

Conflict, Cooperation, and Delegated Diplomacy

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Abstract

Does diplomacy affect the prospects of international conflict and cooperation? Systematic empirical assessment has been hindered by the inferential challenges of separating diplomacy from the distribution of power and interests that underlies its conduct. This paper addresses the question of diplomacy's efficacy by analyzing variation in US diplomatic personnel and their influence over the US foreign policy process. I claim that diplomats hold the strongest preferences for cooperative relations with their host countries, relative to other participants in the process; that they exert substantial influence over the formation and implementation of US policies toward their host countries; but that their influence is intermittently weakened by the short-term shock of an ambassadorial turnover. As a result, when ambassadors are removed from post, diplomacy is more likely to be eschewed for more conflictual means of settling international disagreements, and opportunities for economic exchange are less likely to be realized. This theory is tested using newly collected data on US diplomatic representation, for the global sample of countries from 1960 through 2014. To address concerns of diplomatic staffing being endogenous to political interests, I leverage a natural experiment arising from the State Department's three-year ambassadorial rotation system. The turnover of a US ambassador causes a decrease in US exports to the country experiencing the turnover, and heightens the risk of onset of a militarized dispute between that country and the US. These findings point to bureaucratic delegation as an important but overlooked determinant of macro-level international outcomes.

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In his first week in office, President Donald Trump took the unprecedented step of demanding the immediate resignations of all politically appointed US ambassadors, along with several higher-level State Department officials, with no grace period and no candidates in line to fill the vacancies.¹ A year into the administration, fifty-seven ambassadorial posts and five of eight Deputy and Under Secretary positions remained unfilled.² Critics were quick to denounce this approach, accusing the president of waging “war on the State Department”³ or even on diplomacy itself.⁴ Yet while this particular degree of understaffing was unprecedented, the general devaluation of diplomacy that it represented was not. Political leaders have long insisted on squeezing State Department budgets,⁵ shuttering embassies and consulates,⁶ and foregoing diplomatic tact for Pentagon-driven “mil-think” in the policy process,⁷ while demanding that their diplomatic agents simply “do more with less”.⁸ American diplomacy, it appears, has gone out of favor.

The public concern over Trump’s diplomatic neglect, and the historical trajectory that it culminated, raises a fundamental question: does diplomacy affect the prospects of international conflict and cooperation, independent of the distribution of power and interests that underlies its conduct? Systematic empirical evidence of diplomacy’s efficacy has remained elusive. At a basic level, much of the work of diplomacy is unobservable to researchers, conducted behind closed doors or through classified cables.⁹ Insofar as diplomacy can be observed and quantified, its variation is largely driven by more fundamental political forces, calling into question any claims of diplomacy causing a particular outcome. States self-select into negotiations likely to bear fruit, or into those rendered necessary by impending

1 Williams and Siemaszko (2017)
2 Bureau of Public of Affairs (2018)
3 Chalfant (2017)
4 Editorial Board (2017)
5 Mathias (1986)
6 Keeley (2000)
7 Farrow (2018)
8 Malone (2013)
9 Trager (2017)

threats. They open embassies in countries where opportunities for commercial exchange are rich, and close them when relations sour. Trump’s gutting of diplomatic personnel came amidst a broader foreign policy reorientation, leaving observers uncertain as to whether the understaffing itself bore any real consequence.

This paper addresses the question of diplomacy’s utility by analyzing diplomats’ influence in the formation and implementation of US foreign policy.¹⁰ Specifically, I examine the variation in diplomatic influence that arises from the routine rotation of US ambassadors. The theory draws from broad literatures on bureaucratic politics, public administration, and diplomatic history, and brings their insights to bear in explaining patterns of international conflict and cooperation.

Central to the argument are the relative interests and capabilities of the various participants in the foreign policy process. Among all participants, the chief of mission (COM) to a foreign country typically holds the strongest preferences for cooperative relations with that country,¹¹ and wields substantial influence over the policies enacted towards it.¹² But the removal and replacement of an ambassador—with the embassy overseen by an acting official in the interregnum—causes a short-term negative shock to the weighting of diplomatic inputs in the policy process,¹³ relative to the inputs of other actors who are not similarly motivated to protect the bilateral relationship. These intragovernmental dynamics have international implications, as a transitory diplomat lacking internal influence will in turn be less capable of committing to and implementing the kinds of agreements that undergird bilateral cooperation. In short, during an ambassadorial turnover, the agent most interested in maintaining positive relations with a given country is rendered least effective in doing so, and diplomatic

¹⁰ Definitions of the term “diplomacy” vary so widely that no single analysis can hope to engage them all. For a sense of the diversity of definitions and approaches to the topic, see Plischke (1979), Leguey-Feilleux (2009), Sending et al. (2015), and Trager (2017). The conceptualization of diplomacy invoked in this study is perhaps closest to one referenced in Plischke (p.28): diplomacy defined simply (if somewhat circularly) as “the business of the diplomat”.

¹¹ Miles (1978); Wilson (1989); Lindsey (2017)

¹² Allison and Zelikow (1999); Halperin and Clapp (2007)

¹³ O’Connell (2009)

outcomes suffer as a result.

The argument yields general propositions about the efficacy of delegated diplomacy, which I test in the specific empirical context of US ambassadorial appointments, focusing on two primary outcomes. First, I consider militarized interstate disputes (MIDs) as escalatory actions which are rendered necessary by the inability to achieve mutually acceptable settlements through diplomatic negotiation. Second, I examine export volumes as the product of diplomats both seeking out commercial opportunities and enforcing cooperative arrangements in the face of short-term incentives to renege.¹⁴ If ambassadorial turnovers impose constraints on diplomats' abilities to make credible commitments to their foreign counterparts, they should in turn increase the likelihood of militarized escalation and decrease volumes of economic exchange. Using newly collected data on US diplomatic personnel for the global sample of countries from 1960-2014, I find support for these predictions: the removal of an ambassador decreases US exports to the country experiencing the turnover, and heightens the risk of onset of a militarized dispute between that country and the US. Subgroup analysis suggests that the aggregate effects are driven primarily by turnovers of career ambassadors, rather than non-career (or "political") appointees. The quantitative analyses are complemented by a set of qualitative case studies illustrating the mechanisms underlying the empirical results.

The use of ambassadorial turnover as a measure of diplomacy—or specifically, as a measure of disruption in the conduct of diplomacy—is, at first glance, susceptible to the same methodological concerns that plague the various measures described above: the timing of rotation and duration of vacancy are likely endogenous to unobservable political considerations, and the direction and magnitude of the resulting bias are difficult to determine ex ante. To address this concern, my research design leverages a natural experiment arising from a distinct feature of modern US diplomatic practice: the State Department's routinized three-year ambassadorial rotation system.¹⁵ An ambassador's entrance into office in year $t - 3$

¹⁴ Gertz (2018)

¹⁵ Ambassadorial rotation is not unique to the US; however, few countries rotate ambassadors at the same

is a strong predictor of turnover in year t , and is assigned (conditionally) independently of outcomes in year t . Using a $t - 3$ ambassadorial appointment to instrument for turnovers thus circumvents the confounding and strategic behavior that would otherwise undermine any attempt to estimate the causal effects of diplomatic representation.

A primary contribution of this study is its effort to reconcile a fundamental tension in the ways that scholars and practitioners think about diplomacy.¹⁶ Formal and quantitative IR scholars tend to theorize diplomacy as a set of discrete and delimited interactions conducted by leaders or unitary states, typically involving threats to use force and deterrence thereof,¹⁷ or negotiations over cooperative agreements and their subsequent enforcement.¹⁸ A rich body of firsthand and qualitative accounts of diplomacy, in contrast, gives primacy to the agency of individual diplomats carrying out the routine work of interstate relations.¹⁹ The present study leverages insights from the agent-oriented literature to generate theoretical predictions which can be tested systematically on international outcomes. My claim is that those interactions predominantly theorized by IR scholars are occurring constantly and in all corners of the world, at varying intensities but at a volume that far outstrips a leader's capacity to manage them all,²⁰ so the relevant party to the exchange is often neither the country nor the leader but the agents to whom authority has been delegated. The empirical analyses show that, *ceteris political* paribus, international conflict and cooperation vary substantially as a function of the attributes of those agents: in the conduct of foreign relations, personnel is policy. The concluding section provides some discussion as to how these findings might generalize beyond the present context of analysis and offer broader insights into the nature of delegated diplomacy.

rate that the US does, or with the same degree of vacancy between appointments (Leguey-Feilleux, 2009; Plischke, 1979).

¹⁶ For a discussion of this tension, see Jönsson and Hall (2005).

¹⁷ Sartori (2005); Ramsay (2011)

¹⁸ Fearon (1998)

¹⁹ Keeley (2000); Plischke (1979); Kopp and Gillespie (2011)

²⁰ Lindsey and Hobbs (2015)

Ambassadors and Agency in International Relations

I begin with a brief exposition of three empirical cases which will be referenced throughout the theoretical discussion.²¹ These cases illustrate the roles that ambassadors play in the foreign policy process, the preferences they hold, and the channels through which they can influence policy outcomes; subsequent analysis considers how their influence is diminished during ambassadorial turnovers, and the international consequences that follow.

Agency in practice

The bombing of the USS Cole, a US Navy destroyer docked off the port of Aden, by Al Qaeda militants in October 2000 sparked an acute tension in the US-Yemeni relationship. According to a State Department investigator on the scene, US marines and FBI agents “rushed in in full force. . . like they were attacking Aden rather than coming to do an investigation. . . and they didn’t know if they were going to be met by violence or an arm[ed] struggle.”²² Another diplomat on the ground recalls that “paranoia was added to legitimate threat. Yes, the situation was teetering on the brink of being out of control.”²³ These conditions gave rise to an intense interagency dispute over the appropriate balancing of priorities in the US response, the primary antagonists of which were chief FBI investigator John O’Neill, and Barbara Bodine, the US ambassador to Yemen. Upon the FBI’s arrival in the country, Bodine “pleaded with O’Neill to consider the delicate diplomatic environment he was entering. O’Neill responded that he was here to investigate a crime, not to conduct diplomacy.”²⁴ The ambassador insisted on keeping the FBI’s presence at minimally intrusive levels, demanding that they operate in smaller numbers and with smaller weapons; ultimately tensions mounted

²¹ This paper makes frequent reference to two firsthand sources. First are interviews from the Oral History collection of the [Association for Diplomatic Studies & Training \(2020\)](#), cited as “ADST: [Interviewee name]”. Second are documents from the Foreign Relations of the United States series of the State Department’s [Office of the Historian \(2020\)](#), cited as “FRUS: [Document number]”.

²² ADST: Michael Metrinko (p.207)

²³ ADST: Edmund James Hull (p.126)

²⁴ [Wright \(2006\)](#), p.365

to the point that Bodine exerted her authority as chief of mission to block O’Neill’s entry into the country.

When Ambassador John Wolf arrived in Kuala Lumpur in 1992, he “found the embassy was basically on idle. . . We had a big canvas on which to paint, but mostly it was blank.” The Malaysian government had for some time been in the market for military aircraft; but US-Malaysian relations were “strained”²⁵ and the “sterile official dialogue meant that this issue was far off policy radars.”²⁶ Wolf exercised his discretion to remove what was previously a major sticking point in US-Malaysian relations—a Malaysian proposal for an exclusive “East Asian Economic Caucus”, which ultimately proved fruitless but had driven Secretary of State Jim Baker “apoplectic”²⁷—off of the bilateral agenda, creating space for more productive engagement. Turning to the aircraft push, Wolf took the initiative to arrange a series of high-level meetings to promote the US-manufactured F-18 over its Russian competitor: these included visits from the McDonnell Douglas CEO and from officers in the Defense Security Assistance Agency, as well as a trip with the Malaysian defense minister to a US aircraft carrier to see the F-18s in action. He further guided McDonnell Douglas through the bidding process, advising them on ways to integrate the proposal with the Malaysian prime minister’s “Vision 2020” industrialization plan and pushing them to internalize the notion that they “can’t just sell a piece of metal; you need to sell a relationship.”²⁸ The effort proved successful. Within a year of the commencement of the sales push, a deal was signed for the purchase of \$750 million of American-made aircraft.

Two persistent issues dominated US-Peruvian relations from the start of Ambassador Johnny Jones’s tenure there in 1963. First was an incompatibility between US and Peruvian views of national jurisdiction over territorial fishing waters: Peru asserted a 200-mile sovereign zone off its coastline, while the US recognized only a three-mile limit. Second was

²⁵ ADST: John Wolf (p.77-78)

²⁶ Keeley (2000, p.96)

²⁷ ADST: John Wolf (p.78)

²⁸ Ibid. (p.80-85)

Peruvian President Belaúnde’s inaugural commitment to nationalize the US-owned International Petroleum Company (IPC), coupled with the domestic political imperative to do so without compensation and in violation of US understanding of international investment law. Due to a set of foreign aid amendments restricting assistance following the confiscation of US fishing vessels or other assets, these policy disagreements came to be viewed as “time bombs threatening [Ambassador Jones’s] mission”, and he worked assiduously to defuse them. On the former issue, his deputy recalls, Jones “succeeded early on in negotiating an informal modus operandi which effectively muffled the problem...[T]he Peruvians pretty much looked the other way or if a vessel was detained, a quick visit by an Embassy rep to the affected port would result in a ‘solution’ without violence.”²⁹ As for the IPC matter, Jones fought within his own administration to ensure that bilateral aid be used effectively to incentivize a settlement between the company and the Peruvian government, withheld only when talks broke down but released when they resumed in good faith.³⁰ A major setback in bilateral relations followed the Peruvian military’s ouster of Belaúnde in October of 1968. Voices throughout Congress and elsewhere in the administration wanted to make an example of Peru, calling for the immediate suspension of aid and severance of diplomatic ties; but Jones insisted that recognition of the new government was “the only way the United States could expect to continue to protect and promote its interests in Peru.”³¹ The ambassador’s position won out, and bilateral negotiations proceeded.

