats accept jurisdiction. The effect for democrats is insignificant and has an unstable sign.

The robustness of the findings is further assessed in the Supplementary Appendix. First, the models in Table 1 are replicated using the unimputed data set with listwise deletion. Second, the models are replicated using a sample limited to capital-eligible states, omitting four states that received none of the measured capital from 1998 to 2017. The findings are consistent with those in Table 1.

⁸³Hill 2010, Hollyer and Rosendorff 2011

⁸⁴See Iacus, King and Porro 2011. Observations are weighted at analysis stages according to ratios of treated and controls per strata.

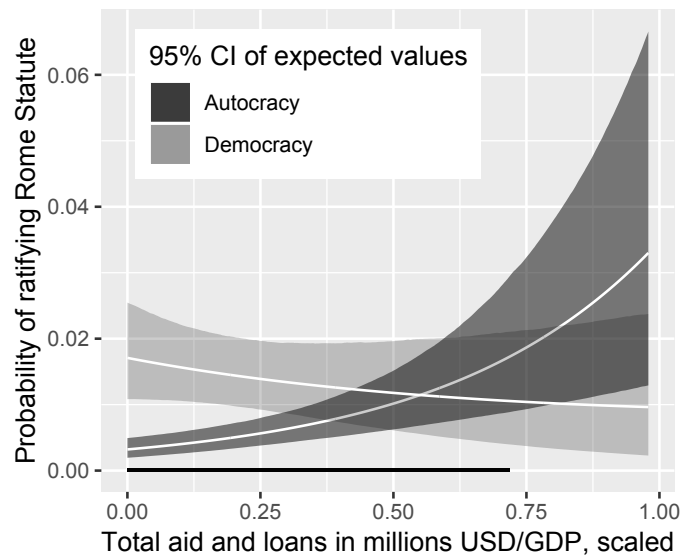
⁸⁵Sample medians of *Total aid and loans* are 6.4% and 6.3% of GDP for autocracies and democracies, respectively. Covariates are dichotomized at medians for matching.

Table 1: Effects of foreign capital on consent to ICC jurisdiction (1998–2017) using event history logit with fixed effects or matching

Data set	Parameter	Shared baseline hazard			Country-specific baseline hazards		
		Est	SE	p	Est	SE	p
All states <i>N</i> = 7919	Coef. of <i>DAC-EC aid</i> in autocracies	2.88*	1.85	0.06	4.53*	2.27	0.03
	Coef. of <i>DAC-EC aid</i> in democracies	−5.17*	2.46	0.04	−2.61	2.63	0.24
	Mean AIC	954			448		
All states <i>N</i> = 7919	Coef. of <i>Multilateral aid</i> in autocracies	2.35	3.10	0.15	6.89*	3.63	0.04
	Coef. of <i>Multilateral aid</i> in democracies	−13.60*	6.65	0.05	4.66*	2.61	0.08
	Mean AIC	956			454		
All states <i>N</i> = 7919	Coef. of <i>Multilateral loans</i> in autocracies	1.36*	0.83	0.05	3.63*	1.16	0.01
	Coef. of <i>Multilateral loans</i> in democracies	−1.17	0.95	0.19	1.70	1.74	0.25
	Mean AIC	956			448		
All states <i>N</i> = 7919	Coef. of <i>Total aid and loans</i> in autocracies	1.03*	0.70	0.07	2.93*	0.98	0.01
	Coef. of <i>Total aid and loans</i> in democracies	−1.41*	0.76	0.07	0.25	1.27	0.39
	Mean AIC	954			448		
Autocracies matched on <i>High aid</i> <i>N</i> = 3085	Coef. of <i>High aid and loans</i> in autocracies	1.10*	0.71	0.06	1.80*	1.40	0.08
	Mean AIC	210			78		
Democracies matched on <i>High aid</i> <i>N</i> = 1852.6	Coef. of <i>High aid and loans</i> in democracies	−0.50	0.41	0.19	0.42	0.59	0.31
	Mean AIC	380			200		

Note: P-values are one-tailed for autocracies, two-tailed for democracies. * indicates significance at 10%.

Figure 1: Effects of *Total aid and loans* on consent to ICC jurisdiction (1998–2017)



Note: Estimates are from Equation 1 with a shared baseline hazard and shared intercept. Black horizontal line shows the 95% interval of observed non-zero values of *Total aid and loans*, which is scaled by the inverse hyperbolic sine function. Axis covers the inner 99% interval.

Testing Hypothesis 2

Hypothesis 2 is tested in samples of autocrats and democrats separately using event history logits of the following form.

$$\text{Leader exits office} \sim \text{Bernoulli}(\text{logit}^{-1}[\alpha_k + \gamma_t + d_{kt} + \text{ICC jurisdiction} \cdot \beta + Z\delta]) \quad (2)$$

Fixed country-specific and annual intercepts capture unobserved confounds. The baseline hazard of exit (d_{kt}) is specified as shared in one set of models. In another set of models the hazard is country-specific to capture additional heterogeneity and address possible concerns about the proportional-hazards assumption. The term $Z\delta$ includes all of the independent and control variables in Equation 1 plus *GDP growth*, *Unemployment*, *Oil rents*, *Civil conflict*, and *Interstate war*.

If Hypothesis 2 is correct, then the coefficient on *ICC jurisdiction* should be negative for autocrats but statistically indistinguishable from zero for democracies. The coefficient estimates of *ICC jurisdiction* in Table 2 are in fact negative and significant for autocrats in each model specification, but near zero and insignificant for democrats.

Robustness checks

To assess robustness to model dependence, Equation 2 is refit to separate sets of autocracies and democracies produced by coarsened exact matching on *ICC jurisdiction*.⁸⁶ Again, matching dramatically improves overall balance. The coefficient estimates from these models in Table 2 offer an additional degree of support for Hypothesis 2. *ICC jurisdiction* has a strong negative effect on autocratic exit from office.⁸⁷ This is statistically significant in models with shared and country-specific baseline hazards. The sample average treatment effect on the treated (SATT) calculated from the model fit with country-specific hazards and imputed data implies that the ICC's jurisdiction decreased an autocrat's quarterly probability of losing office by 0.066 (SE = 0.022). This estimated causal effect is both substantively and statistically significant. The empirical rate at which all autocrats exited office from 1998-Q3 to 2017-Q4 was 0.034 (SD = 0.182) exits per quarter. For democrats, on the other hand, the effect of *ICC jurisdiction* on exit is null in all models.

The appendix reports similar findings in analyses using either listwise deletion or the sample confined to capital-eligible states. Some fragility emerges in the results when matching and listwise deletion are applied together to autocracies, where 16% of the data are lost to listwise deletion. Yet the effect of *ICC jurisdiction* remains substantively and statistically significant in all specifications with country-specific baseline hazards of exit.

⁸⁶Covariates are split trichotomously at the 1/3 and 2/3 percentiles for matching.

⁸⁷*e.g.* Autocratic Rome Statute non-party/party pairs are: Burundi's Domitien Ndayizeye in 2005 and Sierra Leone's Ahmad Kabbah in 1999; South Sudan's Salva Kiir in 2017 and Afghanistan's Ashraf Ahmadzai in 2016; or in Comoros, Azali Assoumani in 2006 and Ikililou Dhoinine in 2015.

Table 2: Effects of ICC jurisdiction on political survival (1998–2017) using event history logit with fixed effects or matching

Data Set	Parameter	Models with a shared baseline hazard			Models with country-specific baseline hazards		
		Est	SE	p	Est	SE	p
Autocracies <i>N</i> = 6644	Coef. of <i>ICC jurisdiction</i>	−1.03*	0.35	0.01	−1.39*	0.39	0.01
	Mean AIC	1846			1554		
Autocracies matched on <i>ICC jurisdiction</i> <i>N̄</i> = 775	Coef. of <i>ICC jurisdiction</i>	−1.00*	0.46	0.02	−1.67*	0.60	0.01
	Mean AIC	255.7			195.3		
Democracies <i>N</i> = 7132	Coef. of <i>ICC jurisdiction</i>	−0.21	0.23	0.26	0.02	0.25	0.40
	Mean AIC	2945			2543		
Democracies matched on <i>ICC jurisdiction</i> <i>N̄</i> = 2574.5	Coef. of <i>ICC jurisdiction</i>	−0.21	0.18	0.20	0.11	0.21	0.35
	Mean AIC	1160			955.6		

Note: P-values are one-tailed for autocracies, two-tailed for democracies. * indicates significance at 10%.

Table 3: Effects of ICC jurisdiction on battle deaths in civil conflict (1998–2017) using OLS with fixed effects or matching

Data set	Parameter	Models with a shared time trend			Models with country-specific time trends		
		Est	SE	p	Est	SE	p
Autocracies $N = 6644$	Coef. of <i>ICC jurisdiction</i>	-0.83*	0.10	0.01	-0.20*	0.08	0.01
	Mean Adj. R^2	0.67			0.82		
Autocracies matched on <i>ICC jurisdiction</i> $\bar{N} = 873.5$	Coef. of <i>ICC jurisdiction</i>	-0.86*	0.20	0.01	-0.57*	0.13	0.01
	Mean Adj. R^2	0.44			0.70		
Democracies $N = 7132$	Coef. of <i>ICC jurisdiction</i>	0.21*	0.04	0.01	0.01	0.03	0.37
	Mean Adj. R^2	0.79			0.88		
Democracies matched on <i>ICC jurisdiction</i> $\bar{N} = 2584.5$	Coef. of <i>ICC jurisdiction</i>	0.07	0.05	0.14	0.04	0.02	0.12
	Mean Adj. R^2	0.24			0.84		

Note: P-values are one-tailed for autocracies, two-tailed for democracies. * indicates significance at 10%.

Testing Hypothesis 3

The continuous dependent variable in tests of Hypothesis 3 is modeled by OLS as follows.

$$\text{Battle deaths} = \alpha_k + \gamma_t + d_{kt} + \text{ICC jurisdiction} \cdot \beta + Z\delta + \varepsilon. \quad (3)$$

Fixed country-specific and annual intercepts capture unobserved confounds. The term d_{kt} is a non-linear time trend in *Battle deaths* is specified to rule out spurious associations between the ICC's expanding jurisdiction and conflict patterns. Shared and country-specific trends are specified in different sets of models. The term $Z\delta$ include all of the covariates in Equation 1 plus *GDP growth*, *Male youth unemployment*, *Oil rents*, *Ethnic polarization*, *Religious polarization*, *Military expenditures*, *Military personnel*, *Refugees*, *Population*, and *Ruggedness*. The dependent variable is scaled to reduce right-skew arising due to the rarity of civil conflict and the intense warfare in Afghanistan, Syria, Yemen, and Iraq since 2001.⁸⁸

If Hypothesis 3 is correct, then the coefficient on *ICC jurisdiction* should be negative for autocrats but statistically indistinguishable from zero for democracies. Table 3 in fact reports large, negative, significant coefficient estimates for *ICC jurisdiction* among autocracies. Estimates for democracies are small, positive, and significant in models with a global trend in battle deaths, but essentially zero and statistically insignificant in models with country-specific trends in battle deaths.

Robustness checks

Table 3 reports the fitting of Equation 3 to separate sets of matched autocracies and matched democracies. Overall balance in the pretreatment covariates is dramatically improved. Hypothesis 3 again finds support. *ICC jurisdiction* has a large and negative effect in autocracies in all model specifications.⁸⁹ For democrats, the coefficient estimates are small and positive in the model with a shared time trend, but are insignificant in the model with country-specific time trends. The SATT estimated from models fit to matched autocracies with country-specific trends implies that the ICC's jurisdiction decreased the expected number of annual battle-related deaths related to civil conflicts by 47 (SE = 33) deaths.⁹⁰ For an empirical comparison, the mean number of such deaths from 1998-Q3 to 2017-Q4 in autocracies was 446 (SD = 3,121). Among autocracies under the court's jurisdiction, it was 384 (SD = 1,958).