The above episodes are illustrative of the first two components of this paper’s argument: the unique policy preferences held by diplomats on the ground, and the influence they wield to realize those preferences. I discuss each of these points in turn.

²⁹ ADST: Ernest Siracusa (p.30-31)

³⁰ FRUS: 1964-68v31/d470; ADST: Ernest Siracusa (p.34)

³¹ Walter (2010, p.146)

Preferences

Relative to other participants in the foreign policy process—including officials from other agencies, the military, the White House, and Congress—diplomats tend to hold the strongest preferences for cooperative relations with the countries in which they operate. Put differently, they exhibit the greatest willingness to pay for positive bilateral relations, whether that payment takes the form of time spent and effort exerted (their own or their subordinates’), favors called in and political capital exhausted, or other policy objectives compromised in service of the overall relationship. In their authoritative examination of the US foreign policy bureaucracy, Halperin and Clapp observe that diplomats on the ground are “strongly motivated to improve relations with” their host governments, often viewing that objective as “vital to the security of the United States, whereas priorities decided on in Washington seem out of touch”³² The claim is not that other participants seek uncooperative relations per se, but simply that they are less willing to make the tradeoffs necessary to avoid them.

Explanations vary as to the sources of these preferences. Perhaps the simplest account follows from Miles’ Law—“where you stand depends on where you sit”—which holds that, in public administration, there is “no such thing as pure objectivity... Every person has a function to perform and that assigned responsibility markedly influences one’s judgment.”³³ Diplomats are tasked with improving diplomatic relations, so the policy positions they adopt reflect the organizational position they occupy. Other rational-choice explanations point to processes of top-down³⁴ or bottom-up³⁵ selection of diplomatic “types” into the diplomatic bureaucracy; sociological theories highlight the role of the Foreign Service’s organizational culture in instilling values and perspectives in the organization’s members over time.³⁶ The reality is likely some combination of the above, and each yields similar implications for the

³² Halperin and Clapp (2007, p.276)

³³ Miles (1978)

³⁴ Lindsey (2017)

³⁵ Gailmard and Patty (2007)

³⁶ Wilson (1989)

purposes of the present study.

How these preferences manifest in policy debates will depend on the particular issues at stake and alignment of vested interests. Bodine’s clash with the FBI was fundamentally a disagreement over the relative importance of the holistic relationship with Yemen versus the narrow investigative goals being pursued there. Jones found his diplomatic efforts in Peru coming into conflict, on the one hand, with US business interests channeling their myopic and maximalist demands through Congress;³⁷ and on the other, with the broader objectives of his superiors at State, who “appreciated the ambassador’s ‘local problems’” but had to achieve “a careful and exacting coordination of US interests and timing, including some related by non-Peruvian factors”.³⁸ While a range of actors other than Ambassador Wolf—officials in State or Commerce at the Assistant or Under Secretary level, for instance—could plausibly have orchestrated the aircraft sale to Malaysia, those actors were evidently more concerned with addressing other problems or exploiting other opportunities elsewhere; only the ambassador’s purview was sufficiently delimited as to render the sales effort a worthwhile pursuit.

Reflecting on her handling of the Cole situation, Bodine is unapologetic. “Diplomatic relations,” she writes, “provide a context within which we are able to operate—or not. . . It was my job to make sure everyone involved understood that our actions must not subvert our goals.”³⁹ James Blanchard proudly describes his fellow ambassadors as “the only Americans who worry full-time about the complete relationship with a particular country. . . Nobody else does.”⁴⁰ Farrow depicts this dynamic somewhat less charitably, noting that “[e]yes in many a White House have rolled when the subject of ‘State’s objections’ has been raised.”⁴¹ Yet however such arguments are received by other participants in the process, the ambassador

37 Walter (2010, p.67)

38 Ibid., p.153

39 Bodine (2006)

40 Blanchard (1998, p.131)

41 Farrow (2018)

will often find herself the strongest (if not the only) advocate for the importance of relations with her host country to the broader conception of the US national interest.

Influence

Considerations of diplomats' policy preferences are only consequential insofar as those preferences are somehow determinative of policy outcomes. Government behavior on any given international issue is inherently multifaceted—a “collage” of actions and decisions,⁴² each rendered by different participants at different levels of government—and so examination of a diplomat's role in the policy process should likewise account for the various channels through which she might exert her influence over the outcome.

The channels of influence available to a chief of mission can be grouped into two general categories: areas in which she has authority to take action independently, and areas in which she lacks that authority but can advise and advocate for decisions made by her superiors. As for the first channel, US ambassadors tend to enjoy quite a broad discretionary window. The chief of a foreign mission has, for instance, “wide latitude to decide how and at what level in the host government to carry out an instruction from Washington”; as a result, ambassadors in the field “can easily come to feel that it is their responsibility...to effectively shape policy” toward their host country.⁴³ One ambassador recalls, as his final instruction before arriving at post, that “senior officials in Washington hoped they would not have to pay too much attention to Bolivia.”⁴⁴ Another notes that an “ambassador to a major country can actually have a wider range of authority and activity than all but the most senior cabinet members.”⁴⁵ An important source of a COM's discretionary authority is her statutory grant of “full responsibility for the direction, coordination, and supervision of all Government

⁴² Allison and Zelikow (1999, p.257)

⁴³ Halperin and Clapp (2007, p.278)

⁴⁴ Keeley (2000, p.24)

⁴⁵ Blanchard (1998, p.121)

executive branch employees in that country”⁴⁶ and her ensuing authority to deny clearance for any such employee to enter the country in an official capacity.⁴⁷ Thus even when the broad contours of policy are determined in Washington, the chief of mission is left with considerable leeway to determine how that policy will be implemented by personnel on the ground.

Where a diplomat lacks authority to act independently, she may still shape policy indirectly through the advice and recommendations she provides to her superiors.⁴⁸ According to Allison, “[m]ost problems are framed, alternatives specified, and proposals pushed” by a mid-level official such as an ambassador. The challenge she faces is “how to get an issue on an action-channel, how to get the government ‘to do what is right.’ The incentives push [her] to become an active advocate.”⁴⁹ Echoing this perspective, Halperin and Clapp suggest that “[a]rguments in favor of a decision are the most important form in which information reaches the president and other senior participants”; the ability to formulate compelling arguments and proposals, to navigate them through the interagency process and to “take account of criticisms and to get as many participants as possible on board”⁵⁰ constitute an important means of bureaucratic influence.

Stepping back from the internal politics, of interest to a foreign government is whether the US chief of mission wields sufficient influence—through whatever channel is available—to shape the overall policy that the US puts forward in the bilateral relationship. The credibility of a diplomat’s commitment to pursuing a given course of action will thus depend on her ability to enact the policy herself, or to convince her superiors to adopt her desired position, or some combination of the two. Ambassador Bodine was able to independently set de facto bilateral policy by curtailing O’Neil’s investigation and discarding the most combustible

⁴⁶ Foreign Service Act (1980)

⁴⁷ Kopp and Gillespie (2011, p.145)

⁴⁸ Saunders (2017)

⁴⁹ Allison and Zelikow (1999, p.308)

⁵⁰ Halperin and Clapp (2007, p.139)

element of the policy collage. Ambassador Wolf’s pursuit of the aircraft sale was undertaken at his own behest, and made possible in part by his assurances to the Malaysians that the feuding over the economic caucus would be abandoned at all levels. The relative stability of US-Peruvian relations during Jones’s tenure was supported by the ambassador’s day-to-day management of the low-level fisheries issues as well as his internal work of advocacy, persuasion, and negotiation regarding the aid and recognition decisions made above his jurisdiction. In these instances, the respective COMs’ intragovernmental influence generated credibility in their intergovernmental dealings. The remainder of the analysis considers how the process of ambassadorial turnover causes an intermittent weakening of diplomatic influence in the policy process, and the international implications that follow.

Consequences of Ambassadorial Turnover

Since the early twentieth century, the US State Department has rotated ambassadors in and out of foreign missions on a regular basis. When one ambassador’s tour ends, the President must nominate a successor, the Senate must confirm that successor, and the host government must accept her credentials. If there is any delay (or “vacancy”) between one ambassador’s departure and her successor’s arrival at post (as is almost always the case), oversight of the embassy’s operations passes on to a Foreign Service Officer who serves in the acting role of *chargé d’affaires ad interim*.⁵¹

This paper argues that the removal and replacement of a US ambassador induces a negative shock to diplomatic influence in the US foreign policy process vis-à-vis the country experiencing the turnover. In the following analysis, I first consider why acting officials, as well as appointed officials at the very beginning and end of their tenures, prove less influential as compared to mid-tenure appointees. I then show how this micro-level variation in diplomatic influence can lead to macro-level variation in diplomatic outcomes.

⁵¹ The terms “*chargé d’affaires*”, “*chargé*”, and “*interim*” or “*acting*” official are used interchangeably here. See Appendix [D](#) for more precise discussion of titles.

Continuity during turnover

Before examining the consequences of ambassadorial turnover, it is important to delimit the analysis by noting which aspects of bilateral relations are not interrupted by a turnover in the US ambassadorial post. Considering only the letter of the law, there would be little reason to expect diplomatic influence to wane under a *chargé d'affaires* (and even less so under an incoming or outgoing ambassador). US statutory law is explicit in granting full chief-of-mission authority to interim officials.⁵² The Vienna Convention of 1961—the treaty governing diplomatic representation under international law—likewise states that, “[e]xcept as concerns precedence and etiquette, there shall be no differentiation between heads of mission by reason of their class.”⁵³ In practice, the immediate impact of a US ambassadorial turnover may seem similarly modest, at first glance. Beyond the ambassador herself, nearly all of the embassy’s personnel are assigned and rotated on schedules independent of the ambassador’s.⁵⁴ Further, the US and any given country have a multitude of channels of diplomatic communication available other than the US ambassador stationed abroad—including, most notably, the foreign country’s embassy in DC.⁵⁵

When assessing the impact of a US ambassadorial turnover, it is thus important to keep in mind that any such impact will be limited to those aspects of the job which can neither be effectively performed by lower-level embassy personnel, nor fully substituted by the foreign country’s diplomatic representative in the US—nor clearly stipulated by legal authorities. Within these bounds, however, the potential for bilateral harm is substantial. To see why, we can draw insights from the broader literatures on bureaucratic politics, public administration, and firsthand accounts of foreign policy officials.

⁵² Foreign Service Act (1980), §102(3)

⁵³ Vienna Convention on Diplomatic Relations (1961), Article 14.2.

⁵⁴ Jett (2014, p.177)

⁵⁵ For further discussion of foreign diplomatic representation in the US, see Appendix B.3

Rotation, vacancy, and influence

In embassies, and across federal offices more broadly, acting officials enjoy a narrower window of discretion than do permanent appointees—or at least perceive as much. In a thorough assessment of vacancies in executive branch appointments, O’Connell identifies the primary cost of vacancy to be “agency inaction”: agencies under interim leadership tend to “make fewer policy decisions. . . [and] launch fewer controversial actions”⁵⁶ even when acting officials enjoy the full legal authorities of their Senate-confirmed counterparts. These discretionary limitations are to some degree self-imposed, or imposed by professional norms and expectations: a former acting Under Secretary of Homeland Security recounts that, “[a]s the acting person. . . you’re very much aware that you are temporary. . . so there’s this sense of discomfort about trying to bring organizational change.”⁵⁷ In addition to avoiding change in favor of the status quo, acting officials are also less likely to push back against directives from the White House or decisions by other participants which they believe to be misguided.⁵⁸ A chargé in Bodine’s position would have been legally authorized to deny the chief FBI investigator’s clearance for entry into Yemen, but we can reasonably speculate that she would have opted not to exercise that authority.

Alternatively, inaction or complaisance under an acting official may be explained not by discretionary limitations, but rather by a lack of clout with important stakeholders in the US government and beyond. Acting officials, their professional competence and intra-agency esteem notwithstanding, “will not be as powerful as permanent appointees in dealing with the agency’s major outside constituencies”⁵⁹ and will lack “access to the external network to get what they need from the White House and the other agencies.”⁶⁰ We might think of external influence as a personal attribute of individual diplomats—and in fact, such influence can

⁵⁶ O’Connell (2009, p.938)

⁵⁷ Quoted in Phelps (2019)

⁵⁸ O’Connell (2020, p.696)

⁵⁹ O’Connell (2009, p.942)

⁶⁰ Ibid. p.948

be a major determinant of how an ambassador gets selected in the first place⁶¹—but it can also emerge as a perquisite of the job. Upon commencement of the sales push for aircraft in Malaysia, Wolf quickly “discovered how useful it was to have the first name ‘Ambassador,’ ‘cause I immediately picked up the phone to make several calls” to industry leaders and government officials;⁶² the same access was not afforded to the lower-level embassy positions he had previously occupied.

A diplomat’s influence may be further curtailed by her superiors’ unwillingness to consult her on important policy decisions. Presidents of both parties have at times expressed skepticism or distrust of careerists whom they did not personally appoint. In sensitive deliberations over normalizing relations with China, President Carter was “leery of channeling my proposals through the State Department, because I did not feel that I had full support there”;⁶³ Nixon “loathed the foreign service.”⁶⁴ As a consequence, according to one former ambassador, “[w]ithout the appointees in place, State. . . [is] at a real disadvantage in policy debates.”⁶⁵

While the discussion thus far has focused on the limited influence of interim officials, the concerns carry over in substantial measure to Presidentially-appointed ambassadors at the very beginning or end of their tenures. Newly appointed ambassadors may take a less active role in policy advocacy, or exercise their discretionary authority less freely, until they feel they have adequately “learned the ropes” in the new position.⁶⁶ Many diplomatic initiatives require weeks or months to develop; an incoming ambassador may be delayed in getting ambitious projects underway, and an outgoing ambassador may be unable to see them through to completion (while an interim official is cut short on both ends). Insofar as policy implementation requires collaboration across agencies or bureaus, a COM on her way

⁶¹ This is true of career and non-career appointees alike (Jett, 2014, ch.3-4)

⁶² ADST: John Wolf (p.85)

⁶³ Quoted in Halperin and Clapp (2007, p.245)

⁶⁴ Kopp and Gillespie (2011, p.10)

⁶⁵ Tucker (2017)

⁶⁶ O’Connell (2009, p.938)

out the door will find it difficult to call in favors which she will not have the opportunity to reciprocate, and a new entrant to the job will need time to build relationships and goodwill. Altogether, these factors render a transitional chief of mission less influential than a long-term appointee in shaping US policy toward her host country.