The appendix reports similar findings from models using listwise deletion and models limiting analysis to capital-eligible states. The effects of *ICC jurisdiction* among autocrats are nullified when coarsened exact matching and listwise deletion are jointly applied. But in this scenario 66% of observations are lost due to missing data, casting doubt on the result.

⁸⁸The inverse hyperbolic sine function is used. It is similar to the logarithm but defined at zero. Fixed country-intercepts also address concerns about the magnitude of deaths in these states since 2011.

⁸⁹e.g. Autocratic Rome Statute non-party/party pairs are: Paul Kagame's Rwanda in 2009 and Yoweri Museveni's Uganda in 2008; Lesotho in 1998 and 2002 under Pakalitha Mosisili; Cote D'Ivoire in 2003 and 2006 under Gbagbo; or Tajikistan in 1998 and 2004 under Emomali Rakhmonov.

⁹⁰The raw count of battle deaths is set as the dependent variable for the model on which this estimate is based, instead of the scaled measure (where the SATT is larger).

A note on inference

Causal inference with non-experimental data always merits caveats about internal validity. The empirical strategy used here relies on assumptions of unconfoundedness and non-interference to estimate average treatment effects, as do synthetic control methods.⁹¹ Alternative strategies require assumptions too. Parametric selection estimators require distributional assumptions and correct functional forms mapping predictors to outcomes. Estimation with instrumental variables requires homogeneity, exclusion, independence, instrument strength, and monotonicity. Robustness under missing data scenarios is underexplored. Qualitative case analysis may suggest possible confounders and instruments. But it will never reveal unit-level counterfactuals—the unwitnessed politics in Cote D’Ivoire or Burundi absent the ICC’s jurisdiction, for example. Indeed, this is the motivation for a focus on average treatment effects.

CONCLUSION

An under-appreciated power of the ICC is the ability to derail political careers. ICC prosecutions credibly communicate personal guilt for atrocities by producing reliable evidence of guilt at great expense through judicial activity. In many cases this information is new, and may trigger leader-specific economic statecraft by capital-disbursing democracies. Accepting jurisdiction thus entails a security effect and an exposure effect. The security effect arises because jurisdiction impairs cooperation between actors who might otherwise organize anti-regime violence. The exposure effect arises because jurisdiction gives the court’s prosecutor the discretion to target a leader and his protégés. In states where leaders lack overriding ideological motives to accept the ICC’s jurisdiction, the trade-off between these two effects will be decisive: leaders will accept jurisdiction when they expect the security effect to outweigh the exposure effect. Two factors determining this trade-off are a reliance on capital publicly financed and disbursed by wealthy democracies, and electoral institutions exposing leaders to prosecution. The former increases both security and exposure, while the latter increases exposure by constraining obstructionism.

This article presents evidence consistent with that theory using statistical tests on leaders from 1998 to 2017. A greater reliance on foreign capital financed by wealthy democracies made autocrats more likely to accept the ICC’s jurisdiction, and the court’s jurisdiction prevented anti-regime violence and prolonged the tenure of autocrats. Democrats were subject to none of these effects. The ICC is a deterrent in the unwitting service of certain autocrats, and it has extended their rule by some margin.

The findings shed some light on the alternative theories of sovereign consent to the court’s jurisdiction. The negative impact of ICC jurisdiction on anti-regime violence in autocracies is consistent with the civil-peace theory, but none of the theories directly predict that the court’s jurisdiction lengthens autocratic tenure. The diffuse-reciprocity theory suggests that dependency on the capital of wealthy democracies would make all regime types more likely to accept jurisdiction—but this is unsupported by the evidence. Yet a broader theory might leverage the idea that the disbursing democracies and recipient autocracies know that the court is a back-stop for keeping

⁹¹ Abadie, Diamond and Hainmueller 2010

autocracies compliant with international law. Accordingly, autocrats might accept the court's jurisdiction in the hope of diffuse or specific reciprocity. The conditional effect of foreign aid and loans on state consent to jurisdiction is consistent with that theory. But other evidence is not. Wealthy democracies have emphasized domestic reform since the end of the Cold War, but empirically they neither make disbursements conditional on consent to the major rights agreements of the UN, nor reward consenting states in any way.⁹² A review of OECD DAC, World Bank, and IMF publications from this period fails to reveal a more focused agenda for Rome Statute ratification or accession.⁹³ Individual donors have diverse agendas, but the largest (the U.S.) tried to impair the court in its infancy, opposed it from 2001–2008, selectively engaged it from 2008–2016, and again announced opposition to the court in 2018 after the appointment of John Bolton as US National Security Advisor. It is possible that reciprocal international relations explain at least a fraction of this capital's effect on the acceptance of ICC jurisdiction. But at present the case is weak.

Returning to this article's argument, seven implications emerge. First, autocrats may find the ICC useful for precisely the reason that it endangers their careers. The court produces credible, actionable information and has leverage over foreign aid and lending. The court poses a non-negligible threat even to leaders who expect a low probability of a successful prosecution against themselves and their allies. This contrasts with two extant theories underscoring the limits of international law and institutions in promoting human rights and preventing atrocities. The *empty-promises theory* argues that international institutions promoting human rights law lack the means to punish non-compliance in the short term, and that leaders make empty promises to respect human rights to win good reputations. Yet another theory advanced by Eric Posner and others argues that the inflexible and ambitious authority of the ICC would prevent many states from accepting it.⁹⁴

Second, when corrupt or weak domestic institutions fail to credibly commit governments to humane governance and obedience to international law, they will also fail to constrain governments from sabotaging oversight by international institutions tasked with monitoring and enforcement. Credible commitment to international agreements requires domestic institutional arrangements that can constrain executive malfeasance. This insight implies scope conditions for the civil-peace theory, which argues that leaders may tie their hands to foster long-run domestic cooperation by delegating oversight to international institutions when domestic institutions are too corrupt or weak.

Third, centralizing the regulation of atrocity law in an international court will unevenly affect compliance. While James Morrow argues that interstate-war belligerents in fact regulate their conduct via decentralized reciprocal enforcement, Kenneth Abbott suggests why civil-war belligerents cannot adopt such a strategy: They lack long-term incentives to protect civilians, and the threat of reciprocal punishment often lacks credibility.⁹⁵ A centralized approach is sometimes seen as a solution to this problem—one that may civilize warfare to some extent. Michael Gilligan,

⁹²Nielsen and Simmons 2013

⁹³Keywords "ICC," "Rome Statute" and "international criminal justice" were searched for in reports and declarations available on these organizations' websites after 2000.

⁹⁴Posner 2009, Chapman and Chaudoin 2012

⁹⁵Morrow 2007, Abbott 1999

for example, argues that such an approach could deter atrocities *ex ante* by giving haven states a credible threat to deny sanctuary to deposed leaders whom the court wants to arrest.⁹⁶ Bolton and Jack Goldsmith, on the other hand, suggest that a centralized approach will fail without the cooperation of major powers—cooperation that is unlikely to materialize because of unacceptable exposures to prosecution.⁹⁷ This article argues that international capital flows and a court credibly communicating defendants’ guilt underpin a centralized approach to enforcing atrocity law. This approach will fail in some circumstances—specifically, when a state receives little money from wealthy democracies, or when its rulers can obstruct undesired prosecutions.

Fourth, whether an international court trades off peace in pursuit of justice depends on the nature of the threat that it uses to deter politicians from committing international crimes. If that threat is the court’s leverage over foreign money rather than imprisonment, then a mere indictment may provoke a significant increment of economic punishment by wealthy democracies, even though indictment alone reveals far less about leaders than a trial would. This insight contrasts with theories that, if an international court’s deterrent threat is imprisonment, then the court’s indictees will try to evade capture in ways that prolong conflicts that produce atrocities.⁹⁸ It also suggests that targets of prosecution, fearing the political fallout from indictments and convictions, might make public appeals to a justice-versus-peace trade-off when none exists.

Fifth, this article prompts us to revisit Judith Shklar’s apology for the International Military Tribunal at Nuremberg.⁹⁹ Shklar acknowledged the tribunal’s alleged injustices—its one-sidedness and disregard of *nullum crimen sine lege*—but defended the tribunal for substituting legalism for the extrajudicial disposal of foes, promoting liberalism, and establishing jurisprudence on crimes against humanity. In comparison, the injustice of the ICC is that certain autocrats under its jurisdiction are manipulating a liberal vision of the international rule of law to entrench their rule. The apology for the court is twofold. Autocrats must expose themselves to prosecution insofar as their past actions dictate, and the court may deter their enemies from atrocity-generating struggles. This realization softens the irony that in spite of numerous controversies, one persistent consensus among states in every major drafting committee and diplomatic conference on the Rome Statute was that the statute should grant self-referral rights to “interested” states of territory and nationality.¹⁰⁰

Sixth, the theory developed here suggests how to understand the reversal of threats to exit the Rome Statute by leaders of Kenya, Uganda, South Africa, and Gambia. These leaders may know that their hands are clean. If their hands are unclean, they may realize that the circumstances of executive replacement in their states still grant them the insulation which they or their predecessors understood existed upon first consent to jurisdiction. In either event, they may see the court as a still-useful deterrent to rivals and their foreign patrons. Alternative explanations are unpersuasive. Refusing to surrender a foreign head of state to the ICC—a decision made by

⁹⁶Gilligan 2006

⁹⁷Bolton 2001, Goldsmith 2003

⁹⁸Hencken Ritter and Wolford 2012, Prorok 2016

⁹⁹Shklar 1964

¹⁰⁰Robinson 2011

South Africa, Nigeria, and other states vis-à-vis the Sudanese president—is compliant with the proper contextual interpretation of the Rome Statute in light of the customary obligation to respect sovereign immunity.¹⁰¹ The obligations do not clash. A state’s naive interpretation of the treaty to the contrary is likely a pretext to justify treaty suspension or exit, if the need were to arise. The fact that self-referrals and the Security Council triggered the preponderance of the court’s Africa situations suggests that state rhetoric of prosecutorial anti-Africanism was disingenuous.

Paradoxically, the trend of authoritarian resurgence in recent years may have reinforced the *de jure* jurisdiction of the court while limiting its *de facto* jurisdiction over certain persons. The insistence of states in the twentieth century to confine the concept of criminality to natural persons¹⁰² has been reincarnated in a *de facto* exclusion of criminality for persons bearing the personality of the state—and their protégés. A troubling prediction follows. The difficulty of timely escape from the court’s jurisdiction means that the attention of the court’s prosecutor on leaders of unconsolidated democracies that are party to the Rome Statute could very well motivate those elected leaders to find ways to undermine electoral turnover. That has the potential to nullify the court’s dividends of peace and justice in the long run.

A question beyond the scope of this article is whether there is a politically viable legal remedy for states with the interest in punishing such abuse of rights but without the economic means to do so or conclusive proof of obstructionism. The answer is almost certainly yes: by invoking the international responsibility of offending states under the *erga omnes partes* character of Rome Statute obligations evident in the treaty’s preamble.

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¹⁰¹Tladi 2015

¹⁰²Crawford 2010

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Supplementary Appendix



Autocratic Consent to International Law: the Case of the International Criminal Court’s Jurisdiction, 1998–2017

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Contents

1	List of files required for replication	3
2	Guide to variables used in regressions appearing in the article	4
3	Tables of summary statistics for imputed and original data sets	6
4	Tables of balance statistics for the matched data sets used in regressions appearing in the article	8
5	Analyses the un-imputed data set with listwise deletion	11
5.1	Missing data fractions by variable and a note about the multiple imputation model’s fit . . .	11
5.2	Models predicting acceptance of ICC jurisdiction with listwise deletion	13
5.3	Models of leader exit with listwise deletion	14
5.4	Models of battle deaths in conflicts of a civil nature with listwise deletion	16
6	Details of the multiple imputation model used to produce data sets that regressions in the article are fit to: Multivariate Normal imputation for time-series cross-sectional data	18
6.1	Specifics of the model	18
6.2	Extra variables entering the imputation model as predictors but not used afterward	18
6.3	Cross-sectional regions used in the multiple imputation and as fixed effects in regressions appearing in Table 2 of the article	20
6.4	Fit diagnostics for the multiple imputation model using overimputation plots	22
7	Alternative specifications of models reported in the article using the multiply imputed data set	25
7.1	Slimmed models with no control variables	25
7.2	Tests of Hypotheses 1, 2, and 3 using a data set limited to only capital-eligible states, 1998–2017	28

List of Tables

1	Names and sources of variables	4
4	Summary statistics on the original data before imputation, 1998–2017	6

3	Summary statistics on the multiply imputed data, 1998-2017	7
5	Table of balance statistics: control variable means and N for regressions in the main article's models of <i>Leader accepts</i> reported in Table 1, matching on <i>High aid</i>	8
6	Table of balance statistics: control variable means and N for models of <i>Leader exits office</i> in the main article's Table 2, matching on <i>ICC jurisdiction</i>	9
7	Balance statistics for models of log <i>Battle deaths</i> in the article's Table 3, matching on <i>ICC jurisdiction</i>	10
8	Fraction of observations which are missing on each variable observed for 195 countries, 1998-2017	12
9	Effect of foreign capital on jurisdiction acceptance in autocracies and democracies: slope coefficient estimates from event history logits with listwise deletion for missing observations, 1998-2017	13
10	Effect of the ICC's jurisdiction on the probability that democrats and autocrats exit office: slope coefficient estimates from event history logits with listwise deletion for missing observations, 1998-2017	15
11	Effects of the ICC's jurisdiction on the scaled number of annual battle deaths in conflicts of a civil nature: slope coefficient estimates from OLS regressions with listwise deletion for missing observations	17
12	Fourteen-fold regional classification of states	20
14	Effect of <i>Total aid and loans</i> on whether a leader accepts ICC jurisdiction, 1998-2017: coefficient estimates from event history logits with no control variables	26
15	Effect of the ICC's jurisdiction on the probability that a leader exits office, 1998-2017: coefficient estimates from event history logits with no control variables	26
16	Effects of the ICC's jurisdiction on the scaled number of annual battle deaths of a civil nature, 1998-2017: slope coefficient estimates from normal regressions with no control variables	27
17	Effect of foreign capital on acceptance of ICC jurisdiction in capital-eligible autocracies and democracies, 1998-2017: slope coefficient estimates from event history logits	29
18	Effect of the ICC's jurisdiction on the probability that capital-eligible democrats and autocrats exit office, 1998-2017: slope coefficient estimates from event history logits	30
19	Effects of the ICC's jurisdiction on the scaled number of annual battle deaths in conflicts of a civil nature among capital-eligible states, 1998-2017: slope coefficient estimates from OLS regressions	31

List of Figures

1	Overimputation plots for independent variables and control variables that have a moderate to high fraction of missing observations in the 1998-2017 data set	24
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1 List of files required for replication

The files containing the data and code scripts for the article plus supplementary analyses will be available online after publication.

- `iccddata.RData` contains the un-imputed data.
- `iccimputations.RData` contains the imputed data used in the article.
- `Unimputed analyses Jan 2019.R` produces models run on the un-imputed data set with listwise deletion.
- `Imputation Jan 2019.R` processes the un-imputed data set and runs the multiple imputation model.
- `Summary statistics Jan 2019.R` produces summary statistics of the data set.
- `Jurisdiction models Jan 2019.R` produces the article's Table 1 and Figure 1 plus relevant portions of this appendix.
- `Exit models Jan 2019` produces the article's Table 2 plus relevant portions of this appendix.
- `Deaths models Jan 2019` produces the article's Table 3 plus relevant portions of this appendix.

2 Guide to variables used in regressions appearing in the article

Table 1: Names and sources of variables

Name in main article	Name in data files & scripts	Source
<i>Dependent variables</i>		
Leader accepts	ratifyrome	International Criminal Court 2018 (hereafter ICC). (1) Goemans, Gleditsch and Chiozza 2009 (hereafter Archigos), (2) websites of executive branches and embassies of the states, (3) Central Intelligence Agency of the United States of America 2019 (hereafter CIA).
Leader exits office	leaderexitsoffice	Uppsala Conflict Data Program Battle-Related Deaths Version 18.1 and Pettersson and 2018 (hereafter UCDP).
Battle deaths	logdeaths	
<i>Independent variables</i>		
DAC-EC Aid	demaidscaled	Organisation for Economic Co-Operation and Development International Development Statistics 2018 (hereafter OECD.Stat DAC2a)
Multilateral aid	multilataid.scaled	OECD.Stat DAC2a
Multilateral loans	dod.scaled	Sum of DT.DOD.MWBG.CD and DT.DOD.DIMF.CD at The World Bank 2019
Total aid and loans	capital.scaled	Ibid.
ICC jurisdiction	RomeStatuteRatification	ICC
<i>Control variables for models of jurisdiction</i>		
GDP	gdp.scaled	NY.GDP.MKTP.CD at The World Bank 2019
Rule of law	ruleoflaw	v2x rule indicator from Varieties of Democracy Version 8, July 2018. See also Coppedge et al. 2019.
Democracy	democracy	v2x regime indicator from Varieties of Democracy Version 8, July 2018. See also Lüthmann, Tannenber and Lindberg 2018.
Civil	civil	Central Intelligence Agency of the United States of America 2012 (hereafter CIAF) and Mitchell and Powell 2011.
Common	common	Ibid.
Mixed	mixed	Ibid.
Islamic	islamic	Ibid.
Prior civil conflict	pre98conflict	UCDP
Prior infant mortality	medianIMR	Abouharb and Kimball 2007
Prior repression	ptssum.mean	Gibney, Cornett and Wood 2010
Qtrs. of incumbency	quartersinoffice	Archigos, CIA
Qtrs. of incumbency since 1998(3)	quartersinoffice.since1998Q3	Ibid.
<i>Extra control variables for models of leader exit and battle deaths</i>		
Growth	loggrowth	NY.GDP.MKTP.KD.ZG at The World Bank 2019

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Unemployment	allunemp	SL.UEM.TOTL.ZS at The World Bank 2019
Male youth unemployment	maleunemp	SL.UEM.1524.MA.ZS at The World Bank 2019
Military expenditures (as % of GDP)	logexpmil	MS.MIL.XPND.GD.ZS and MS.MIL.TOTL.P1 at The World Bank 2019
Military personnel (as % of labor force)	logsoldierspercent	MS.MIL.TOTL.TF.ZS at The World Bank 2019
Population	logpop	SP.POP.TOTL at The World Bank 2019
Oil rents	logoilrent	NY.GDP.PETR.RT.ZS at The World Bank 2019
Refugees	refugees	SM.POP.REFG.OR at The World Bank 2019
Ethnic polarization	eth	Montalvo and Reynal-Querol 2005
Religious polarization	rel	Ibid.
Rugged terrain	rug	Nunn and Puga 2012. East Timor, Kosovo, Montenegro, South Sudan calculated using raster data in the <code>dismo</code> and <code>raster</code> libraries in R.
Civil conflict	x.ucdp.incidence	UCDP
Interstate war	cowwar	www.correlatesofwar.org (December 2018) and UCDP
<i>Baseline hazards and time-trends</i>		
Shared hazard of jurisdiction acceptance	ratify.single	Author's calculation
Country-specific hazard of jurisdiction acceptance	ratify.country	Author's calculation
Shared hazard of leader exit	exit.single	Author's calculation
Country-specific hazard of leader exit	exit.country	Author's calculation
Shared trend of civil war deaths	deaths.single	Author's calculation
Country-specific trend of civil war deaths	deaths.country	Author's calculation
<i>Observation ID variables</i>		
Quarter	quarter	
Year	year	
Leader identification code	lcode	Author assigned
State identification code	ccode	Sarkees and Wayman 2010
Region identification code	region	Author (see below in this file)

3 Tables of summary statistics for imputed and original data sets

Table 4: Summary statistics on the original data before imputation, 1998–2017

	Min	33rd	Med.	Mean	67th	Max	SD	MAD
<i>Dependent variables:</i>								
Leader accepts	0.00	0.00	0.00	0.01	0.00	1.00	0.11	0.00
Leader exits office	0.00	0.00	0.00	0.04	0.00	1.00	0.20	0.00
Battle deaths	0.00	0.00	0.00	1.02	0.00	11.84	2.47	0.00
<i>Independent variables:</i>								
DAC-EC Aid (% GDP)	0.00	0.00	0.02	0.05	0.04	1.15	0.09	0.02
Multilateral aid (% GDP)	0.00	0.00	0.00	0.01	0.01	1.00	0.04	0.00
Multilateral loans (% GDP)	0.00	0.01	0.03	0.08	0.08	1.23	0.13	0.05
Total aid and loans (% GDP)	0.00	0.02	0.07	0.14	0.15	1.81	0.19	0.10
ICC jurisdiction	0.00	0.00	0.00	0.43	1.00	1.00	0.50	0.00
Democracy	0.00	0.00	1.00	0.52	1.00	1.00	0.50	0.00
<i>Control variables:</i>								
GDP	3.27	9.30	10.14	10.25	11.08	27.77	2.54	2.11
Rule of law	0.02	0.30	0.48	0.48	0.64	0.98	0.28	0.34
Civil law	0.00	0.00	0.00	0.38	1.00	1.00	0.49	0.00
Common law	0.00	0.00	0.00	0.09	0.00	1.00	0.29	0.00
Mixed law	0.00	0.00	0.00	0.31	0.00	1.00	0.46	0.00
Islamic law	0.00	0.00	0.00	0.21	0.00	1.00	0.41	0.00
Prior conflict	0.00	0.00	0.00	0.38	1.00	1.00	0.49	0.00
Prior infant mortality	0.04	0.36	0.59	0.67	0.84	1.98	0.45	0.53
Prior repression	0.00	2.00	2.50	2.54	3.00	5.00	1.19	1.32
Civil conflict	0.00	0.00	0.00	0.16	0.00	1.00	0.36	0.00
Interstate war	0.00	0.00	0.00	0.00	0.00	1.00	0.06	0.00
Oil rents	0.00	0.00	0.00	0.88	0.59	4.85	1.39	0.00
GDP growth	-0.59	0.03	0.04	0.04	0.05	1.04	0.06	0.03
Population	9.84	15.48	16.34	16.04	17.15	21.74	2.22	1.98
Refugees	0.00	6.56	7.88	7.92	9.62	16.35	3.35	3.51
Military personnel (% labor force)	0.00	0.58	0.89	1.02	1.22	3.30	0.69	0.67
Military expenditures (% GDP)	0.00	0.53	0.90	1.13	1.34	17.54	1.17	0.83
Male youth unemployment	0.00	0.09	0.13	0.16	0.18	0.65	0.12	0.10
Unemployment	0.00	0.05	0.07	0.08	0.09	0.44	0.06	0.05
Ethnic polarization	0.02	0.43	0.59	0.56	0.68	0.98	0.23	0.24
Religious polarization	0.00	0.29	0.62	0.53	0.82	1.00	0.34	0.42
Ruggedness	0.00	0.53	0.95	1.38	1.64	6.74	1.37	1.04
Quarters of incumbency	0.00	8.00	15.00	28.47	27.00	328.00	36.28	17.79
Quarters of incumbency since 1998-Q3	0.00	6.00	12.00	16.93	19.00	77.00	16.60	13.34

Notes: SD is standard deviation and MAD is median absolute deviation. Skewed continuous variables are scaled by the inverse hyperbolic sine function, including measures of battle deaths, financial statistics, economic indicators, population, military statistics, and refugees. For reference, data are entirely non-missing for 95 states and 375 leaders. DAC member states as of 2010 are excluded.