Turnover and commitment problems

The intragovernmental dynamics described above carry implications for intergovernmental relations. From a foreign government’s perspective, a US ambassadorial turnover generates a commitment problem: the diplomat assigned to the country may seek bilateral cooperation, but her commitment to manifest that preference in action depends on her ability to influence US policy. If she lacks influence internally, her commitments will lack credibility. I consider how these dynamics impact the bilateral propensity for militarized conflict and economic exchange.

Commitment and conflict

My analysis of commitment and conflict follows from a standard bargaining framework, in which the US and a foreign government find themselves in disagreement over an international policy issue. Either side has the option to escalate from quiet diplomacy to the public threat or use of force,⁶⁷ and will do so if its expected utility of escalation exceeds that of accepting a negotiated settlement (or of abiding the status quo while working towards one). Militarized escalation may prove effective at winning the issues in contention, but invariably carries some cost for the overall diplomatic relationship. Disaggregating the state as we have in the preceding discussion, we observe that participants across the US government vary in their relative aversion to militarization as a bargaining tactic; it is the diplomats who are the most strongly motivated to find mutually agreeable settlements short of hostilities, and

⁶⁷ For clarification, I use the term “escalation” to refer to the onset of a crisis or dispute, rather than escalation of an existing dispute to higher levels of hostility or war; c.f. [Reed \(2000\)](#)

the most willing to incur the tradeoffs necessary to achieve that goal. The disempowerment of diplomatic agents in the policy process thus implies that the prevailing policy will more closely reflect the interests of participants who are less averse to using force against a given country, or less inclined to accommodate that country's demands for the sake of avoiding a downturn in relations.

Broadly speaking, a US chief of mission can mitigate the risk of conflict through the commitments she makes to her host government: she can commit to altering a US policy which, if unchanged, would push her host government toward escalation; or she can commit to providing some compensation or side payment which would induce her host government to cede the issue in contention and thus preempt the need for escalation by the US. These policy changes may require decisions which the COM has the discretion to enact independently, or decisions which she will need to convince her superiors to approve of, or both. In either case, however, an acting or transitional official will lack credibility in making the necessary commitments, if it is commonly understood that she lacks the influence necessary to carry out her end of the bargain. An ambassadorial turnover thus increases the likelihood that a bilateral disagreement eludes resolution through quiet diplomacy and negotiation, and ultimately gives rise to a militarized dispute. These dynamics are examined more thoroughly in a set of case studies following the quantitative analysis.

Commitment and trade

A second consequence of ambassadorial turnovers can be found in their impact on bilateral economic cooperation. Here I focus specifically on US exports abroad, as this is one of the responsibilities explicitly delineated in the Foreign Service Act: "Each chief of mission to a foreign country," states §3927(c), "shall have as a principal duty the promotion of United States goods and services for export to such country."⁶⁸ We should note that the concept of economic diplomacy usually entails two distinct kinds of activity, the first being diplomacy

⁶⁸ Foreign Service Act (1980)

in pursuit of broader institutional reforms or policy changes, and the second (alternatively referred to as “commercial diplomacy”) being diplomacy to promote specific transactions or resolve specific disputes.⁶⁹ The present focus is on the latter, which should translate more directly to short-term shocks in trade flows.

As in the preceding discussion of commitment and conflict, turnovers can undermine a COM’s ability to make credible commitments to her host government in the domain of economic cooperation. The Malaysian case is instructive in this regard. Throughout his time in Kuala Lumpur, Ambassador Wolf continually worked to impress upon his host government “that Malaysia had a worthy friend in the United States, and that there could be unique advantages to a strong relationship with the US.”⁷⁰ In doing so, Wolf sought to incorporate into the aircraft deal considerations beyond just the price and quality of the goods in that particular transaction: he encouraged the Malaysians to envision the full range of cooperative opportunities, and the many forms of potential reciprocity, which might follow from that exchange. The delivery of any such benefits would not be without cost for the US diplomat, and would likely require buy-in from other participants in the policy process—but those were hurdles that Wolf was willing and able to overcome. These types of diffuse or implicit commitments will be less attractive coming from a more transitory and less influential chief of mission.

In other situations, interstate cooperation is facilitated by credible commitments of punishment for defection from a cooperative arrangement. A primary obstacle to reciprocal cooperation in trade is the ever-present incentive for governments to opportunistically restrict foreign imports—through means ranging from tariffs and non-tariff barriers to preferential procurement practices and failure to enforce contracts—for the benefit of domestic producers.⁷¹ Governments will generally abstain from such behavior only if they expect it to be met with retaliation; but adjudication of disputes through formal institutions is

⁶⁹ Keeley (2000, p.86)

⁷⁰ ADST: John Wolf (p.79)

⁷¹ Goldstein and Martin (2000)

costly⁷² and cooperation must often be enforced by informal and extra-institutional means. Gertz argues that ambassadors can facilitate resolution of investment disputes by “linking specific investor complaints to the broader diplomatic relationship,”⁷³ and the same logic applies to trade disputes as well. Anecdotes abound of ambassadors intervening to prevent opportunistic behavior by their host governments: one ambassador recalls lobbying the Spanish government to protect the intellectual property rights of American computer and pharmaceutical companies, and to ease a dubbing license requirement that harmed American entertainment exports; another recounts pressuring the Mexican government to overturn several non-transparent bid awards for government contracts, which were ultimately granted to American firms.⁷⁴ Even in post-NAFTA Canada, Ambassador Blanchard had to intervene on a “commodity-by-commodity, case-by-case basis” to protect US exports ranging from wheat and lumber to magazines and televisions stations.⁷⁵ A chief of mission unable to commit to future retaliatory measures, or to induce other bureaucratic actors to share the punitive burden, is likely to find her host government behaving less favorably toward US exporters in various small ways—none of which may rise to the level of warranting a formal proceeding, but which collectively amount to a reduction in exports during the turnover period.

Careerists and political appointees

The discussion thus far has elided the distinction between “career” and “political” ambassadors—that is, ambassadors who did or did not rise through the ranks of the Foreign Service—a distinction which has been the focus of some recent empirical research on US ambassadors⁷⁶ as well as presidential appointments more broadly.⁷⁷ Throughout the period of analysis,

72 Davis (2012)

73 Gertz (2018)

74 Keeley (2000, p.88-95)

75 Blanchard (1998, p.126; 139-145)

76 Hollibaugh (2015); Jett (2014)

77 Hollibaugh et al. (2014)

about 26% of embassies at any point in time are overseen by non-career ambassadors (as compared to 63% by career ambassadors and 11% by acting officials). Ambassadors of either type have the same statutory authorities and are appointed through the same formal processes, though the informal means of selection differ.⁷⁸

The routine appointment of non-career ambassadors is largely unique to modern American diplomatic practice, and the empirical implications it generates are not entirely clear. With respect to policy preferences, political appointees are not the “types” who select into the Foreign Service, nor have they experienced the socialization that occurs over the course of a career there; but insofar as Miles’ Law operates in this domain, we might expect that, once they find themselves sitting in an embassy overseas, non-career ambassadors will come to adopt similar stances as do careerists. As for bureaucratic influence, we might expect non-career ambassadors to more closely resemble career ambassadors than acting officials, on average, simply by virtue of their status as presidential appointees; indeed, as Halperin and Clapp note, “[t]he single most important determinant of the influence of any senior official is his or her relationship with the president.”⁷⁹ On the other hand, previous research has shown non-career ambassadors to be systematically less qualified for the position than careerists.⁸⁰ Lacking experience in operating the levers of bureaucracy, or lacking familiarity with their receiving country or region, these appointees are likely to be less effective in influencing US policy to achieve their objectives, whatever those objectives may be. Given ambiguous theoretical predictions, I leave it as an empirical matter to assess whether career and non-career appointments (and their subsequent turnovers) have similar impacts on trade and conflict outcomes.

78 Jett (2014, ch.3-4)

79 Halperin and Clapp (2007, p.226)

80 Scoville (2019)

Research Design

We now turn to an empirical assessment of the theoretical propositions developed above: that ambassadorial turnovers increase the risk of onset of militarized disputes, and decrease volumes of US exports.

Sample selection, measurement, and OLS specification

The first step of the research design is defining the sample to be analyzed. I begin with the sample of sovereign country-year observations, as defined by the Correlates of War, from 1960 to 2014. From this sample I identify the subset characterized by normal diplomatic relations with US. The decision of whether or not to establish diplomatic relations is a strategic one, considered in prior literature as a long-term political investment or as a marker of international status.⁸¹ Examination of these strategic decisions is beyond the scope of this paper; rather, I limit the analysis to conditions of already-established diplomatic relations—which characterizes the vast majority of country-years from 1960 to present—and consider the non-strategic sources of variation in diplomatic representation under those conditions.

Specifically, I define the variable $Eligible_{i,t}$, which takes a value of zero if any of the following hold (and one otherwise): the US has not yet recognized the independence of a foreign country and exchanged ambassadors with that country; once-normal diplomatic relations with that country are severed or otherwise interrupted; or the US does not have an embassy operating in that country with a resident ambassador. I restrict the sample of analysis to the eligible country-year observations. The goal of this sample selection is a more credible estimation of causal effects, for a slightly more narrowly defined population. A thorough discussion of this decision, along with the precise coding of “normal” relations, is provided in the appendix.

⁸¹ Neumayer (2008); Fordham (2011); Duque (2018); Kenkel (2018)

Outcomes

This study examines two primary outcomes.⁸² First, an ambassadorial turnover is predicted to increase the likelihood that the US or host country resorts to militarized escalation in order to resolve a policy incompatibility. I operationalize this concept of escalation using onsets of Militarized Interstate Disputes, or MIDs. MIDs are defined to be “cases of conflict in which the threat, display or use of military force short of war” by one state actor is explicitly directed towards another.⁸³ These incidents are treated in the literature as “symbolic acts in a bargaining process”⁸⁴ or as indicating the emergence of a “crisis” which may or may not advance further to a state of war.⁸⁵ I make no prediction relating the influence of diplomatic agents to the outcomes of these disputes. Of interest here is simply the question of whether, holding constant the political and structural conditions that determine the dyadic propensity for conflict, a militarized dispute becomes more likely to occur as a result of an ambassadorial turnover. To study the ambassador’s impact on dispute initiation, I use as an outcome measure $MID\text{Onset}_{i,t}$, an indicator for whether country i entered into a MID in year t in which the US was an opposing participant.

The second outcome of interest is the annual volume of US exports to a given host country, which are predicted to decrease when US commercial diplomacy efforts are hampered by an ambassadorial turnover. Specifically, $US\text{Exports}_{i,t}$ is the constant USD value of all US exports in goods to country i in year t , taken from the Correlates of War Project Trade Dataset. All analyses use the common transformation of $\ln(US\text{Exports}_{i,t} + 1)$.

⁸² Coverage for the MID outcome ends in 2010, while coverage for the trade outcome extends to 2014, so the sample sizes differ by outcome.

⁸³ Jones et al. (1996)

⁸⁴ Maoz and Russett (1993)

⁸⁵ Reed (2000)

Turnover

The independent variable of interest is ambassadorial turnover, which I operationalize in two ways. First, $Turnover_{i,t}$ is an indicator for whether the ambassadorial post in country i experienced any vacancy in year t , regardless of the length of the vacancy. It takes a value of zero when the same presidentially-appointed ambassador is in office for the entire year, and one otherwise. Thus it indicates that a country-year saw one or more of (1) the end of an outgoing ambassador’s tenure, (2) a “vacancy” with an acting official in charge, and (3) the start of an incoming ambassador’s tenure. This operationalization follows from the preceding discussion of turnovers themselves, including the adjustment periods shortly before and after a vacancy, being causes of disruption in diplomatic relations. For robustness, additional tests consider a continuous measure, $Vacancy_{i,t}$, which is the portion of year t that country i goes without an ambassador. These variables were constructed by scraping the State Department’s “Chiefs of Mission By Country” database and recording the start and end dates for all presidentially-appointed ambassadors.⁸⁶ “Career” and “political” ambassadors are pooled together for the main analyses, and later disaggregated, as discussed below.

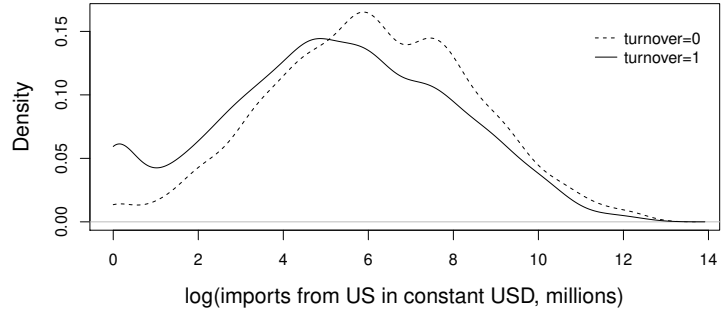
Bivariate relationships

As a first pass at assessing the relationships between ambassadorial turnovers and the outcomes of interest, consider the conditional distributions depicted in Figure [I](#). The upper panel includes all country-years, and the lower panel is restricted to the sample of analysis (the country-years characterized by normal diplomatic relations). For both outcomes, the bivariate relations are more pronounced in the full sample than in the sample of analysis, suggesting that the sample selection process does away with one major source of confounding. For the conflict outcome, it is clear in both samples that MID events are more likely to occur in turnover years than in non-turnover years; the main inferential challenge in testing this paper’s argument is to demonstrate that the relationship is causal, with turnovers caus-

⁸⁶ See appendix for coding details.