Table 3: Summary statistics on the multiply imputed data, 1998-2017

	Min	33rd	Med.	Mean	67th	Max	SD	MAD
<i>Dependent variables:</i>								
Leader accepts	0.00	0.00	0.00	0.01	0.00	1.00	0.11	0.00
Leader exits office	0.00	0.00	0.00	0.04	0.00	1.00	0.20	0.00
Battle deaths	0.00	0.00	0.00	1.02	0.00	11.84	2.47	0.00
<i>Independent variables:</i>								
DAC-EC Aid (% GDP)	0.00	0.00	0.02	0.05	0.04	2.36	0.09	0.02
Multilateral aid (% GDP)	0.00	0.00	0.00	0.01	0.01	1.00	0.04	0.00
Multilateral loans (% GDP)	0.00	0.01	0.03	0.08	0.08	1.53	0.13	0.05
Total aid and loans (% GDP)	0.00	0.02	0.06	0.14	0.15	2.36	0.19	0.09
ICC jurisdiction	0.00	0.00	0.00	0.43	1.00	1.00	0.50	0.00
Democracy	0.00	0.00	1.00	0.52	1.00	1.00	0.50	0.00
<i>Control variables:</i>								
GDP	3.27	9.30	10.15	10.27	11.11	27.77	2.55	2.13
Rule of law	0.02	0.34	0.50	0.49	0.62	0.98	0.26	0.31
Civil law	0.00	0.00	0.00	0.38	1.00	1.00	0.49	0.00
Common law	0.00	0.00	0.00	0.09	0.00	1.00	0.29	0.00
Mixed law	0.00	0.00	0.00	0.31	0.00	1.00	0.46	0.00
Islamic law	0.00	0.00	0.00	0.21	0.00	1.00	0.41	0.00
Prior conflict	0.00	0.00	0.00	0.38	1.00	1.00	0.49	0.00
Prior infant mortality	0.04	0.36	0.59	0.67	0.84	1.98	0.45	0.53
Prior repression	-0.52	1.98	2.31	2.49	2.98	5.00	1.19	1.20
Civil conflict	0.00	0.00	0.00	0.16	0.00	1.00	0.36	0.00
Interstate war	0.00	0.00	0.00	0.00	0.00	1.00	0.06	0.00
Oil rents	-1.34	0.00	0.06	1.00	1.22	5.56	1.38	0.09
GDP growth	-0.59	0.03	0.04	0.04	0.06	1.04	0.06	0.03
Population	9.84	15.48	16.34	16.04	17.15	21.74	2.22	1.98
Refugees	-1.14	6.38	7.76	7.79	9.53	16.35	3.35	3.63
Military personnel (% labor force)	-0.97	0.72	1.05	1.18	1.45	4.52	0.76	0.80
Military expenditures (% GDP)	0.00	0.56	0.96	1.30	1.44	17.86	1.57	0.91
Male youth unemployment	0.00	0.10	0.15	0.20	0.21	0.78	0.17	0.12
Unemployment	0.00	0.05	0.07	0.13	0.11	0.73	0.16	0.06
Ethnic polarization	-0.10	0.44	0.55	0.54	0.65	1.05	0.20	0.20
Religious polarization	-0.15	0.36	0.52	0.51	0.69	1.20	0.29	0.36
Ruggedness	0.00	0.53	0.95	1.38	1.64	6.74	1.37	1.04
Quarters of incumbency	0.00	8.00	15.00	28.47	27.00	328.00	36.28	17.79
Quarters of incumbency since 1998-Q3	0.00	6.00	12.00	16.93	19.00	77.00	16.60	13.34

Notes: SD is standard deviation and MAD is median absolute deviation. Skewed continuous variables are scaled by the inverse hyperbolic sine function, including measures of battle deaths, financial statistics, economic indicators, population, military statistics, and refugees. Statistics are taken on 171 countries excluding the 23 OECD DAC members as of 2010, comprising 788 leaders (treating multiple tenures in office as distinct leaders).

4 Tables of balance statistics for the matched data sets used in regressions appearing in the article

Table 5: Table of balance statistics: control variable means and N for regressions in the main article's models of *Leader accepts* reported in Table 1, matching on *High aid*

	Matched data set		Full data set	
	Low aid	High aid	Low aid	High aid
<i>Autocracies only</i>				
L_1 statistic	0.54	0.65	0.25	0.75
GDP	10.00	9.80	12.00	9.40
Rule of Law	0.25	0.26	0.33	0.30
Common	0.00	0.00	0.03	0.03
Mixed	0.27	0.27	0.22	0.33
Islam	0.39	0.39	0.54	0.31
Prior Repression	2.80	2.80	2.80	2.90
Prior infant mortality	0.87	0.88	0.60	0.97
Prior conflict	0.51	0.51	0.30	0.62
\bar{N}	1555.8	1529.2	2545.5	2542.5
<i>Democracies only</i>				
L_1 statistic	0.45	0.72	0.24	0.75
GDP	9.40	8.30	12.00	8.10
Rule of Law	0.61	0.57	0.68	0.57
Common	0.15	0.15	0.22	0.12
Mixed	0.56	0.56	0.18	0.55
Prior repression	1.90	2.10	2.30	2.00
Prior infant mortality	0.47	0.58	0.43	0.62
Prior conflict	0.30	0.30	0.39	0.27
\bar{N}	831.1	1021.5	1417.7	1413.3

Note: The matching algorithm is coarsened exact matching. Statistics are taken on ten pooled copies of the multiply imputed data. The L_1 statistic is a multivariate frequency statistic calculated from the multidimensional histogram of the binned pretreatment covariates. It varies from zero to one. Maximum balance in these covariates is achieved by minimizing the difference between L_1 for treated and L_1 for control groups. See Iacus, King and Porro 2011a and Iacus, King and Porro 2011b for details on coarsened exact matching and the distance metric.

Table 6: Table of balance statistics: control variable means and N for models of *Leader exits office* in the main article's Table 2, matching on *ICC jurisdiction*

	Matched data		Un-matched data	
	No ICC jurisdiction	ICC jurisdiction	No ICC jurisdiction	ICC jurisdiction
<i>Autocracies only</i>				
L_1 statistic	0.34	0.33	0.21	0.32
Civil conflict	0.18	0.18	0.24	0.20
Interstate war	0.00	0.00	0.01	0.00
Oil rents	0.44	0.39	1.70	0.84
Unemployment	0.09	0.10	0.09	0.09
GDP growth	0.04	0.04	0.05	0.04
GDP	8.30	8.70	11.00	9.70
Rule of law	0.35	0.34	0.31	0.32
Total aid and loans	0.32	0.30	0.14	0.21
Common	0.03	0.03	0.03	0.05
Mixed	0.37	0.37	0.28	0.39
Islam	0.42	0.42	0.42	0.26
Prior conflict	0.50	0.50	0.46	0.52
Prior infant mortality	1.00	1.00	0.78	0.97
Prior repression	2.80	2.70	2.90	3.10
\bar{N}	390.50	384.50	5058.00	1586.00
<i>Democracies only</i>				
L_1 statistic	0.64	0.65	0.55	0.65
Civil conflict	0.02	0.02	0.13	0.05
Interstate war	0.00	0.00	0.00	0.00
Oil rent	0.47	0.48	0.49	0.62
Unemployment	0.20	0.19	0.19	0.16
GDP growth	0.03	0.03	0.03	0.04
GDP	9.90	10.00	10.00	10.00
Rule of law	0.69	0.70	0.62	0.68
Total aid and loans	0.10	0.09	0.16	0.10
Common	0.19	0.19	0.17	0.13
Mixed	0.23	0.23	0.37	0.29
Islam	0.05	0.05	0.06	0.04
Prior conflict	0.23	0.23	0.33	0.27
Prior infant mortality	0.41	0.41	0.53	0.53
Prior repression	1.70	1.70	2.20	2.10
\bar{N}	1039.90	1534.60	2760.00	4372.00

Note: The matching algorithm is coarsened exact matching. Statistics are taken on ten pooled copies of the multiply imputed data. The L_1 statistic is a multivariate frequency statistic calculated from the multidimensional histogram of the binned pretreatment covariates. It varies from zero to one. Maximum balance in these covariates is achieved by minimizing the difference between L_1 for treated and L_1 for control groups. See Iacus, King and Porro 2011a and Iacus, King and Porro 2011b for details.

Table 7: Balance statistics for models of log *Battle deaths* in the article’s Table 3, matching on *ICC jurisdiction*

	<u>Matched data</u>		<u>Un-matched data</u>	
	No ICC jurisdiction	ICC jurisdiction	No ICC jurisdiction	ICC jurisdiction
<i>Autocracies only</i>				
L_1 statistic	0.32	0.34	0.20	0.37
Military expenditures	0.53	0.61	1.20	0.85
Military personnel	1.20	1.00	1.40	0.92
Population	16.00	16.00	17.00	16.00
Ethnic polarization	0.50	0.49	0.53	0.54
Religious polarization	0.58	0.58	0.54	0.63
Ruggedness	1.50	1.40	1.50	1.30
Refugees	8.90	8.90	8.90	9.40
Oil rents	0.64	0.65	1.70	0.84
GDP growth	0.04	0.05	0.05	0.04
Male youth unemployment	0.15	0.15	0.16	0.16
GDP	8.60	9.00	11.00	9.70
Rule of law	0.34	0.33	0.31	0.32
Total aid and loans	0.28	0.28	0.14	0.21
Common	0.02	0.02	0.03	0.05
Mixed	0.44	0.44	0.28	0.39
Islam	0.30	0.30	0.42	0.26
Prior conflict	0.57	0.57	0.46	0.52
Prior infant mortality	1.00	1.00	0.78	0.97
Prior repression	2.90	2.90	2.90	3.10
\bar{N}	439.30	434.20	5058.00	1586.00
<i>Democracies only</i>				
L_1 statistic	0.56	0.64	0.48	0.70
Military expenditures	1.20	1.50	1.60	1.40
Military personnel	1.10	1.10	1.30	0.96
Population	15.00	15.00	15.00	16.00
Ethnic polarization	0.57	0.55	0.55	0.54
Religious polarization	0.47	0.46	0.44	0.48
Ruggedness	1.40	1.30	1.40	1.30
Refugees	5.70	6.30	6.50	6.80
Oil rents	0.45	0.51	0.49	0.62
GDP growth	0.04	0.04	0.03	0.04
Male youth unemployment	0.27	0.28	0.25	0.24
GDP	9.30	9.80	10.00	10.00
Rule of law	0.62	0.63	0.62	0.68
Total aid and loans	0.15	0.15	0.16	0.10
Common	0.16	0.16	0.17	0.13
Mixed	0.34	0.34	0.37	0.29
Islam	0.04	0.04	0.06	0.04
Prior conflict	0.30	0.30	0.33	0.27
Prior infant mortality	0.53	0.57	0.53	0.53
Prior repression	1.80	1.90	2.20	2.10
\bar{N}	1148.00	1436.50	2760.00	4372.00

Note: The matching algorithm is coarsened exact matching. Statistics are taken on ten pooled copies of the multiply imputed data. The L_1 statistic is a multivariate frequency statistic calculated from the multidimensional histogram of the binned pretreatment covariates. It varies from zero to one. Maximum balance in these covariates is achieved by minimizing the difference between L_1 for treated and L_1 for control groups. See Iacus, King and Porro 2011a and Iacus, King and Porro 2011b for details.

5 Analyses the un-imputed data set with listwise deletion

Studies of multiple imputation in medicine and international relations suggest that analyses using multiply imputed data will generally reduce bias and increase efficiency, since many of the incomplete data sets in these areas of study have missing observations are best categorized as “Missing at Random” rather than “Missing Completely at Random” or “Non-Ignorable” (White and Carlin 2010, Lall 2016). But there is no consensus on this point. Theoretical and simulation results show that listwise deletion returns unbiased slope parameters of regression models in many cases when data are Missing Completely at Random and Missing at Random. Also, when data are Non-Ignorable, multiple imputation may produce greater bias than listwise deletion (Pepinsky 2018). A new area of inquiry is how violations of the unverifiable parametric assumptions of imputation models might introduce new biases into estimates derived from imputed data sets (Arel-Bundock and Pelc 2018).

For the purpose of comparison and as a robustness check, this section of the appendix reports regressions run on the unimputed data set. Whereas the main article reports regression analyses on 10 copies of the multiply imputed data set, this appendix section reports the same analyses performed on the un-imputed data, with listwise deletion applied. Estimates of the control variables and fixed effects are suppressed, as in the main article, so that attention may focused on the independent variables of theoretical interest.

In the results that follow, listwise deletion results in a fairly large fraction of the observations being dropped from analyses due to missingness in the control variables. For the reader’s reference, some of the tables below therefore report slimmed versions of the regressions which omit all control variables but which include country and yearly fixed effects. These regressions, in which only a tiny fraction of observations are deleted, are reported alongside regressions including all control variables and fixed effects, in which many observations are deleted. All regressions using matched data sets include the full suite of control variables, since the control variables are used in obtaining the matched samples. Results from each table are briefly discussed in the subsections below.

5.1 Missing data fractions by variable and a note about the multiple imputation model’s fit

The fraction of observations missing for each variable used in regressions is reported in Table 8. Variables recording acceptance of ICC jurisdiction, battle deaths, leaders’ political survival, democracy, and financial flows are nearly completely recorded. Some of the control variables used in tests of Hypotheses 1–3 are missing in a large fraction of observations. These variables result in most of the observations lost to listwise deletion.

Readers interested in how well the imputation model used in the main article performs in imputing these highly incomplete variables may turn to section 6.4 of this document, where overimputation plots show that the model predicts observed entries of these variables with high accuracy. These diagnostics suggest that any discrepancy between results from models estimated with listwise deletion and models estimated with the multiply imputed data set is due to bias and inefficiency arising from listwise deletion—and not due to biases arising from the multiple imputation.

Table 8: Fraction of observations which are missing on each variable observed for 195 countries, 1998–2017

<i>Dependent variable names</i>	<i>Fraction of observations missing</i>
Leader ratifies	0.00
Battle deaths	0.00
Leader exits office	0.00
<i>Independent variable names</i>	<i>Fraction of observations missing</i>
ICC jurisdiction	0.00
DAC-EC Aid	0.00
Multilateral aid	0.00
Multilateral loans	<0.01
Total aid and loans	<0.01
High aid	<0.01
<i>Control variable names</i>	<i>Fraction of observations missing</i>
Quarters in office	0.00
Legal tradition variables	0.00
Prior conflict	0.00
Civil conflict	0.00
Interstate war	0.00
Rugged terrain	0.00
Prior infant mortality	0.00
Democracy	0.00
Population	0.01
GDP	0.03
GDP growth	0.04
Prior repression	0.04
Oil rents	0.06
Refugees	0.07
Unemployment	0.10
Male youth unemployment	0.10
Rule of law	0.11
Military personnel	0.20
Military expenditures	0.23
Ethnic polarization	0.31
Religious polarization	0.31

Note: The 23 OECD DAC member states as of 2010 are included here.

5.2 Models predicting acceptance of ICC jurisdiction with listwise deletion

Table 9 below replicates most of the article’s findings about the effects of various forms of aid from wealthy democracies on the likelihood that autocrats and democrats accept the ICC’s jurisdiction. Although estimate sizes differ, they generally match those reported in the main article. The major difference between these results and those in the main article concerns estimates for democracies. Here, only the coefficients of *Multilateral aid* exert statistically significant effects on the probability that leaders of democracies accept the ICC’s jurisdiction; as in the main article, these coefficients are negative for the model with a shared baseline hazard, and positive for the model with the country-specific hazards. These findings strengthen the support for Hypothesis 1: the effect of the independent variables should be positive for autocracies, but null for democracies.

In any event, it is useful to examine the amount of data was lost due to listwise deletion and complete-case analysis. List-wise deletion reduced the data set from 7,919 observations to 6,745 observations in these regressions: by about 14.8%.¹

Table 9: Effect of foreign capital on jurisdiction acceptance in autocracies and democracies: slope coefficient estimates from event history logits with **listwise deletion** for missing observations, 1998-2017

Data Set	Parameter	Models with a shared baseline hazard			Models with country-specific baseline hazards		
		Est	SE	p	Est	SE	p
Full <i>N</i> = 6745	Coef. of <i>DAC-EC Aid</i> in autocracies	3.31*	2.32	0.07	8.70*	2.19	0.01
	Coef. of <i>DAC-EC Aid</i> in democracies	-4.59	3.07	0.13	-3.99	5.03	0.29
	AIC	836			385		
Full <i>N</i> = 6745	Coef. of <i>Multilateral aid</i> in autocracies	1.66	3.48	0.18	5.37*	3.69	0.07
	Coef. of <i>Multilateral aid</i> in democracies	-15.50*	7.72	0.05	4.71*	2.78	0.09
	AIC	834			395		
Full <i>N</i> = 6745	Coef. of <i>Multilateral loans</i> in autocracies	0.99*	0.84	0.10	4.43*	1.12	0.01
	Coef. of <i>Multilateral loans</i> in democracies	-1.31	0.84	0.12	0.64	1.46	0.36
	AIC	835			388		
Full <i>N</i> = 6745	Coef. of <i>Total aid and loans</i> in autocracies	0.84*	0.74	0.10	4.06*	0.98	0.01
	Coef. of <i>Total aid and loans</i> in democracies	-1.30	0.82	0.11	0.80	1.50	0.34
	AIC	835			386		
Autocracies matched on <i>High aid</i> <i>N</i> = 2570	Coef. of <i>High aid</i> in autocracies	0.98*	0.69	0.07	2.03*	1.27	0.06
	AIC	218			79.9		
Democracies matched on <i>High aid</i> <i>N</i> = 1100	Coef. of <i>High aid</i> in democracies	-0.82	0.51	0.11	0.14	0.67	0.39
	AIC	210			121		

Note: List-wise deletion applied in the presence of missing data. Fixed effects are specified for 14 regions and 20 years. All measures of aid are continuous except for *High aid*, a dummy variable equaling one when *Total aid and loans* exceeds the sample 75th percentile. Control variables include *GDP*, *Rule of law*, *Common*, *Mixed*, *Islamic*, *Prior conflict*, *Prior repression*, and *Prior infant mortality*. Baseline hazards modeled with cubic polynomials as a function of the number of quarters a leader has been in office since the opening of the Rome Statute for ratification. P-values are one-tailed for autocracies, two-tailed for democracies. Significance at 10% level is indicated by *.

¹Recall that this data set consists only of leader-quarters after 1998 Q3 and until acceptance of the Rome Statute, as is required in event-history analysis.

5.3 Models of leader exit with listwise deletion

The findings in Table 10 display findings from the models of leader exit estimated separately on autocracies and on democracies, with listwise deletion applied in the presence of missing data. List-wise deletion results in the loss of roughly 20% of the observations for autocracies in the models including the full suite of control variables (from 6,644 observations to 5,540 observations), and about 26% of the observations for models including democracies (from 7,132 observations to 5,276 observations).

Comparing the matched data set of autocracies obtained after listwise deletion (here) to the matched data set of autocracies obtained after multiple imputation (Table 2 of the main article), a loss of roughly 16% of the data is evident, from 775 observations to 648 observations. The loss of this much information may impair the ability of coarsened exact matching to estimate causal effects with precision.

Nevertheless, the results here match those reported in the main article—with minor exceptions. For autocracies, the estimates of *ICC Jurisdiction* are all in the expected negative direction, and all but one (matched data, shared baseline hazard of exit) are statistically significant at conventional levels. Aside from this coefficient, effect sizes and standard errors here are roughly comparable to the analyses performed with the multiply imputed data set in the article.

For democracies, coefficient estimates of *ICC Jurisdiction* are generally near zero and statistically insignificant. This is comparable to what is reported in the article's Table 2. The exception here is that the estimate from the model with matched data and country-specific hazards is positive and statistically significant. This suggests that the ICC's jurisdiction had a detrimental causal effect on the political survival of leaders of the ex-DAC democracies—causing these democrats to lose office—even when statistically adjusting for unobserved country-level differences in baseline political survival over time.

Bear in mind, however, that coarsened exact matching with listwise deleted data results in a data set that is 33% smaller than coarsened exact matching with the multiply imputed data (from 2574 observations reported in Table 2 the article, to 1724 observations here). Using that larger set of matched observations, this positive effect is attenuated and statistically insignificant.

To conclude, the findings here generally support Hypothesis 2. A plausible explanation for discrepancies between Table 10 and Table 2 of the main article is the loss of valuable information arising from the combination of listwise deletion and coarsened exact matching.

Table 10: Effect of the ICC’s jurisdiction on the probability that democrats and autocrats exit office: slope coefficient estimates from event history logits with **listwise deletion** for missing observations, 1998-2017

Specification	Parameter	Models with a shared baseline hazard			Models with country-specific baseline hazards		
		Est	SE	p	Est	SE	p
Autocracies, $N = 6644$	Coef. of <i>ICC Jurisdiction</i>	-0.89*	0.35	0.02	-1.63*	0.42	0.01
Fixed effects: yes	AIC	1882			1436		
Control variables: no							
Autocracies, $N = 5540$	Coef. of <i>ICC Jurisdiction</i>	-0.75*	0.37	0.05	-1.49*	0.45	0.01
Fixed effects: yes	AIC	1592			1236		
Control variables: yes							
Matched autocracies, $N = 648$	Coef. of <i>ICC Jurisdiction</i>	-0.11	0.43	0.19	-1.01*	0.57	0.04
Control variables: yes	AIC	251.9			172		
Democracies, $N = 7132$	Coef. of <i>ICC Jurisdiction</i>	-0.08	0.23	0.38	0.14	0.26	0.35
Fixed effects: yes	AIC	2945			2404		
Control variables: no							
Democracies, $N = 5276$	Coef. of <i>ICC Jurisdiction</i>	-0.01	0.26	0.40	0.36	0.31	0.20
Fixed effects: yes	AIC	2238			1818		
Control variables: yes							
Matched democracies, $N = 1724$	Coef. of <i>ICC Jurisdiction</i>	-0.09	0.22	0.37	0.43*	0.26	0.10
Control variables: yes	AIC	1550			1210		

Note: List-wise deletion applied in the presence of missing data. Fixed effects are specified by country and year. Control variables include *Civil conflict*, *Oil Rents*, *GDP growth*, *Unemployment*, plus predictors in the Table 2 models: the interaction of *Democracy*, and *Total aid and loans*, *GDP*, *Rule of law*, *Common*, *Mixed*, *Islamic*, *Prior conflict*, *Prior repression*, and *Prior infant mortality*. Any time-invariant control variables are dropped in fixed-effects specifications. Baseline hazards are modeled as cubic polynomial functions of the number of quarters a leader has been in office. Significance at 10% level is indicated by *.