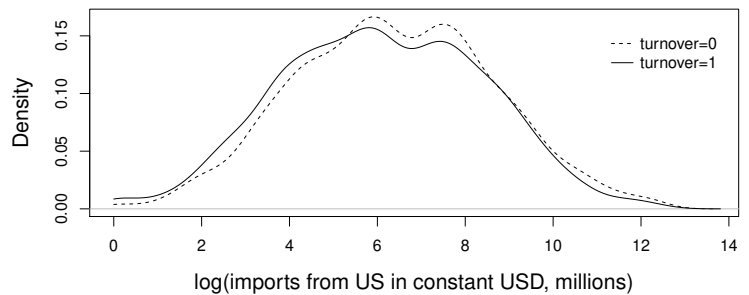
Figure 1: Conditional distributions of MID Onset $_{i,t}$ and $\log(\text{Imports from US}_{i,t})$, by turnover status

		MID Onset $_{i,t}$	
		0	1
Turnover $_{i,t}$	0	4088 (99.03%)	40 (0.97%)
	1	3973 (96.69%)	136 (3.31%)



(a) All country-years

		MID Onset $_{i,t}$	
		0	1
Turnover $_{i,t}$	0	3557 (99.03%)	35 (0.97%)
	1	2636 (98.10%)	51 (1.90%)



(b) Normal relations

ing MID s and not simply the reverse. For the trade outcome, the difference in conditional distributions in the sample of analysis is more subtle; in addition to causality concerns, a challenge in the trade analyses will be to gain the statistical power needed to distinguish the effect of commercial diplomacy from the geopolitical and macroeconomic factors that drive the aggregate flows.

OLS specification

Moving beyond the descriptive patterns presented above, we can consider the following generic OLS model of the relationship between turnovers and outcomes:

$$Y_{it} = Turnover_{it}\beta + X_{i,t-1}\theta + Y_{i,t-1}\phi + \alpha_i + \tau_t + \tau_t \times Political\ Appointee_{i,t} + \epsilon_{it},$$

where α_i are country fixed effects, τ_t are year fixed effects which are interacted with a political appointee indicator (as explained below), and $X_{i,t-l}$ is a vector of lagged controls specific to the outcomes of interest. Data sources and summary statistics for all variables are listed in Table A1.

To fully specify the MID onset model, we will want to account for other factors that have been studied as predominant drivers of conflict.⁸⁷ These include regime type, measured by *polity2* score; democratic transitions, measured as change in polity; economic interdependence, in the form of bilateral trade flows; an index of military capabilities; a mutual defense alliance indicator; and foreign policy alignment, measured by UNGA voting affinity. Country fixed effects account for other common controls such as shared borders and geographic distance.

For the export model, a common approach to testing the effects of economic diplomacy is to incorporate the diplomatic variable of interest into a gravity model of trade.⁸⁸ In line with this literature, I control for the foreign country's population and GDP, as well as regime type (polity) and indicators for whether that country is party to a free trade agreement with the US or a member of the GATT or WTO. Geographic distance and contiguity are again subsumed by country fixed effects, and US population and GDP are subsumed by year fixed effects.

An important consideration in both models is the potential confounding influence of US

⁸⁷ Even if these controls are not needed for unconfoundedness, their inclusion can improve the precision of the estimated effect of turnover. Robustness checks drop these controls.

⁸⁸ See [Moons and van Bergeijk \(2017\)](#) for a review.

election cycles on the relationship between ambassadorial appointments and foreign policy behavior. This concern is primarily addressed by inclusion of year fixed effects; but because these cyclical patterns may differentially affect countries that receive career and non-career ambassadors, I also include the interaction of year fixed effects with an indicator of what type of appointee a given country has most recently received prior to the start of year t .⁸⁹ In addition to the covariates mentioned above, I also include a lagged dependent variable in each model, to account for the possibility of turnovers being strategically manipulated in anticipation of future trade or conflict outcomes; anticipated outcomes are unobservabale, but recent outcomes provide a reasonable proxy.

IV design

The fundamental concern with the OLS specification outlined above is that it falls short of addressing any unobserved confounding in the relationship between turnovers and outcomes. To see why this is the case, consider the various reasons why the turnover and vacancy measures would take on a non-zero value. First, as previously mentioned, ambassadors are routinely rotated between different foreign missions and positions back in Washington. Second, incoming presidents typically dismiss the non-career ambassadors appointed by their predecessors (and sometimes the careerists as well), so as to replace them with their own political allies. Third, an ambassador may be removed due to poor performance (or conversely, due to strong performance leading to promotion). Fourth, ambassadors may be withdrawn as part of a larger strategy of diplomatic sanction or protest against the host country.

The confounding potential of the latter two causes of turnover should be clear, as these are situations in which the ambassador is withdrawn for reasons related, perhaps directly, to the outcomes of interest. In the former two cases, the primary concern is that the duration of the vacancy between appointments (as well as any deviation from the routine rotation

⁸⁹ So the models effectively include two fixed effects for each year: a career-appointee-year effect, and a political-appointee-year effect. See Appendix [B.2](#) for further discussion of electoral cycles in appointments.

schedule) may correlate with unobservable political interests and priorities. Furthermore, these confounding factors may bias OLS estimates in either direction. An ambassador may be withdrawn in anticipation of a militarized dispute, inducing a positive OLS coefficient (which certainly does not capture the causal impact of the turnover); or alternatively, an ambassador may be kept in place beyond her scheduled departure date, or a vacancy may be filled promptly, precisely because a conflict seems imminent—or because it has already begun—in which case the bias deflates OLS estimates toward zero. Similar considerations may apply to trade outcomes as well: vacancies may be left open for an extended period only when doing so is not harmful for US exports, or alternatively when opportunities for commerce are already bleak, yielding potentially countervailing biases.

The strategy I propose to address this problem gains causal leverage by effectively isolating the variation in turnovers that arises solely from the routinized rotation system. Regular rotation of Foreign Service Officers between postings at foreign missions and back home at State Department headquarters was first codified in the Rogers Act of 1924;⁹⁰ in practice, this converged to a standardized three-year appointment system (for career and non-career ambassadors alike) by the latter half of the twentieth century.⁹¹ Though ambassadors may occasionally hold office for a longer or shorter period, the first-stage results reported below demonstrate that the norm of a three-year term is strongly adhered to.

I create the instrument $Enter_{i,t-3}$, an indicator for whether any ambassador entered office in country i in year $t - 3$, and use it as an exogenous predictor of turnover in year t . Using this instrument provides two distinct advantages over the OLS regression, corresponding to two potential sources of endogeneity in the turnover measure: endogenous vacancy *onset*, arising from strategic manipulation of ambassadorial *tenure*; and endogenous vacancy *duration*, arising from strategic manipulation of ambassadorial *appointment*. To the first point, the two-stage least squares estimator only recovers the local average treatment effect

⁹⁰ Office of the Historian (2019)

⁹¹ Jett (2014)

(LATE) for the population of compliers, which here refers to the “types” of country-years for which turnovers—both observed and counterfactual—follow the standard rotation schedule. Cases in which ambassadors are dismissed early or retained beyond the three-year norm are effectively omitted from the IV estimation. The intuition behind the second point is that the appointment process is much more likely to be manipulated with respect to contemporaneous outcomes, as compared to anticipated outcomes three years into the future. In other words, we would expect outcomes in year t to be more weakly related to the unobservable confounders at the time of appointment in year $t - 3$, as compared to potential confounders in year t .

This latter consideration, in more general terms, points to the requirement of conditionally independent assignment of the instrument with respect to potential outcomes. My approach to fully satisfying this criterion involves controlling for the same lagged covariates from the OLS models detailed above (all lagged to $t - 4$ rather than $t - 1$ to avoid post-treatment bias), as well as lagged dependent variables,⁹² and including country and year fixed effects (the latter interacted with *Political Appointee* _{$i,t-3$}). In addition, to capture any remaining unobservable confounding, I control for the total vacancy that the post experienced in years $t - 6$ through $t - 4$ (labelling this variable *Prior Vacancy* _{$i,t-4:t-6$}), a period which represents a full ambassadorial “life cycle”. In the normal conduct of diplomatic relations, at least one vacancy will typically occur during this three-year span preceding year $t - 3$; how quickly that vacancy gets filled should provide a strong proxy measure for unobservable diplomatic priorities. This IV strategy thus rests on the identifying assumption that, within the sample characterized by normal diplomatic relations and conditional on covariates, the systematic variation in *Enter* _{$i,t-3$} arises simply from the previous history of a given ambassadorial post’s rotation schedule: that is, whether this post experienced turnover in years $t - 6$, $t - 9$, and $t - 12$, as opposed to years $t - 7$, $t - 10$, and $t - 13$, and so on.

⁹² The main text results include three lags of the dependent variables ($t - 6$, $t - 5$, $t - 4$), to account for cyclical variation in these outcomes due to ambassadorial rotation cycles; results are robust to dropping these.

As in any observational study, the claim of independent assignment of treatment cannot be definitively proven. There are, however, two pieces of supporting evidence that should strongly mitigate against concerns of endogeneity. First, Table A12 checks for relationships between the $Enter_{i,t-3}$ instrument and any of the pre-treatment covariates, after residualizing over country and year intercepts. The results show near-zero correlations with each variable except for $Prior\ Vacancy_{i,t-4:t-6}$: the only predictor of an ambassadorial entrance in a given year is the post’s recent history of vacancy. To more flexibly control for this potential confounder, I include quadratic and cubic terms of $Prior\ Vacancy_{i,t-4:t-6}$ in all specifications. The second piece of evidence against endogenous assignment is the placebo test shown in Figure 2, and discussed in detail after presentation of the main results.

The two stages of the IV estimation take the following form:

$$\begin{aligned}
Turnover_{it} &= \gamma Enter_{i,t-3} + f(Prior\ Vacancy_{i,t-4:t-6}) + Y_{i,t-4}\phi_1 + Y_{i,t-5}\phi_2 + Y_{i,t-6}\phi_3 \\
&\quad + X_{i,t-4}\theta_1 + \rho_i + \lambda_t + \lambda_t \times Political\ Appointee_{i,t-3} + \eta_{it} \\
Y_{it} &= \beta \widehat{Turnover}_{i,t} + f(Prior\ Vacancy_{i,t-4:t-6}) + Y_{i,t-4}\phi_4 + Y_{i,t-5}\phi_5 + Y_{i,t-6}\phi_6 \\
&\quad + X_{i,t-4}\theta_2 + \alpha_i + \tau_t + \tau_t \times Political\ Appointee_{i,t-3} + \epsilon_{it},
\end{aligned}$$

where $f(\cdot)$ indicates a third-degree polynomial, with outcome variables and covariates as described above. As in the OLS specification, all models will be estimated separately with the binary $Turnover_{i,t}$ and the continuous $Vacancy_{i,t}$. The sample will be limited to observations of $Eligible_{i,t-3} = 1$ (rather than $Eligible_{i,t} = 1$) so as to avoid selecting on a post-treatment variable. Summary statistics of all variables, along with characterizations of the effective sample and the IV compliers, are provided in Table A1.

Empirical Results

The main empirical results are reported in Tables 1 and 2 below. First consider the OLS results for MID onsets in Table 1. The first column reports an approximately one percentage

point increase in the probability of MID onset associated with an ambassadorial turnover. The second column confirms this result using the continuous vacancy measure; the median vacancy duration is about 100 days, so an estimate of $\hat{\beta}_{Pct.Vacant}$ that is three to four times as large as $\hat{\beta}_{Turnover}$ is intuitively reasonable.⁹³

Columns 3 and 4 report the first stage of the IV estimation, regressing turnover and vacancy (respectively) on the $Enter_{i,t-3}$ indicator and all other covariates. Consistent with the three-year rotation schedule, the appointment of an ambassador in year $t - 3$ is a strong predictor of turnover and vacancy in year t . This first-stage relationship allows for the second-stage IV estimation, which is reported in Columns 5 and 6. Coefficients are again positive and statistically significant, and in this case larger in magnitude than those estimated by OLS: an ambassadorial turnover is estimated to have a causal impact of increasing by 2.2pp the probability of MID onset with the country experiencing the turnover.

⁹³ Distributions of the vacancy measures are presented in Figure [A2](#).