5.4 Models of battle deaths in conflicts of a civil nature with listwise deletion

Table 11 reports estimates from the models of battle deaths in civil conflict (i.e. the proxy for anti-regime violence) using listwise deletion in the presence of missing data.

For autocracies, the fixed-effects regressions with the full suite of control variables reflect those estimated with the multiply imputed data sets for autocracies, even though 51.6% of observation are lost to listwise deletion. This large loss of information is due to the large list of predictors on the right-hand side of the regressions. The coefficient estimates of *ICC Jurisdiction* are negative, of a similar magnitude as those reported in the article, and statistically significant in models estimated on the un-matched data set. For models estimated on the matched data sets, however, only 295 observations remain after listwise deletion and the matching algorithm. As in the previous two tables, the combination of listwise deletion and matching reduces the sample dramatically. This data set of autocracies resulting from listwise deletion plus matching is 66.2% smaller than its analogue analyzed in Table 3 of the main article (295 observations here versus 874 observations there). Regression results on this matched data set differ from those emerging from analyzing the imputed data: the coefficient estimates for *ICC Jurisdiction* are close to zero and statistically insignificant.

For democracies, the models yield a mixture of positive and negative, significant and insignificant coefficient estimates for *ICC Jurisdiction*. In models specifying a single global trend in battle deaths, coefficient estimates are positive and significant in the slimmed fixed-effects specifications with no control variables as well as in the fixed effects specification with all control variables. These positive effects attenuate to near zero in models specifying country-specific trends in battle deaths. Turning to the data set resulting from listwise deletion and matching: the resulting number of observations is 51.2% smaller than it is in the data set resulting from imputation and the same matching procedure. The coefficient estimates of *ICC Jurisdiction* are near zero and statistically insignificant. The negative effect of the ICC's jurisdiction on battle deaths is statistically insignificant in models analyzed with the matched data set of autocracies.

In context, these findings support Hypothesis 3. Notwithstanding the full finding for matched autocracies here, the generally negative coefficient estimates for autocracies and discordant mix of estimates for democracies is consistent with the theoretical expectation that the ICC's jurisdiction gives a degree of protection from domestic threats to autocracies, but not to democracies. One plausible explanation for the finding using the matched data set is the large loss of information due to the combination of coarsened exact matching and complete-case analysis using a data set with many missing observations on independent variables. Of course, this proviso applies to the results for democracies as well.

These matching estimates suggest a reason why multiple imputation may be desirable in this context. Coarsened exact matching forms strata from the interaction of all covariates in a regression in order to identify treated and control observations. The number of observations per strata is a key determinant of the ability of the algorithm to identify matches, and therefore, of the statistical power of whatever estimator (OLS, logit, etc) is applied to analyze the matched data set. The interaction of a large number of discretized control variables in forming the strata intensifies the challenge of implementing this method using incomplete data sets. It is unclear whether theoretical or simulation results on the effect of matching versus listwise deletion in the simple regression context (i.e., Pepinsky 2018) also extend to exact matching plus regression.

Table 11: Effects of the ICC’s jurisdiction on the scaled number of annual battle deaths in conflicts of a civil nature: slope coefficient estimates from OLS regressions with **listwise deletion** for missing observations

Specification	Parameter	Models with a shared baseline hazard			Models with country-specific baseline hazards		
		Est	SE	p	Est	SE	p
Autocracies, N = 6644	Coef. of <i>ICC Jurisdiction</i>	−0.95*	0.11	0.01	−0.19*	0.08	0.01
Fixed effects: yes	Adj. R-squared	0.63			0.81		
Control variables: no							
Autocracies, N = 3212	Coef. of <i>ICC Jurisdiction</i>	−0.80*	0.16	0.01	−0.23*	0.13	0.07
Fixed effects: yes	Adj. R-squared	0.71			0.82		
Control variables: yes							
Matched autocracies, N = 295	Coef. of <i>ICC Jurisdiction</i>	−0.21	0.25	0.40	0.01	0.19	0.94
Control variables: yes	Adj. R-squared	0.89			0.96		
Democracies, N = 7132	Coef. of <i>ICC Jurisdiction</i>	0.18*	0.04	0.01	−0.01	0.03	0.95
Fixed effects: yes	Adj. R-squared	0.79			0.88		
Control variables: no							
Democracies, N = 3381	Coef. of <i>ICC Jurisdiction</i>	0.26*	0.07	0.01	−0.04	0.06	0.47
Fixed effects: yes	Adj. R-squared	0.83			0.90		
Control variables: yes							
Matched democracies, N = 1273	Coef. of <i>ICC Jurisdiction</i>	0.11	0.09	0.23	−0.01	0.04	0.97
Control variables: yes	Adj. R-squared	0.44			0.91		

Note: Note: List-wise deletion applied in the presence of missing data. Fixed effects are specified by country and year for the unmatched data sets. Control variables include *Military expenditures*, *Military personnel*, *Population*, *Ethnic polarization*, *Religious polarization*, *Ruggedness*, *Refugees*, *Oil rents*, *GDP growth*, *Male youth unemployment*, plus predictors in the Table 2 models: *GDP*, *Rule of law*, *Total aid and loans*, *Common*, *Mixed*, *Islamic*, *Prior conflict*, *Prior infant mortality*, and *Prior repression*. Any time-invariant control variables are dropped in fixed-effects specifications. Time trends in *Battle deaths* are modeled as cubic polynomial functions of the year. P-values are one-tailed for autocracies, two-tailed for democracies. Significance at the 10% level is represented by *.

6 Details of the multiple imputation model used to produce data sets that regressions in the article are fit to: Multivariate Normal imputation for time-series cross-sectional data

6.1 Specifics of the model

The multiple imputation model is the workhorse Multivariate Normal model, as described in Schafer 1997 and presented in modified form for time-series cross-sectional data as Amelia II by Honaker and King 2010. This particular implementation is in common use across political science (e.g. Lall 2016).²

The imputation model specifies time trends for each variable are modeled with cubic polynomials within each of 14 regions, which are shown in Table 12 of this appendix. Holding the fraction of missing data constant, a longer time span results yields better imputation estimates at extra computational cost. The data set is extended backward to 1988(1) and 2017(4) for this reason.

The imputation model includes inverse-hyperbolic sine transformations and functions of variables entering regressions reported in the main analysis tables intended to render the data set nearer to a Multivariate Normal distribution. Interactions of many of the variables are included in the imputation model to increase the information available to it. Weakly informative Bayesian prior distributions are set on 20 variables with missing observations. An empirical ridge prior is set to ensure model convergence.

Importantly, the imputation model is augmented with extra variables having a low rate of missingness and some correlation with the indicators of internal conflict, economic conditions, and domestic politics relied upon in the article's regressions. To the degree that each of these extra variables is related to missingness in cross-national data sets, any bias introduced by the imputation model will shrink (Lall 2016). Variables measuring electoral democracy may be especially useful in this context, since they are correlated with the transparency of state reporting of economic and demographic statistics to international agencies like the World Bank (Hollyer, Rosendorff and Vreeland 2011). These variables are mentioned in the next subsection.

Ten copies of the imputed data were estimated. Honaker and King suggest that as few as three copies suffice to account for imputation uncertainty, but this view remains conjectural and the claim is likely contingent on particular data sets and imputation specifications. Overimputation plots were used as diagnostic tools to assess the fit of the model to the data set.

6.2 Extra variables entering the imputation model as predictors but not used afterward

Extra well-observed variables related to financial conditions, development, domestic political institutions, conflict, and geography are included in the multiple imputation model. Their inclusion reduces imputation error, giving us estimates of the missing data that are more accurate and precise. Many other potential variables were investigated, but excluded because they themselves feature a high rate of missing. The extra imputation model variables fall into several categories.

1. *Financial variables*: The World Bank 2019 supplies data on the total land area in hectares, the urban population as a percent of total population, and a state's net receipt or repayment of non-concessional debt from the IMF (DT.NFL.IMFN.CD). AidData.org 2010 supplies variables measuring the number of aid projects sponsored by the People's Republic of China which were completed within a country from 1990-2000.
2. *Political regime type variables*: Varieties of Democracy Version 8 (Coppedge et al. 2019) supplies variables associated with democratic governance derived from Bayesian measurement models using expert surveys as data. These are: v2x delibdem, v2x egaldem, v2x partipdem, v2x libdem, v2x polyarchy, and v2x regime.

²See Amelia II Version 1.7.5 of May 2018, as updated from the version described in Honaker, King and Blackwell 2012.

3. *Civil conflict variables*: UCDP supplies three variables measuring the number of days during which a state experienced an armed civil conflict (1945–1998), the number of days on which a state participated in an internationalized armed civil conflict (1945–1998), and the total number of battle-related deaths in civil and internationalized civil conflicts (1988–1998) in a state.
4. *Geographic variables*: Nunn and Puga 2012 supplies measures of the degree to which a country's geography is tropical, and the degree to which it is desert. A small number of these observations for entities that became states after 2010 or that were omitted from the data set were coded via original research.
5. *Leader age*: the age of all leaders was coded using publicly available information and data from the Archigos Project on their dates of birth.

6.3 Cross-sectional regions used in the multiple imputation and as fixed effects in regressions appearing in Table 2 of the article

A fourteen-fold classification of states into geo-political regions is used for specification of fixed effects in regressions where acceptance of ICC jurisdiction is the dependent variable are also used as cross-sections in the model for multiple imputation. To aid comparison, the names of the states in the table match those in Sarkees and Wayman 2010 or—if they are not included there—in *The World Factbook* of the Central Intelligence Agency of the United States of America 2012. Forward slashes indicate the words “and the.” Note that the table columns extend into the next page.