Table 1: MID Onset, OLS and IV

DV:	OLS		DV:	First Stage		2SLS	
	MID Onset $_{i,t}$			Turnover $_{i,t}$	Vacancy $_{i,t}$	MID Onset $_{i,t}$	
	(1)	(2)		(3)	(4)	(5)	(6)
Turnover $_{i,t}$	0.009 (0.003) p = 0.005		Turnover $_{i,t}$		0.022 (0.011) p = 0.040		
Vacancy $_{i,t}$		0.027 (0.010) p = 0.005	Vacancy $_{i,t}$				0.095 (0.047) p = 0.044
			Enter $_{i,t-3}$	0.246 (0.019) p = 0.000	0.058 (0.007) p = 0.000		
log Imports from US $_{i,t-1}$	-0.004 (0.003) p = 0.280	-0.003 (0.003) p = 0.319	log Imports from US $_{i,t-4}$	0.010 (0.010) p = 0.308	0.008 (0.009) p = 0.396	-0.003 (0.004) p = 0.560	-0.003 (0.004) p = 0.479
log Exports to US $_{i,t-1}$	-0.001 (0.002) p = 0.646	-0.001 (0.002) p = 0.671	log Exports to US $_{i,t-4}$	-0.002 (0.006) p = 0.738	-0.005 (0.006) p = 0.404	0.002 (0.003) p = 0.380	0.003 (0.003) p = 0.288
UNGA Ideal Diff $_{i,t-1}$	0.005 (0.005) p = 0.330	0.004 (0.005) p = 0.376	UNGA Ideal Diff $_{i,t-4}$	0.016 (0.011) p = 0.147	0.020 (0.009) p = 0.021	0.004 (0.004) p = 0.298	0.003 (0.004) p = 0.492
Polity $_{i,t-1}$	-0.001 (0.001) p = 0.035	-0.001 (0.001) p = 0.033	Polity $_{i,t-4}$	0.001 (0.001) p = 0.602	0.001 (0.001) p = 0.599	-0.001 (0.001) p = 0.041	-0.001 (0.001) p = 0.038
Δ Polity $_{i,t-1}$	0.0003 (0.001) p = 0.783	0.0004 (0.001) p = 0.717	Δ Polity $_{i,t-4}$	-0.001 (0.003) p = 0.834	-0.002 (0.002) p = 0.339	0.001 (0.001) p = 0.160	0.001 (0.001) p = 0.105
Capabilities $_{i,t-1}$	3.878 (0.481) p = 0.000	3.903 (0.488) p = 0.000	Capabilities $_{i,t-4}$	-0.003 (0.651) p = 0.996	-0.356 (0.466) p = 0.446	3.891 (0.608) p = 0.000	3.925 (0.614) p = 0.000
Ally $_{i,t-1}$	0.015 (0.007) p = 0.037	0.016 (0.007) p = 0.027	Ally $_{i,t-4}$	0.028 (0.029) p = 0.347	-0.017 (0.023) p = 0.464	0.015 (0.007) p = 0.023	0.017 (0.008) p = 0.024
Prior Vacancy $_{i,t-3:t-1}$	0.016 (0.014) p = 0.243	0.015 (0.014) p = 0.295	Prior Vacancy $_{i,t-6:t-4}$	0.031 (0.056) p = 0.579	0.028 (0.037) p = 0.441	-0.001 (0.016) p = 0.940	-0.003 (0.016) p = 0.836
Prior Vacancy $^2_{i,t-3:t-1}$	-0.021 (0.017) p = 0.217	-0.021 (0.017) p = 0.227	Prior Vacancy $^2_{i,t-6:t-4}$	-0.041 (0.066) p = 0.532	-0.051 (0.049) p = 0.289	0.005 (0.016) p = 0.767	0.009 (0.016) p = 0.577
Prior Vacancy $^3_{i,t-3:t-1}$	0.006 (0.005) p = 0.229	0.005 (0.005) p = 0.258	Prior Vacancy $^3_{i,t-6:t-4}$	0.012 (0.018) p = 0.514	0.015 (0.015) p = 0.314	-0.002 (0.004) p = 0.638	-0.003 (0.004) p = 0.441
MID Onset $_{i,t-1}$	0.099 (0.040) p = 0.015	0.096 (0.041) p = 0.018	MID Onset $_{i,t-4}$	-0.052 (0.064) p = 0.417	-0.033 (0.030) p = 0.273	0.082 (0.042) p = 0.053	0.084 (0.041) p = 0.044
MID Onset $_{i,t-2}$	0.093 (0.045) p = 0.040	0.092 (0.045) p = 0.042	MID Onset $_{i,t-5}$	0.029 (0.070) p = 0.677	0.045 (0.036) p = 0.205	0.041 (0.038) p = 0.278	0.037 (0.038) p = 0.322
MID Onset $_{i,t-3}$	-0.005 (0.042) p = 0.906	-0.005 (0.043) p = 0.908	MID Onset $_{i,t-6}$	0.008 (0.066) p = 0.908	-0.014 (0.031) p = 0.656	-0.004 (0.035) p = 0.915	-0.002 (0.034) p = 0.947
Observations	6,279	6,279	Observations	6,279	6,279	6,279	6,279

Note: All models include country FE, year FE, and year FE \times *Political Appointee* $_{i,t-3}$. Standard errors clustered by country, with p-values from a two-sided t-test. First-stage F-statistics are 161.05 for turnover (column 3) and 63.40 for vacancy (column 4).

Turning to Table 2, we see a similar pattern in the relationship between turnovers and US exports. Because the outcome is log transformed, sufficiently small coefficient values can be interpreted as percentage changes: by OLS, an ambassadorial turnover is estimated to decrease exports by 2.3%, while the 2SLS estimates an effect of 10%. The coefficient on the continuous vacancy variable is again about four times the magnitude of the binary coefficient (for both OLS and 2SLS), and the first stage results are similar to those reported in Table 1. For substantive interpretation, the effect size is best understood relative to the variability in the outcome measure: by the 2SLS estimate in Column 14, a turnover causes a decrease of .04 standard deviations of log exports; after residualizing the outcome over country and year intercepts, the effect amounts to 0.17 standard deviations.

Table 2: US Exports, OLS and IV

DV:	OLS		DV:	First Stage		2SLS	
	log Imports from US _{<i>i,t</i>}			Turnover _{<i>i,t</i>}	Vacancy _{<i>i,t</i>}	log Imports from US _{<i>i,t</i>}	
	(7)	(8)		(9)	(10)	(11)	(12)
Turnover _{<i>i,t</i>}	-0.023 (0.012) p = 0.061		Turnover _{<i>i,t</i>}			-0.101 (0.043) p = 0.018	
Vacancy _{<i>i,t</i>}		-0.106 (0.035) p = 0.003	Vacancy _{<i>i,t</i>}				-0.463 (0.197) p = 0.019
			Enter _{<i>i,t-3</i>}	0.259 (0.019) p = 0.000	0.057 (0.007) p = 0.000		
log GDP _{<i>i,t-1</i>}	0.044 (0.029) p = 0.132	0.046 (0.029) p = 0.107	log GDP _{<i>i,t-4</i>}	0.035 (0.018) p = 0.051	0.026 (0.015) p = 0.091	0.105 (0.040) p = 0.009	0.114 (0.039) p = 0.004
log Population _{<i>i,t-1</i>}	0.039 (0.033) p = 0.238	0.037 (0.033) p = 0.269	log Pop _{<i>i,t-4</i>}	-0.015 (0.020) p = 0.441	-0.030 (0.013) p = 0.019	-0.004 (0.042) p = 0.928	-0.016 (0.042) p = 0.700
log Exports to US _{<i>i,t-1</i>}	0.028 (0.011) p = 0.012	0.027 (0.011) p = 0.013	log Exports to US _{<i>i,t-4</i>}	-0.004 (0.006) p = 0.552	-0.008 (0.006) p = 0.190	0.031 (0.017) p = 0.060	0.028 (0.016) p = 0.077
Polity _{<i>i,t-1</i>}	0.001 (0.002) p = 0.515	0.001 (0.001) p = 0.531	Polity _{<i>i,t-4</i>}	0.001 (0.001) p = 0.679	-0.0003 (0.001) p = 0.785	0.003 (0.003) p = 0.331	0.003 (0.003) p = 0.363
FTA _{<i>i,t-1</i>}	0.094 (0.035) p = 0.007	0.095 (0.035) p = 0.007	FTA _{<i>i,t-4</i>}	-0.007 (0.038) p = 0.858	0.008 (0.018) p = 0.676	0.263 (0.075) p = 0.000	0.267 (0.075) p = 0.000
GATT/WTO _{<i>i,t-1</i>}	0.020 (0.026) p = 0.438	0.020 (0.026) p = 0.435	GATT/WTO _{<i>i,t-4</i>}	-0.038 (0.018) p = 0.039	-0.027 (0.014) p = 0.061	0.035 (0.057) p = 0.537	0.027 (0.057) p = 0.636
Prior Vacancy _{<i>i,t-3:t-1</i>}	-0.058 (0.052) p = 0.263	-0.055 (0.052) p = 0.286	Prior Vacancy _{<i>i,t-6:t-4</i>}	0.020 (0.057) p = 0.719	0.009 (0.036) p = 0.798	-0.113 (0.083) p = 0.171	-0.111 (0.082) p = 0.179
Prior Vacancy ² _{<i>i,t-3:t-1</i>}	0.053 (0.062) p = 0.399	0.054 (0.062) p = 0.384	Prior Vacancy ² _{<i>i,t-6:t-4</i>}	-0.030 (0.065) p = 0.641	-0.027 (0.046) p = 0.562	0.142 (0.094) p = 0.132	0.132 (0.094) p = 0.161
Prior Vacancy ³ _{<i>i,t-3:t-1</i>}	-0.017 (0.017) p = 0.296	-0.016 (0.016) p = 0.323	Prior Vacancy ³ _{<i>i,t-6:t-4</i>}	0.008 (0.018) p = 0.665	0.008 (0.015) p = 0.591	-0.039 (0.025) p = 0.113	-0.036 (0.025) p = 0.155
log Imports from US _{<i>i,t-1</i>}	0.596 (0.044) p = 0.000	0.593 (0.044) p = 0.000	log Imports from US _{<i>i,t-4</i>}	-0.010 (0.016) p = 0.543	-0.007 (0.010) p = 0.456	0.348 (0.032) p = 0.000	0.346 (0.031) p = 0.000
log Imports from US _{<i>i,t-2</i>}	0.114 (0.031) p = 0.000	0.114 (0.031) p = 0.000	log Imports from US _{<i>i,t-5</i>}	0.014 (0.020) p = 0.492	-0.0003 (0.009) p = 0.978	0.080 (0.019) p = 0.000	0.078 (0.019) p = 0.000
log Imports from US _{<i>i,t-3</i>}	0.051 (0.024) p = 0.032	0.052 (0.023) p = 0.026	log Imports from US _{<i>i,t-6</i>}	-0.002 (0.014) p = 0.913	0.008 (0.007) p = 0.240	0.030 (0.020) p = 0.146	0.034 (0.021) p = 0.102
Observations	6,768	6,768	Observations	6,768	6,768	6,768	6,768

Note: All models include country FE, year FE, and year FE \times *Political Appointee*_{*i,t-3*}. Standard errors clustered by country, with p-values from two-sided t-test. First-stage F-statistics are 178.08 for turnover (column 9) and 59.09 for vacancy (column 10).

It is notable that for both outcomes and both vacancy measures, the IV estimates are larger in magnitude than the OLS estimates. These differences are likely attributable to one (or both) of two general explanations. First is a net deflationary bias in the OLS estimates, which the IV approach corrects for: the discussion above outlined reasons why we might expect strategic manipulation of appointments to bias OLS estimates either toward or away from zero, and it turns out that the bias toward zero dominates. Substantively, this would suggest that vacancies are avoided in situations where they would be the most harmful to bilateral relations. A second explanation, which can hold even in the absence of bias in the OLS, would attribute the difference in the estimates to the basic differences in the estimands: the IV estimator can only recover average treatment effects local to the subpopulation of compliers, and it turns out that the compliers have larger treatment effects than does the population as a whole. In other words, the countries that most closely adhere to the three-year rotation schedule are also the countries for which bilateral relations are most heavily impacted by the status of the US chief of mission. Comparing the covariate profiles of the compliers and the full sample,⁹⁴ we do find some differences consistent with this latter explanation. Both accounts seem intuitively plausible and it would be difficult to rule out either one.

Heterogeneous effects by appointee type

To assess whether career appointees prove more effective than non-career appointees in promoting trade and preventing conflict, we can test for heterogeneous effects across different types of ambassadors and their respective removal. This question does not lend itself to a straightforward empirical specification; one approach is reported here, with others reported in Tables A5 and A6 of the appendix. The approach of Table 3 involves recoding the instrument into two separate measures, which separately indicate whether a career ambassador, or a political ambassador, entered office in year $t - 3$. Columns 13 and 16 report the first-stage

⁹⁴ See Appendix [A.2](#).

estimation, where we see that an entrance of either type is a strongly significant predictor of turnover in year t . Columns 15 and 18 show the reduced-form effect of the entrance instruments on the respective outcomes, and we see the same pattern in both cases: only a career entrance has a significant effect, while the effect of a political entrance is smaller and less precisely estimated; the effect of a career entrance in both cases closely resembles the effect of the pooled entrance instrument used in the main analyses (with the reduced-form effect reported in columns 14 and 17 for comparison). Alternative specifications reported in the appendix include interacting the instrument with appointee type, and splitting the sample by appointee type, and they reveal a similar pattern.