Table 12: Fourteen-fold regional classification of states

<i>North America:</i>	Mozambique	Turkey
Canada	Namibia	Yugoslavia
United States of America	Niger	<i>Ex-Sov. Un. & Eur. Sov. Bloc:</i>
<i>The Caribbean:</i>	Nigeria	Armenia
Antigua & Barbuda	Rwanda	Azerbaijan
Bahamas	Sao Tome and Principe	Belarus
Barbados	Senegal	Bulgaria
Cuba	Seychelles	Czech Republic
Dominica	Sierra Leone	Estonia
Dominican Republic	Somalia	Georgia
Grenada	South Africa	Latvia
Haiti	Swaziland	Lithuania
Jamaica	Tanzania	Moldova
St. Kitts and Nevis	Togo	Poland
St. Lucia	Uganda	Romania
St. Vincent / Grenadines	Zambia	Russia
Trinidad and Tobago	Zimbabwe	Slovakia
<i>Central America:</i>	<i>Mid. E & N Africa:</i>	Ukraine
Belize	Algeria	<i>Ex-Sov. Un. in Asia:</i>
Costa Rica	Bahrain	Kazakhstan
El Salvador	Egypt	Kyrgyzstan
Guatemala	Iran	Tajikistan
Honduras	Iraq	Turkmenistan
Mexico	Israel	Uzbekistan
Nicaragua	Jordan	<i>South Asia:</i>
Panama	Kuwait	Afghanistan
<i>South America:</i>	Lebanon	Bangladesh
Argentina	Libya	Bhutan
Bolivia	Morocco	India
Brazil	Oman	Maldives
Chile	Qatar	Nepal
Colombia	Saudi Arabia	Pakistan
Ecuador	Sudan (and S. Sudan)	Sri Lanka
Guyana	Syria	<i>Southeast Asia:</i>
Paraguay	Tunisia	Brunei
Peru	United Arab Emirates	Cambodia
Suriname	Yemen	East Timor
Uruguay	<i>N, W & Cent. Europe:</i>	Indonesia
Venezuela	Andorra	Laos
<i>Sub-Saharan Africa:</i>	Austria	Malaysia

– table continued from previous page

Angola	Belgium	Myanmar
Benin	Denmark	Philippines
Botswana	Finland	Singapore
Burkina Faso	France	Thailand
Burundi	Germany	Vietnam
Cameroon	Hungary	<i>East Asia:</i>
Cape Verde	Iceland	China
Cent. Af. Rep.	Ireland	Japan
Chad	Italy	Mongolia
Comoros	Liechtenstein	North Korea
Congo	Luxembourg	South Korea
Dem. Rep. of Congo	Monaco	Taiwan
Djibouti	Netherlands	<i>S. Pac. Ocean island states:</i>
Equatorial Guinea	Norway	Fed. States of Micronesia
Eritrea	Portugal	Fiji
Ethiopia	Spain	Kiribati
Gabon	Sweden	Marshall Islands
Gambia	Switzerland	Nauru
Ghana	United Kingdom	Palau
Guinea	<i>Balkans & SE Europe:</i>	Papua New Guinea
Guinea-Bissau	Albania	Samoa
Ivory Coast	Bosnia and Herzegovina	Solomon Islands
Kenya	Croatia	Tonga
Lesotho	Cyprus	Tuvalu
Liberia	Greece	Vanuatu
Madagascar	Kosovo	<i>Australia and New Zealand:</i>
Malawi	Macedonia	Australia
Mali	Malta	New Zealand
Mauritania	Montenegro	
Mauritius	Slovenia	

6.4 Fit diagnostics for the multiple imputation model using overimputation plots

A useful fit diagnostic for imputation models is the visual inspection of overimputation plots (Honaker and King 2010). To generate these plots, a researcher uses the estimated imputation model parameters to predict the observed values for every observation of a particular variable and incorporate model uncertainty into the predictions. These observed versus imputed plots yield a series of 90% confidence intervals (CIs) for imputed observations along the support of the variable, corresponding to all observed observations. If the imputation model fits the data well, then the CIs should overlap the observed values, especially in regions of the support of variables where there is a high rate of missingness. Departures from this pattern indicate that the imputation model has poor fit. The width of the CIs corresponds to the amount of uncertainty in the imputations about the actual values of the data.

This subsection shows the imputation model's observed-versus-imputed plots for all variables with high fractions of missing data that are also specified on the right-hand sides of models reported in the article (see Table 8). It also includes several variables with either a low fraction of missing data (*capital.scaled*) or that are only included in the imputation model (e.g. *polyarchy* from *Varieties of Democracy* Version 8). It is crucial to impute the variables included in the article's regressions accurately, since they constitute the imputation model's contribution to the effect estimates in models of jurisdiction acceptance, political survival, and battle deaths.

The plots below show that the imputation model has excellent fit with respect to the sixteen variables shown. The preponderance of confidence intervals in each plot are centered on the diagonal lines or cross them. Variables with the largest fraction of missing data, such as those measuring a state's population, GDP, and rule of law, are imputed with high accuracy. This is generally true even in regions of variables' support where missing data are concentrated. The plots show confidence intervals in colors that correspond to the fraction of missing data in the region of that confidence interval. Blue indicates low missingness, while green and orange CIs correspond missingness (fractions are indicated in the legend of each plot).

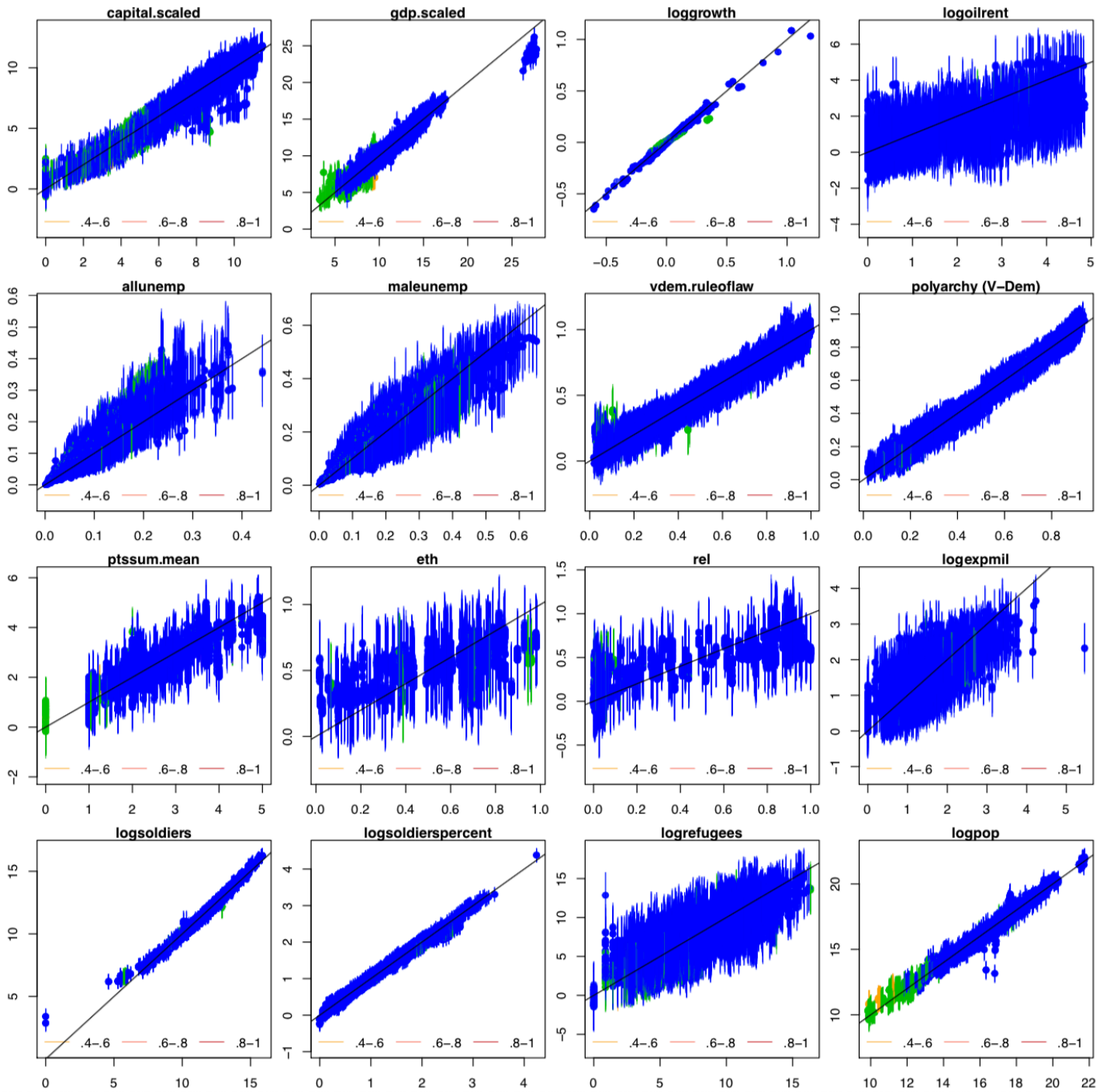
Under the assumption that the pattern of missingness in a variable x is statistically independent of x (i.e. that the data are not NI), the overimputation plots suggest that the model can impute missing data accurately. This lends some degree of confidence that any discrepancy between results from models estimated with listwise deletion and models estimated with the multiply imputed data set is due to bias and inefficiency arising from listwise deletion—not due to mis-specification of the multiple imputation model. In any event, results between these two sets of analyses are generally in agreement, as tables show below.

As an ending note to this discussion, the scenario of NI missingness is always possible, and such a scenario regression analysis on the NI data will be biased under both complete-case analysis and imputation. But for some variables, the threat of NI missingness is minor. It is likely the case that the variables measuring ethnic and linguistic polarization are missing in large part because of the difficulty of collecting such information for certain small and/or remote states (e.g. Kiribati, Vanuatu) and because of the creation of states (e.g. Kosovo, Montenegro, East Timor, and South Sudan) after the initial publication of these data. These, in fact, are the variables with the highest fraction of missing observations. Likewise, the smallness, remoteness, and youth of certain states are the most likely reasons why the *Varieties of Democracy* Project researchers do not field expert surveys about these states with which to measure the rule of law and political regime types in these states. For states in which assessing *de jure* and *de facto* institutions, freedoms, judicial independence, and inter-branch relationships is indeed difficult due to strategic censoring—such as South Sudan, China, North Korea, Oman, and Venezuela—we have observed values on V-Dem variables due to the intense focus of country experts on those states.

In other cases, NI scenarios are plausible: severe wars and recessions, debt and currency crises, and dramatic and poorly understood shifts in domestic political institutions can intensify the challenge of measuring of wartime casualties, demographics, macroeconomic statistics, and institutions. Recorded observations are more likely to be measured with error, and conceptual frameworks may even need modification. In such

circumstances, improvements in conceptualization, measurement, and/or imputation methodology appear to be superior solutions to complete-case analysis and imputation models assuming that the data are not NI.

Figure 1: Overimputation plots for independent variables and control variables that have a moderate to high fraction of missing observations in the 1998–2017 data set



7 Alternative specifications of models reported in the article using the multiply imputed data set

7.1 Slimmed models with no control variables

The regressions in the article feature a limited number of control variables whose theoretical justification is reported in the “Evidence” section. Their purpose is to attenuate bias due to omitted variables and to improve the precision of parameter estimates.

Some political methodologists suggest that the practice of specifying control variables in regressions is misguided. They point out that control variables can increase or decrease omitted variables bias in practice,³ that they generate post-treatment bias if they are causally posterior to the explanatory variables of interest,⁴ and that despite these dangers, they may be included in regressions by convention alone.⁵

Readers who are sympathetic to these points might want to see slimmed specifications of the article’s regressions—specifications with all control variables omitted. These are printed in the tables that follow in this section (corresponding to the article’s Tables 2, 4 and 6, respectively). Table 14 presents tests of Hypothesis 1, where aid variables are exponentiated as in the article. Table 15 presents tests of Hypothesis 2. And Table 16 presents tests of Hypothesis 3. The tables do not present regressions on matched data sets, because matching requires control variables.

Under these re-specifications, the evidence still largely supports all three hypotheses. Tables follow.

³Clarke 2009

⁴King and Zeng 2006, 147

⁵Achen 2002, Ray 2003

Table 14: Effect of *Total aid and loans* on whether a leader accepts ICC jurisdiction, 1998–2017: coefficient estimates from event history logits with no control variables

Data Set	Parameter	Models with a shared baseline hazard			Models with country-specific baseline hazards		
		Est	SE	p	Est	SE	p
Full	Coef. of <i>Total aid and loans</i> in autocracies	1.13*	0.64	0.04	3.09*	0.81	0.01
	Coef. of <i>Total aid and loans</i> in democracies	−1.15*	0.67	0.09	0.46	1.13	0.37
	Mean AIC	942			440		
	<i>N</i> = 7919						

Note: Each model is fit to ten multiply imputed data sets and the results pooled. Fixed effects are specified for 14 regions and 20 years. All measures of aid are continuous. Control variables are omitted. Baseline hazards modeled with cubic polynomials. P-values are one-tailed for autocracies, two-tailed for democracies. Significance at the 10% level is represented by *.

Table 15: Effect of the ICC’s jurisdiction on the probability that a leader exits office, 1998–2017: coefficient estimates from event history logits with no control variables

Data Set	Parameter	Models with a shared baseline hazard			Models with country-specific baseline hazards		
		Est	SE	p	Est	SE	p
Autocracies	Coef. of <i>ICC Jurisdiction</i>	−0.91*	0.35	0.01	−1.30*	0.39	0.01
	Mean AIC	1874			1560		
	<i>N</i> = 6644						
Democracies	Coef. of <i>ICC Jurisdiction</i>	−0.16	0.22	0.30	0.03	0.25	0.40
	Mean AIC	2946			2539		
	<i>N</i> = 7132						

Note: Each model is fit to ten multiply imputed data sets and the results pooled. Control variables are omitted. Fixed intercepts are specified for countries and years. The shared baseline hazard is modeled non-parametrically as a function of the number of quarters a leader has been in office. Country-specific baseline hazards are modeled similarly with cubic polynomials. P-values are one-tailed for autocracies, two-tailed for democracies. Significance at the 10% level represented by *.

Table 16: Effects of the ICC’s jurisdiction on the scaled number of annual battle deaths of a civil nature, 1998-2017: slope coefficient estimates from normal regressions with no control variables

Data set	Parameter	Models with a shared time trend			Models with country-specific time trends		
		Est	SE	p	Est	SE	p
Autocracies	Coef. of <i>ICC Jurisdiction</i>	-0.95*	0.11	0.01	-0.19*	0.08	0.01
	Mean Adj. R-squared	0.63			0.81		
	<i>N</i> = 6560						
Democracies	Coef. of <i>ICC Jurisdiction</i>	0.18*	0.04	0.01	-0.002	0.03	0.40
	Mean Adj. R-squared	0.79			0.88		
	<i>N</i> = 7130						

Note: Each model is fit to ten multiply imputed data sets and the results pooled. Fixed effects are specified for countries and years. Control variables are omitted. Shared and country-specific trends in battle deaths is modeled as a cubic polynomial function of the year. P-values are one-tailed for autocracies, two-tailed for democracies. Significance at the 10% level is represented by *.

7.2 Tests of Hypotheses 1, 2, and 3 using a data set limited to only capital-eligible states, 1998–2017

As a further robustness check Hypotheses 1 through 3 are tested on only the states eligible to receive aid or loans publicly financed by wealthy democracies. Models are fit to the ten multiply imputed data sets. With a large number of sovereign democratic donors, determining aid eligibility is not trivial. The list of aid-eligible states published by the DAC varies by year, and in certain years it has included middle-income countries such as Chile, China, the United Arab Emirates, Saudi Arabia, and Mexico (among others).⁶ Lists by the World Bank, IMF, and regional development banks may vary. Disbursements of non-concessional loans and credits are likely to follow a rather different logic, as they bear interest and are motivated by objectives of global and regional economic stability in addition to national economic development.

The most straightforward to determining whether a state was capital-eligible during the 1998–2017 period is whether it received any non-zero amount of aid or loans from wealthy democracies or the multilateral entities controlled by them. To perform this robustness check, therefore, states with zero receipts of development capital from the OECD DAC, EC, IMF, World Bank, regional banks (Asian, African, Inter-American, and Council of Europe), and national aid agencies run by wealthy democracies in the 1998–2017 period are excluded from the data set.

The four capital-ineligible states excluded in analyses below are Andorra, Iceland, Liechtenstein, and Monaco.⁷ Recall that the 23 sovereign members of the OECD DAC donor club (as of 1998) are excluded from data sets analyzed in the article *a priori*.

Tests of Hypotheses 1, 2, and 3 otherwise identical to those reported in the article are performed and reported. Tables 17, 18, and 19 display the findings of these robustness checks. The results are quite close to those reported in the article. In summary, these robustness checks offer support for the claims that foreign capital causes autocrats but not democrats to accept ICC jurisdiction; and that the ICC's jurisdiction reduces anti-regime violence in autocracies, but not in democracies.

⁶See <http://www.oecd.org/development/financing-sustainable-development/development-finance-standards/historyofdaclistsofaidrecipientcountries.htm>.

⁷The same four states are the only states in the data set that received no Official Development Assistance from the DAC or EC in 1988–2017.

Table 17: Effect of foreign capital on acceptance of ICC jurisdiction in **capital-eligible** autocracies and democracies, 1998-2017: slope coefficient estimates from event history logits

Data Set	Parameter	Models with a shared baseline hazard			Models with country-specific baseline hazards		
		Est	SE	p	Est	SE	p
Full <i>N</i> = 7805	Coef. of <i>DAC-EC Aid</i> in autocracies	2.70*	1.87	0.07	4.49*	2.28	0.03
	Coef. of <i>DAC-EC Aid</i> in democracies	-4.71	2.40	0.06	-2.18	2.61	0.28
	Mean AIC	935			440		
Full <i>N</i> = 7805	Coef. of <i>Multilateral aid</i> in autocracies	2.12	3.15	0.16	6.49*	3.67	0.04
	Coef. of <i>Multilateral aid</i> in democracies	-12.30*	6.63	0.07	4.90*	2.64	0.07
	Mean AIC	936			445		
Full <i>N</i> = 7805	Coef. of <i>Multilateral loans</i> in autocracies	1.27*	0.83	0.06	3.64*	1.17	0.01
	Coef. of <i>Multilateral loans</i> in democracies	-0.99	0.94	0.23	1.90	1.75	0.22
	Mean AIC	937			439		
Full <i>N</i> = 7805	Coef. of <i>Total aid and loans</i> in autocracies	0.94*	0.71	0.08	2.94*	0.98	0.01
	Coef. of <i>Total aid and loans</i> in democracies	-1.27	0.77	0.10	0.44	1.28	0.38
	Mean AIC	935			439		
Autocracies matched on <i>High aid</i> \bar{N} = 2983.2	Coef. of <i>High aid</i> in autocracies	1.10*	0.70	0.07	1.50*	1.30	0.10
	Mean AIC	220			73		
Democracies matched on <i>High aid</i> \bar{N} = 1808.5	Coef. of <i>High aid</i> in democracies	-0.39	0.41	0.25	0.45	0.59	0.30
	Mean AIC	370			200		

Note: Fixed effects are specified for 14 regions and 20 years. Models are estimated on 10 imputed data sets and results pooled. All measures of aid are continuous except for *High aid*, a dummy variable equaling one when *Total aid and loans* exceeds the sample 75th percentile. Control variables include *GDP*, *Rule of law*, *Common*, *Mixed*, *Islamic*, *Prior conflict*, *Prior repression*, and *Prior infant mortality*. Baseline hazards modeled with cubic polynomials as a function of the number of quarters a leader has been in office since the opening of the Rome Statute for ratification. P-values are one-tailed for autocracies, two-tailed for democracies. Significance at 10% level is indicated by *.

Table 18: Effect of the ICC’s jurisdiction on the probability that **capital-eligible** democrats and autocrats exit office, 1998-2017: slope coefficient estimates from event history logits

Specification	Parameter	Models with a shared baseline hazard			Models with country-specific baseline hazards		
		Est	SE	p	Est	SE	p
Autocracies, $N = 6565$	Coef. of <i>ICC Jurisdiction</i>	-1.03*	0.35	0.01	-1.38*	0.39	0.01
Fixed effects: yes	Mean AIC	825			620		
Control variables: yes							
Matched autocracies, $\bar{N} = 1814$	Coef. of <i>ICC Jurisdiction</i>	-0.97*	0.48	0.03	-1.29*	0.56	0.01
Control variables: yes	Mean AIC	255.7			198.7		
Democracies, $N = 6884$	Coef. of <i>ICC Jurisdiction</i>	-0.16	0.23	0.32	0.04	0.26	0.39
Fixed effects: yes	Mean AIC	2830			2450		
Control variables: yes							
Matched democracies, $\bar{N} = 4771$	Coef. of <i>ICC Jurisdiction</i>	-0.18	0.18	0.25	0.16	0.21	0.30
Control variables: yes	Mean AIC	1094			906.5		

Note: Models are estimated on 10 multiply imputed data sets and results pooled. Fixed effects are specified by country and year for the unmatched data sets. Control variables include *Civil conflict*, *Oil Rents* and *GDP growth*, plus predictors in the Table 2 models: the interaction of *Democracy*, *GDP*, *Common*, *Mixed*, *Islamic*, *Prior conflict*, *Prior repression*, and *Prior infant mortality*. Any time-invariant control variables are dropped in fixed-effects specifications. Baseline hazards are modeled as cubic polynomial functions of the number of quarters a leader has been in office. Significance at 10% level is indicated by *.

Table 19: Effects of the ICC’s jurisdiction on the scaled number of annual battle deaths in conflicts of a civil nature among **capital-eligible** states, 1998-2017: slope coefficient estimates from OLS regressions

Specification	Parameter	Models with a shared baseline hazard			Models with country-specific baseline hazards		
		Est	SE	p	Est	SE	p
Autocracies, $N = 6560$	Coef. of <i>ICC Jurisdiction</i>	-0.83*	0.10	0.01	-0.20*	0.08	0.01
Fixed effects: yes	Mean Adj. R-squared	0.67			0.81		
Control variables: yes							
Matched autocracies, $\bar{N} = 896$	Coef. of <i>ICC Jurisdiction</i>	-0.88*	0.19	0.01	-0.59*	0.13	0.01
Control variables: yes	Mean Adj. R-squared	0.45			0.71		
Democracies, $N = 6880$	Coef. of <i>ICC Jurisdiction</i>	0.22*	0.04	0.01	0.01	0.03	0.37
Fixed effects: yes	Mean Adj. R-squared	0.79			0.88		
Control variables: yes							
Matched democracies, $\bar{N} = 2430$	Coef. of <i>ICC Jurisdiction</i>	0.14*	0.05	0.02	0.02	0.31	
Control variables: yes	Mean Adj. R-squared	0.28			0.84		

Note: Models are estimated on 10 multiply imputed data sets and results pooled. Fixed effects are specified by country and year for the unmatched data sets. Control variables include *Military expenditures*, *Military personnel*, *Population*, *Ethnic polarization*, *Religious polarization*, *Ruggedness*, *Refugees*, *Oil rents*, *GDP growth*, *Male youth unemployment*, plus predictors in the Table 2 models: *GDP*, *Rule of law*, *Total aid and loans*, *Common*, *Mixed*, *Islamic*, *Prior conflict*, *Prior infant mortality*, and *Prior repression*. Any time-invariant control variables are dropped in fixed-effects specifications. Time trends in *Battle deaths* are modeled as cubic polynomial functions of the year. P-values are one-tailed for autocracies, two-tailed for democracies. Significance at the 10% level is represented by *.

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