Table 3: MID Onset and US Exports, Career vs. Political Appointees

DV:	First Stage	Reduced Form		DV:	First Stage	Reduced Form	
	Turnover $_{i,t}$	MID Onset $_{i,t}$			Turnover $_{i,t}$	log Imports from US $_{i,t}$	
	(13)	(14)	(15)		(16)	(17)	(18)
Career Enter $_{i,t-3}$	0.309 (0.020) p = 0.000	0.007 (0.003) p = 0.035		Career Enter $_{i,t-3}$	0.326 (0.020) p = 0.000	-0.029 (0.013) p = 0.032	
Political Enter $_{i,t-3}$	0.089 (0.024) p = 0.000	0.002 (0.005) p = 0.626		Political Enter $_{i,t-3}$	0.088 (0.024) p = 0.000	-0.020 (0.025) p = 0.428	
Enter $_{i,t-3}$		0.006 (0.003) p = 0.042		Enter $_{i,t-3}$		-0.026 (0.011) p = 0.021	
log Imports from US $_{i,t-4}$	0.012 (0.010) p = 0.240	-0.002 (0.004) p = 0.601	-0.002 (0.004) p = 0.605	log GDP $_{i,t-4}$	0.038 (0.018) p = 0.038	0.102 (0.042) p = 0.015	0.101 (0.042) p = 0.015
log Exports to US $_{i,t-4}$	-0.001 (0.007) p = 0.845	0.002 (0.003) p = 0.400	0.002 (0.003) p = 0.397	log Pop $_{i,t-4}$	-0.017 (0.020) p = 0.406	-0.002 (0.043) p = 0.959	-0.002 (0.043) p = 0.960
UNGA Ideal Diff $_{i,t-4}$	0.012 (0.011) p = 0.256	0.005 (0.004) p = 0.274	0.004 (0.004) p = 0.284	log Exports to US $_{i,t-4}$	-0.003 (0.006) p = 0.630	0.032 (0.017) p = 0.063	0.032 (0.017) p = 0.063
Polity $_{i,t-4}$	0.0004 (0.001) p = 0.766	-0.001 (0.001) p = 0.049	-0.001 (0.001) p = 0.049	Polity $_{i,t-4}$	0.0003 (0.001) p = 0.829	0.003 (0.003) p = 0.351	0.003 (0.003) p = 0.350
Δ Polity $_{i,t-4}$	-0.001 (0.003) p = 0.788	0.001 (0.001) p = 0.174	0.001 (0.001) p = 0.175	FTA $_{i,t-4}$	-0.006 (0.037) p = 0.877	0.263 (0.077) p = 0.001	0.263 (0.077) p = 0.001
Capabilities $_{i,t-4}$	0.099 (0.669) p = 0.883	3.891 (0.633) p = 0.000	3.893 (0.634) p = 0.000	GATT/WTO $_{i,t-4}$	-0.039 (0.019) p = 0.036	0.039 (0.059) p = 0.503	0.039 (0.059) p = 0.502
Ally $_{i,t-4}$	0.027 (0.028) p = 0.323	0.016 (0.007) p = 0.022	0.016 (0.007) p = 0.022	Prior Vacancy $_{i,t-6:t-4}$	0.0002 (0.058) p = 0.998	-0.115 (0.085) p = 0.175	-0.114 (0.085) p = 0.177
Prior Vacancy $_{i,t-6:t-4}$	0.012 (0.057) p = 0.840	-0.001 (0.017) p = 0.975	-0.001 (0.017) p = 0.956	Prior Vacancy $^2_{i,t-6:t-4}$	-0.006 (0.065) p = 0.921	0.145 (0.097) p = 0.135	0.144 (0.097) p = 0.137
Prior Vacancy $^2_{i,t-6:t-4}$	-0.020 (0.067) p = 0.769	0.004 (0.017) p = 0.819	0.004 (0.017) p = 0.799	Prior Vacancy $^3_{i,t-6:t-4}$	0.002 (0.018) p = 0.915	-0.040 (0.025) p = 0.115	-0.039 (0.025) p = 0.117
Prior Vacancy $^3_{i,t-6:t-4}$	0.007 (0.019) p = 0.706	-0.002 (0.004) p = 0.701	-0.002 (0.004) p = 0.683	log Imports from US $_{i,t-4}$	-0.008 (0.016) p = 0.628	0.349 (0.033) p = 0.000	0.349 (0.033) p = 0.000
MID Onset $_{i,t-4}$	-0.056 (0.064) p = 0.377	0.080 (0.043) p = 0.064	0.080 (0.043) p = 0.065	log Imports from US $_{i,t-5}$	0.011 (0.020) p = 0.586	0.078 (0.019) p = 0.000	0.078 (0.019) p = 0.000
MID Onset $_{i,t-5}$	0.033 (0.069) p = 0.629	0.041 (0.039) p = 0.282	0.042 (0.038) p = 0.280	log Imports from US $_{i,t-6}$	0.001 (0.014) p = 0.933	0.030 (0.021) p = 0.150	0.030 (0.021) p = 0.150
MID Onset $_{i,t-6}$	0.009 (0.066) p = 0.893	-0.004 (0.037) p = 0.922	-0.004 (0.037) p = 0.922				
Observations	6,279	6,279	6,279	Observations	6,768	6,768	6,768

Note: All models include country FE, year FE, and year FE \times *Political Appointee* $_{i,t-3}$. Standard errors clustered by country, with p-values from a two-sided t-test.

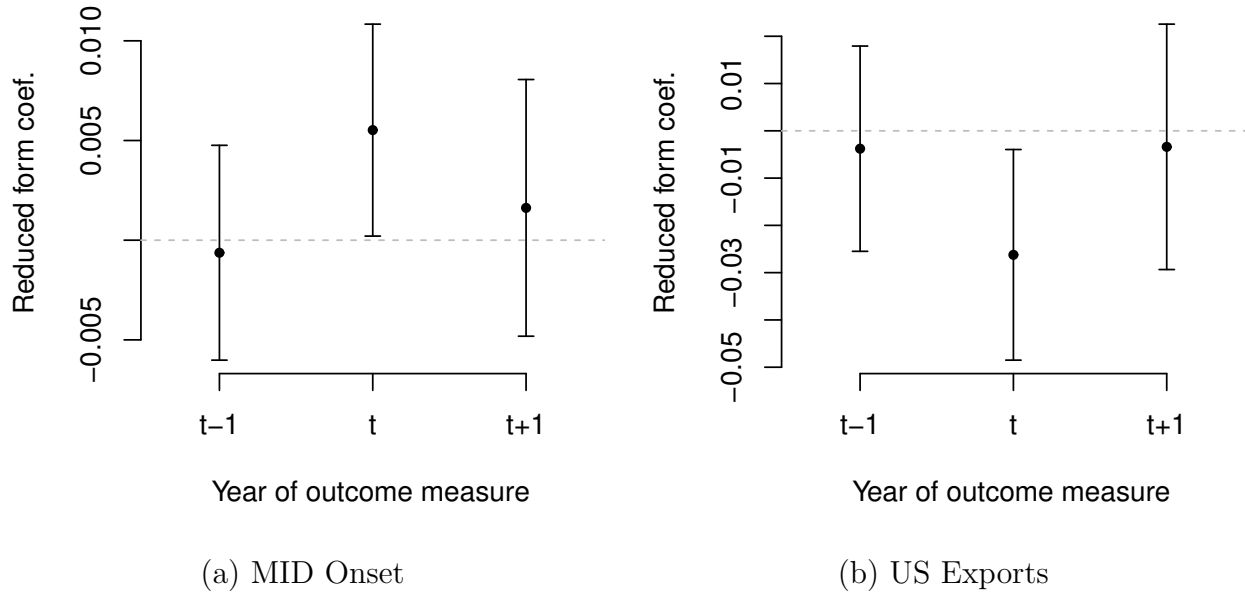
We should be cautious, however, in interpreting these results: while the general research design aims to isolate exogenous variation in the presence or absence of *any* presidentially-appointed ambassador, it is unable to address the non-random assignment of different types of ambassadors to different countries. In other words, these results cannot tell us whether political ambassadors are less effective than career ambassadors in preventing disputes and promoting exports, or whether political ambassadors are simply appointed to countries where these diplomatic outcomes are less sensitive to variation in embassy-level personnel.

Robustness and placebo tests

The appendix reports a series of robustness checks for alternative empirical specifications, including: omitting controls; omitting lagged dependent variables; adding other controls; accounting for a foreign country’s diplomatic representation in the US; and accounting for side accreditation of US ambassadors to multiple countries. The analyses of MIDs are repeated using a range of limited DV specifications including a reduced-form logit, an IV probit, and a set of event-count and zero-inflated event-count models (with the outcome recoded accordingly); and separately, on a reduced sample of only the countries that at some point engage in a MID against the US. All results are consistent with those reported in the main text.

As a final consideration, we may be concerned that the IV design is susceptible to bias arising from endogenous assignment of the entrance instrument. Even if pre-treatment covariates show no relationship with ambassadorial appointments (as reported in Table A12), actors involved in the appointment process may still be manipulating vacancies with an eye towards future outcomes. The placebo tests reported in Figure 2 should largely alleviate these concerns. The figure presents coefficient plots for variations of models 5 and 11, regressing outcomes directly on $Enter_{i,t-3}$ (including all covariates) with the outcome measures varied from $t - 1$ to $t + 1$. For both outcomes, we see that the predicted effects of a $t - 3$ entrance occur only in year t —precisely when the ambassador is scheduled to leave office—and not the year before or after. Attributing the main effects reported in Tables

Figure 2: Reduced-Form Placebo Tests



Note: Reduced-form coefficients and 95% confidence intervals, replicating (a) Column 14 and (b) Column 17 of Table 3, with all regressors included but varying the timing of the outcome measure.

1 and 2 to strategic manipulation of the treatment assignment would thus require a very peculiar type of manipulation: that is, actors in the appointment process would have to be manipulating in anticipation of outcomes precisely three years, but not two or four years, into the future. This does not comport with any reasonable intuition of political actors' behavior; the far more plausible interpretation is that the estimated relationships between turnovers and outcomes are causal.

Case Studies

The quantitative results provide systematic evidence of a relationship between ambassadorial turnovers and diplomatic outcomes. With regards to militarized disputes in particular, the mechanisms underlying this relationship warrant further elaboration. This section examines two cases that illustrate the processes through which ambassadorial turnovers can cause

underlying policy disagreements to give rise to militarized disputes. A third case of a dispute that arose between the US and Peru in 1969, at the conclusion of Ambassador Jones's tenure, is discussed in the appendix (omitted here due to space constraints). Each case study aims to provide a "within-case" analysis, comparing diplomatic developments during a turnover against otherwise similar circumstances shortly before or after. The main principles guiding the case selection process were representativeness with respect to the broader set of militarized disputes contributing to the quantitative results, as well as practical concerns of data richness. A more detailed discussion of case selection is provided in the appendix.

MID#4183: Canada, 1997

Between 1979 and 1997, the United States and Canada engaged in four militarized interstate disputes. Each case involved a use of force by one or the other country's coast guard in order to assert control over contested fishing waters. Within the context of US-Canadian relations, fishing disputes are no small deal: according to one diplomatic historian, "When you talk about Canadian-American relations you're essentially talking about fish." One such disagreement in 1989 led the Canadian coast guard, which was previously unarmed and civilian-manned, to arm their patrol vessels with 50-caliber machine guns and exchange fire with American fishermen—a "shooting war in the Gulf of Maine".⁹⁵ All four disputes constituted escalatory actions as part of a bargaining process over an international policy disagreement. Three of the four coincided with a turnover in the US ambassadorial post.

Throughout the postwar period, the two countries maintained unsettled maritime boundaries, making the allocation of fishing rights an issue that required continuous management and renegotiation.⁹⁶ The US Chief of Mission in Ottawa played an integral role in overseeing this portfolio: as Ambassador Blanchard recalls, "[s]ince the United States doesn't even have a fisheries minister, and responsibility for the industry is spread across several agencies, I

⁹⁵ ADST: James D. Walsh (p.61)

⁹⁶ ADST: Thomas G. Weston (p.112)

often had to act like one in my dealings with [Canadian Minister of Fisheries and Oceans] Brian Tobin.”⁹⁷ In one of several related episodes over Blanchard’s tenure, negotiations over the joint management of Pacific salmon stocks stalled in May 1994. American fisherman in the meantime continued depleting the fisheries. In response, the Canadians threatened to obstruct passage through what were previously understood to be international waters connecting Washington and Alaska. For them the status quo was costly and a speedy resolution was of the essence: as Tobin complained to Blanchard, “Why should we negotiate with you while you’re increasing your fish catch as we speak?”⁹⁸ Blanchard moved quickly to convince the White House to impose a cap on the US salmon haul, and to dissuade coastal state senators from taking legislative action that would further inflame tensions. The Canadians were appeased and talks continued apace.

Blanchard left post in Spring 1996, having managed the various fisheries issues on an on-going basis but leaving no permanent resolution in place. The ambassadorial post remained vacant through Clinton’s re-election and into the following year. In May the Canadian government, frustrated by the lack of progress in negotiations, directed its coast guard to conduct a series of of armed seizures of American fishing vessels; and so began the Salmon Wars of 1997.⁹⁹ Private Canadian fishermen joined the protest, blockading an Alaskan ferry route, and inadvertently devastating the tourism revenues of nearby Canadian port towns. The Premier of British Columbia added fuel to the fire, suing the US government and threatening to evict the US Navy from a leased base off the coast of Vancouver. After much bad blood and economic loss, the Americans acquiesced and returned to the negotiating table.

Unlike Blanchard’s account, an extensive oral history by then-Chargé Thomas Weston says nothing of his own personal efforts in managing the bilateral fisheries concerns. It is perhaps telling to examine Weston’s description of how US-Canadian disputes generally unfolded during his time in Ottawa: “What tends to happen is something will bring an issue

⁹⁷ Blanchard (1998, p.132)

⁹⁸ Ibid. p.135

⁹⁹ Egan (1997)

to a head,” he recounts, “and it will be a crisis...and then [the] ambassador/chargé/chief of mission is deeply involved in it”¹⁰⁰ Blanchard’s account, in contrast, is emphatic on the ambassador’s responsibility to proactively resolve issues before they reach the point of crisis—rather than getting involved after the fact.¹⁰¹ Canada’s decision to use force in May 1997, according to Gibler, was intended “to put out a message that they were serious about enforcing fishing regulations in [their] coastal waters.”¹⁰² With Blanchard at post, the Canadians could be confident that their concerns were in capable diplomatic hands. In his absence, the Canadians felt the need to assert their demands by other means.

MID#2906: UAR, 1964

A dispute of quite a different nature unfolded in 1964 between the US and Egypt (known then as the United Arab Republic, or UAR), shortly after Ambassador Luke Battle’s late-September arrival in Cairo. By the time Battle’s predecessor had departed in June, numerous points of tension had emerged in the US-UAR relationship. President Nasser was openly abetting anti-Western insurgencies in Yemen and the Congo. Hostilities with Israel simmered, and Nasser continually resisted US pressures to entertain disarmament talks.¹⁰³ All the while his economy relied heavily on US assistance, including the PL-480 food aid program which provided over half of Egypt’s wheat supply—a tension not lost on Congressional critics of both parties, who repeatedly sought to impose strict conditionality on bilateral aid to force Nasser in line.¹⁰⁴ By August, two months into the vacancy in Cairo, Nasser’s ambassador in DC expressed to a group of US officials his concern that “the US hadn’t done any real business with the UAR” in recent months. The ambassador himself “understood this matter but the ‘impatient young men’ who ran the government in Cairo were beginning

¹⁰⁰ ADST: Thomas G. Weston (p.113)

¹⁰¹ Keeley (2000, p.30)

¹⁰² Gibler (2018, p.5)

¹⁰³ FRUS: 1964-68v18/d96

¹⁰⁴ Burns (1985, p.150-157)

to wonder.” The PL-480 agreement—originally conceived and negotiated over the course of several months by a previous US ambassador¹⁰⁵—was set to expire, with talks for a renewal not yet begun. The prospect of a large stabilization loan had also been floated; “Cairo was taking the loan as proof of whether the US would continue its present policy” of cooperation and support writ large.¹⁰⁶ But that initiative now languished, with no US ambassador in place to keep it moving forward.

Pressures mounted and Nasser felt himself politically squeezed from all sides. Increasing economic instability made him vulnerable to internal opposition. The arms race and other foreign entanglements were costly, both in direct financial terms and in the looming threat of a punitive US response; but accommodation bore political costs as well, leaving him “fearful of any action that might expose him to [the] charge [that] he was being soft”.¹⁰⁷ Nasser had worked to cultivate relations with the Soviets and thus establish a credible outside option for an aid patron, but then Khrushchev was ousted in October and Nasser had to “start all over again”.¹⁰⁸ A group of students protesting US policy toward the Congo burned down the US Information Service library in Cairo in November; the government did not instigate the attack, but “in order to disguise the fact that the police had lost control, Nasser was prepared to accept responsibility for the attack and even be truculent about it”.¹⁰⁹ Finally in December, Nasser lashed out: the Egyptian air force shot down—albeit with plausible justification¹¹⁰—an American oil company plane near Alexandria, killing the pilot and co-pilot, and four days later Nasser delivered an inflammatory speech denouncing US efforts to strong-arm Egyptian policy.

What made Nasser choose to escalate, both verbally and militarily, and how did the turnover in the US ambassadorial post influence that decision? As Battle’s predecessor

¹⁰⁵ ADST: Raymond Hare (p.31)

¹⁰⁶ FRUS: 1964-68v18/d87

¹⁰⁷ FRUS: 1964-68v18/d96

¹⁰⁸ Burns (1985, p.159)

¹⁰⁹ Ibid., p.158

¹¹⁰ FRUS: 1964-68v18/d120

relayed in his exit interview in June, the prior maintenance of harmonious relations between the two countries rested on “the mutual capacity to hurt each other’s interests.”¹¹¹ By the Fall, Nasser found himself in an acutely vulnerable position in the multidimensional bargain that characterized the bilateral relationship, facing demands from various interests across the US government for concessions which he found politically untenable. Under normal conditions, a US ambassador—the “coordinator of all our varied relationships” with her host country, in Blanchard’s words¹¹²— would have sought to restrain those other actors’ impulses to “fight for every small advantage [they] can get”¹¹³ vis-à-vis the UAR, and worked to triage and temper the demands being made. But the turnover in the ambassadorial post prevented such a corrective from being applied in a timely manner.

Having never served in the region, Battle faced an unusually steep learning curve. As he later flatly acknowledged, “my appointment was the kind that all Foreign Service officers should fight. I was not prepared to go to Cairo as Ambassador.”¹¹⁴ The pressing issues facing Embassy Cairo differed considerably from those Battle encountered in his previous appointment at the Bureau of Educational and Cultural Affairs, and it seems he took some time, as many appointees do, to “get up to speed” in the new position.¹¹⁵ Despite the slow start, Battle did demonstrate in his internal communications an inclination to sympathize with Nasser’s political difficulties from the outset,¹¹⁶ and after several weeks on the job began lobbying his superiors for a PL-480 reauthorization: he warned, for instance, in a telegram on November 11, that the US “must give early sign of continuation [of] cooperative effort or a new policy will exist here whether we intend it or not.”¹¹⁷ But Nasser may not have been fully aware of Battle’s efforts or intentions—the two had not yet held a substantive meeting

¹¹¹ FRUS: FRUS: 1964-68v18/d74

¹¹² Keeley (2000, p.30)

¹¹³ Blanchard (1998, p.121)

¹¹⁴ ADST: Lucius Battle (p.23)

¹¹⁵ O’Connell (2009)

¹¹⁶ FRUS: 1964-68v18/d102

¹¹⁷ FRUS: 1964-68v18/d105

before the downing of the plane¹¹⁸—and in any case Nasser needed results faster than Battle proved capable of delivering.

The Egyptian leader thus determined that his best course of action was to force the US's hand by escalating to the point of crisis. Nasser's message was clear: "The Americans want to give us aid and dominate our policy," he declared in his speech. "I am not prepared to sell Egyptian independence... We can tighten our belts... But we are not going to accept pressure. We are not going to accept gangsterism by cowboys." Confirming these public statements, Nasser subsequently shared with a CIA informant his confidence that "the USA is afraid to cut off aid to Egypt because the US knows that Egypt will react by sabotaging all American efforts in the area."¹¹⁹ Indeed, the Secretary of State soon afterwards wrote to Johnson arguing for the importance of "getting back onto even keel with the UAR," and outlining the multitude of US interests that would be put at risk from further deterioration of relations. "Only the Soviets," he advised, "will benefit from a such a situation."¹²⁰ After some initial pushback from Congress, Johnson agreed to resume aid through a series of short-term agreements—a structure proposed by Battle so as to ensure "almost continual negotiation with the UAR... so that the value of our cooperation is not forgotten."¹²¹

Relations were steadied, if not fully restored, for the next two years of Battle's tenure. Nasser took steps to resolve some of the major bilateral incompatibilities, ceasing arms shipments to the Congo¹²² and entering negotiations to end the conflict in Yemen. Battle, for his part, continued advocating internally for the delivery of bilateral assistance.¹²³ On the ground in Cairo, he developed a personal rapport not only with Nasser but also with the country's preeminent newspaper editor (himself a powerful political figure), causing the latter to tone down his critical coverage of US policy. (The editor later described his moderation as

¹¹⁸ ADST: Lucius Battle (p.25)

¹¹⁹ Burns (1985, p.160)

¹²⁰ FRUS: 1964-68v18/d125

¹²¹ Burns (1985, p.164)

¹²² FRUS: 1964-68v18/d208

¹²³ Burns (1985, p.166); FRUS: 1964-68v18/d233; FRUS: 1964-68v18/d376

a “derelict[ion]”, but one borne out of admiration for the ambassador’s hard-fought efforts to improve bilateral relations.¹²⁴) This likely helped to lighten the domestic pressures pushing Nasser toward an antagonistic posture; as a more direct approach, Battle developed a routine of preemptively seeking out the Egyptian leader to explain away the various provocations that arose from voices in Congress and the American press and to dissuade him from responding in kind. “[W]e got into a pattern of preventative diplomacy and it worked, at least for a while.”¹²⁵ Yet the promising bilateral trajectory would not outlast Battle’s appointment. Nasser had remained “very patient with all the pressure” he felt from the US while Battle was at post to keep things in order; with Battle’s departure, Nasser expressed, “our patience has run out.”¹²⁶ At their farewell meeting in March 1967, Nasser withdrew his final request for US assistance, and with it any remaining possibility of a full bilateral reconciliation.¹²⁷

Conclusion

This study has sought to address the question of diplomacy’s efficacy by examining the varying influence of diplomatic agents in the US foreign policy process. The findings demonstrate that two of the most widely studied phenomena in international relations—economic exchange and militarized conflict—are affected by a factor largely neglected by previous formal and quantitative research: the status of the agent charged with overseeing bilateral diplomatic relations. As a result of the removal of a US ambassador, the country in which they operate receives a lower volume of US exports and becomes more likely to engage in a militarized dispute with the US. The empirical results provide systematic support for a basic proposition long held by practitioners and proponents of diplomacy: diplomacy matters, and it matters who the individuals are who conduct it.

¹²⁴ ADST: Lucius Battle (p.30)

¹²⁵ Ibid. p.32

¹²⁶ Burns (1985, p.170)

¹²⁷ ADST: Lucius Battle (p.35)

A natural question that arises from these findings is whether and to what extent they generalize beyond the present context of analysis. The inferential strategy employed here relied on a particular feature of US diplomatic practice—the routinized three-year rotation system with substantial spans of vacancy between appointments. Extending the analysis to other countries would require, at the very least, different research designs leveraging other context-specific sources of variation. Theoretically, our expectations of what such an analysis would discover may be mixed. On the one hand, an implicit scope condition of the argument presented here is that the home government in question have a sufficiently broad set of international interests and engagements that its institutional response is to compartmentalize and delegate responsibilities to different agents with varying preferences and geographical purviews. The extent to which this characterization fits any given country’s foreign policy apparatus is an empirical question to be considered case by case. Yet there is certainly value in the general perspective of viewing actions and decisions of any given international entity as the product of an internal “pulling and hauling”¹²⁸ among different actors representing organizations with competing conceptions of their nation’s foreign policy interests.¹²⁹ Taking this perspective, we can ask when and why certain participants wield greater or lesser influence in the intragovernmental policy process, and how that variation can help explain the international outcomes we observe.

¹²⁸ Allison and Zelikow (1999)

¹²⁹ Halperin and Clapp (2007, ch.18)

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Supplemental Appendix for: Conflict, Cooperation, and Delegated Diplomacy

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A Summary Statistics and Data Sources

Table A1 lists all variables used in the analysis, along with summary statistics and data sources. Two other characterizations of the sample are also included.

A.1 Nominal vs. Effective Sample

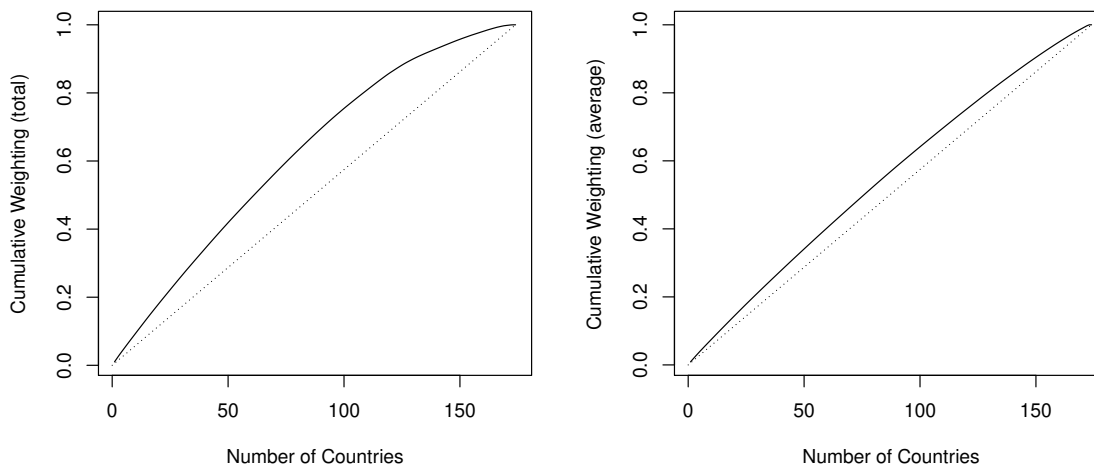
First, following [Aronow and Samii \(2016\)](#), I report summary statistics of pre-treatment covariates according to their weighting in the effective sample. The authors show that when using multiple regression to estimate an average treatment effect (ATE), the regression procedure mechanically weights each observation j by the conditional variance of that observation's treatment value: that is, the estimated treatment effect, $\hat{\beta}$, converges in probability to $E[w_j\tau_j]/E[w_j]$, where τ_j is unit j 's individual treatment effect, and $w_j = (D_j - E[D_j|X_j])^2$, for treatment D_j and covariates X_j . As such, the treatment effect estimated by multiple regression is an ATE for the *effective sample*, which is the nominal sample with each unit weighted inversely to how well its covariates predict its treatment. This means that standard multiple regression estimates can be quite unrepresentative of the ATE across the nominal sample, especially if, for instance, the independent variable of interest is generally sticky or slow moving but experiences dramatic jumps in a limited number of cases. I follow Aronow and Samii's procedure to recover these "regression weights", and record the resulting weighted mean for pre-treatment covariates in the lower panel of Table [A1](#). We see that the effective sample is very similar to the nominal sample across all covariates, meaning that the reported treatment effects should quite closely approximate the average treatment effect with equal weighting applied across the nominal sample.

Figure A1 reports a similar analysis, aggregating regression weights by country. In both figures, countries are aligned on the horizontal axis according to their regression weighting; each country's aggregate weighting is used in the left panel, and its average weighting (aggregate weighting divided by number of years in the sample) is used in the righthand panel. The solid line represents the cumulative weighting, compared against a perfectly flat distribution of weights represented by the dotted line. We see that the distribution of average regression weights across countries is very close to a flat distribution; insofar as any countries are contributing more weight than others to the average treatment effect, this is because they appear in the sample for more years (i.e. countries that became independent partway into the timeframe under analysis, or that ceased to exist due to dissolution or unification).

Table A1: Summary Statistics

Variable	Mean	Min	Max	SD	Source and Notes		
Turnover _{<i>i,t</i>}	0.427	0	1	0.495	Office of the Historian (2018a); indicator for any ambassadorial vacancy or turnover		
Pct. Vacant _{<i>i,t</i>}	0.149	0	1	0.253	Office of the Historian (2018a); portion of the year that the ambassadorial post was vacant		
Enter _{<i>i,t-3</i>}	0.313	0	1	0.464	Office of the Historian (2018a); indicator for any ambassador entrance into office in $t-3$		
MID Onset _{<i>i,t</i>}	0.014	0	1	0.116	Palmer et al. (2019); indicator for whether country i in year t entered into a Militarized Interstate Dispute in which the US was a disputant on the opposing side		
US Exports _{<i>i,t</i>} (log)	6.188	0	12.477	2.279	The Correlates of War Trade Data Set (Barbieri et al. 2008), flow from US to country i , adjusted to constant 2015 USD. The primary source for this dataset is the IMF's <i>Direction of Trade Statistics</i> . The COW data supplements the IMF data with secondary sources, resulting in 19% higher coverage.		
Pre-treatment Covariates	Mean (Nominal Sample)	Min	Max	SD	Mean (Effective Sample)	Mean (Compliers)	Source and Notes
Prior Vac _{<i>i,t-6;t-4</i>}	0.416	0	3	0.472	0.402	0.424	Sum of Pct. Vacant variable from years $t-6$, $t-5$, and $t-4$
GDP _{<i>i,t-4</i>} (log)	23.967	18.74	30.854	2.071	23.839	23.607	World Bank (2018); adjusted to const. 2015 USD; missing values imputed using the Amelia II R package (Honaker et al. 2011)
Population _{<i>i,t-4</i>} (log)	15.763	9.88	22.68	1.65	15.717	15.786	World Bank (2018); missing values imputed
Exports to US _{<i>i,t-4</i>} (log)	5.767	0	14.241	2.703	5.647	5.26	Barbieri et al. (2008); flow from country i to US; missing values imputed
Polity _{<i>i,t-4</i>}	0.712	-10	10.815	7.336	0.535	0.577	Marshall and Jaggers (2002); Polity 2 score; missing values imputed
Δ Polity _{<i>i,t-4</i>}	0.086	-15	16	1.861	0.138	0.222	Change in polity from $t-5$ to $t-4$
Capabilities _{<i>i,t-4</i>}	0.006	0	0.179	0.017	0.005	0.001	Singer et al. (1972)'s capability index; composite measure of six factors
US Ally _{<i>i,t-4</i>}	0.341	0	1	0.474	0.328	0.224	Gibler (2008); whether country i in year t was party to a mutual defense alliance with the US
FTA _{<i>i,t-4</i>}	0.018	0	1	0.132	0.018	0.007	Dir et al. (2014); indicator for whether US and country i are party to a free trade agreement
GATT or WTO _{<i>i,t-4</i>}	0.657	0	1	0.475	0.654	0.692	WTO (2019b) and WTO (2019a); indicator for whether country i is a member of the GATT or WTO
Econ. Aid _{<i>i,t-4</i>}	12.961	0	22.96	7.482	13.355	15.933	USAID (2018); negative values smoothed across adjacent years
Mil. Aid _{<i>i,t-4</i>}	9.031	0	23.12	7.666	9.231	10.942	USAID (2018)
Political Appointee _{<i>i,t-4</i>}	0.296	0	1	0.456	0.259	0.193	Office of the Historian (2018a); indicator for whether the ambassador in office at the start of year $t-3$ was a non-career appointee (or the most recent prior ambassador in case of a vacancy)
SOTU Mention _{<i>i,t-4</i>}	0.067	0	1	0.249	0.063	0.039	Benoit et al. (2018)'s sotu corpus; indicator for whether country i was mentioned in the US President's State of the Union Address
Pres. Visit _{<i>i,t-4</i>}	0.068	0	1	0.252	0.06	0.033	Office of the Historian (2018b); indicator for whether country i received a diplomatic visit from the US President

Figure A1: Country Weights in Effective Sample



Note: In both panels, countries are sorted along horizontal axis by decreasing regression weights. Solid line represents observed cumulative weighting; dotted line represents a flat weighting. Left panel shows countries' total regression weights; right panel shows average regression weights (total weights divided by number of years in the sample).

A.2 IV Compliers

It is well understood that the 2SLS estimator can only recover the local average treatment effect (LATE) for the population of compliers with the treatment assignment (Angrist et al., 1996); in the present context, this is the set of observations that would experience a turnover if an ambassador had entered in the year $t - 3$, but would not otherwise. Because the IV estimates are local to this subpopulation, we would like to know the characteristics of this subpopulation, and how closely it resembles the full population. The lower panel of Table A1 reports the covariate profile of these compliers, focusing on the binary $\text{Turnover}_{i,t}$ treatment uptake, and using the κ -weights from Abadie (2003).

We see that the compliers are similar to the full population on most covariates, but differ in a few respects, which may be informative for understanding the differences between the OLS and IV estimates:

- Compliers have lower military capabilities on average. This suggests that these are countries with which the non-diplomatic actors within the US government would be relatively less averse to risking a militarized dispute.
- Compliers are less likely to be party to a free trade agreement with the US; with fewer formal mechanisms in place to regulate trading relations, the ambassador plays a larger role in promoting US exports by enforcing cooperation through extra-institutional means.
- Compliers are less likely to have recently received a diplomatic visit from the president, or

to have been mentioned in a State of the Union address, suggesting that these are countries that are largely off the president's political agenda and are thus more sensitive to lower-level bureaucratic competition over the formation of US policy. (Note that diplomatic visits and SOTU mentions are not used in any of the main analyses, but are simply used here for the purpose of characterizing the compliers.)

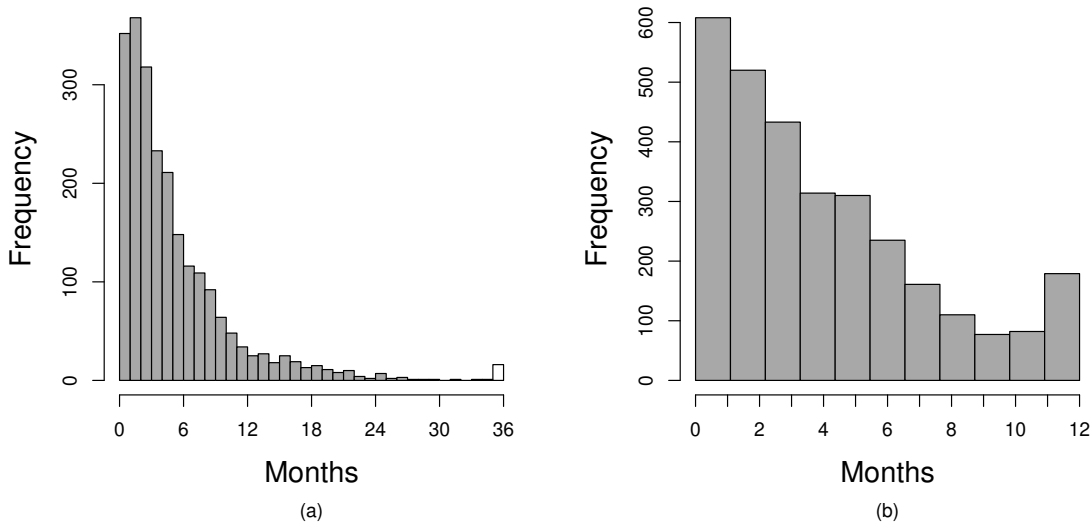
- Compliers are slightly more likely to receive career ambassadors, rather than politically appointed ambassadors. Results in Tables [A5](#) and [A6](#) suggest that career ambassadors are the ones driving the aggregate effect of turnovers on conflict (though see caveats below on interpretation of those analyses).

Each of these differences points towards the compliers having larger average treatment effects than the population as a whole; and indeed, the IV estimates turn out to be consistently larger than the OLS estimates in Tables 1 and 2 of the main text.

A.3 Vacancy Measure

Figure A2 shows the construction of the ambassadorial vacancy variable. Panel (b) depicts vacancy spells, which may span multiple calendar years. Under conditions of normal diplomatic relations (as defined in section D.4 below), the vast majority of vacancies—91%—last less than one year; only 1.5% run beyond two years.¹ The median vacancy duration is 103 days. Panel (a) depicts the distribution of the $Pct.Vacant_{i,t}$ variable which is used in the analyses. To demonstrate how these variables are constructed: if an ambassador leaves office on September 1 of year t , and her replacement enters on May 1 of year $t + 1$, the vacancy spell was seven months (three in year t and four in year $t + 1$); this translates into $Pct.Vacant_{i,t} \approx 0.25$ and $Pct.Vacant_{i,t+1} \approx 0.33$, with the binary measures $Turnover_{i,t} = Turnover_{i,t+1} = 1$.

Figure A2: Distribution of Vacancy Durations



Note: (a) Observations are vacancy spells, which may span multiple calendar years. All observations above 36 months are collected in the last bin. (b) Observations are within-country-year vacancies, excluding the observations of zero vacancy. In both figures, the sample is restricted to observations for which the US and host country maintain normal diplomatic relations.

¹Unlike many other federal appointments, there is no statutory limitation regarding the length of time an ambassadorial post may remain vacant. See <https://www.gao.gov/assets/80/75055.pdf>, footnote 3.

B Supplemental Analyses

This section provides additional analysis and discussion which was omitted from the main text due to space constraints.

B.1 Qualitative analysis

B.1.1 Case selection

Here I discuss, in greater depth than the main text allowed for, the process of case selection for the qualitative analyses.

My basic organizing principle was to analyze cases that provided within-case variation in both the dependent variable (MID onset) and main independent variable (turnover): that is, for each case, focus on a turnover period that experienced a MID, along with a period of time shortly before or afterward which faced otherwise extremely similar bilateral circumstances. This allows us examine what the ambassador in each case did to manage the risk of conflict while in office, and how that diplomatic work was disrupted by the turnover, resulting in the outbreak of a MID. This principle comports with the justification for a “most similar cases” design, as discussed by [Seawright and Gerring \(2008\)](#) as well as [Nielsen \(2016\)](#), insofar as we consider the turnover period, and the period before or after, as two separate, “paired” cases. The reasoning behind this case selection strategy, as explained by Seawright and Gerring, is that because “the two cases are similar across all background conditions [X_2] that might be relevant to the outcome of interest. . . It may be presumed. . . that the presence or absence of [“treatment” variable] X_1 is what causes variation on Y ” ([Seawright and Gerring, 2008](#), p.304). [Goemans and Spaniel \(2016\)](#) likewise suggest, as a method of examining a counterfactual claim qualitatively, that “the researcher can look for an exogenous shock”—the turnover of the ambassador, in the present context—“that altered the relevant parameters [of the theoretical model] at hand. . . This is the most desirable counterfactual, as it relies the least on the researcher’s ability to make historical inferences.”²

As for the decision of which cases to select for the paired/within-case analyses, I sought to select cases which were generally representative of the kinds of cases that contribute to the quantitative results. Representativeness, according to Seawright and Gerring, is a primary objective of both the “typical case” and “diverse case” selection strategies: for the former, “the researcher wants to find a typical case of some phenomenon so that he or she can better explore the causal mechanisms at work in a general, cross-case relationship” (p.299); a justification of the latter is that “[e]ncompassing a full range of variation is likely to enhance the

²Their study is focused specifically on qualitative testing of formal theoretical models, but I believe their insights are applicable to the present context.

Table A2: Case study contextual variables

	MID#4183: Canada, 1997	MID#2906: UAR, 1964	MID#350: Peru, 1969
Timing of MID relative to turnover	During extended vacancy	Shortly after ambassador's entrance	"Lame duck" period before ambassador's exit
Disputant regime type	Full democracy	Single-party dictatorship	Post-coup military junta promising return to democracy
Nature of underlying relationship	Stable	Volatile, high priority to US	Volatile, low priority to US
Issues in contention	Territorial fishing waters	UAR involvement in conflicts in Yemen and Congo, arms race with Israel, and food aid	Expropriation of assets of multinational corporation, and territorial fishing waters
US President's year in office	5	2	1

representativeness of the sample of cases chosen by the researcher" (p.300).

The search for a "typical" case motivated the inclusion of a fishing dispute, given that fishing disputes constitute a substantial portion of MIDs (and especially post-WWII MIDs between democracies, as documented by [Mitchell and Prins \(1999\)](#)). The goal of representativeness also led me to opt against choosing cases like Cold War-era US-Soviet MIDs, for instance, despite the fact that they were quite frequent (and sometimes coincided with turnovers in the ambassadorial post). Theoretically, it seems unlikely that even a credentialed US ambassador would wield substantial influence over US-Soviet relations during the Cold War. Empirically, as it turns out, no post-WWII US ambassadors to the Soviet Union served a standard three-year term, meaning that US-Soviet MIDs are not the ones driving the IV results.

Given that the quantitative results aggregated over the global sample of countries, with wide variation in the regime type of the disputant, the nature of the underlying bilateral relationship with the US, and the nature of the underlying issues in contention, I also wanted the qualitative cases to reflect that diversity (to the extent possible). Likewise, the quantitative results included disputes that occurred during a vacancy, shortly after an ambassador's entrance, or shortly before an ambassador's exit, and I wanted the case studies to reflect those temporal dynamics as well. Having included a dispute with Canada, a close ally and developed democracy, I sought to complement that case with others featuring different regime types, and more volatile relations with deeper rifts and greater risks of escalation. Further, this set of cases includes MIDs at the aforementioned three different stages of the turnover process (before, during, and

after the vacancy). Variation across the cases along a set of contextual variables is depicted in Table A2.

The final criterion for case selection was the practical concern of data availability.³ I wanted to ensure that each of the case studies could be supported not only by detailed historical data, but also by multiple primary and secondary sources for each case. The three cases I selected all had the advantage of having book-length accounts written about bilateral relations surrounding the disputes, as well as extensive interviews with multiple chiefs of mission (from the ADST series or elsewhere), contemporary news stories, and (in the Peru and UAR cases) extensive coverage in the online FRUS series.

B.1.2 MID#350: Peru, 1969

Here I continue the analysis of the militarized dispute between Peru and the US at the end of Ambassador Johnny Jones's tenure in 1969.

With the inauguration of Richard Nixon, Ambassador Jones was soon informed by the White House that his "days in Peru were not exactly numbered, but that they wouldn't last very much longer."⁴ With this development, Jones effectively ceased to be a long-term player with whom the Peruvian government saw value in negotiating.⁵ In the ambassador's first meeting with President Velasco after the coup in October 1968, Velasco had been eager to justify the military's actions, to convey their plans for economic stabilization and a return to civilian government, and to express the need for "help and understanding" from the United States;⁶ when the two met again after Nixon's inauguration, Velasco seemed "ill at ease and harassed", and unwilling to engage Jones in substantive discussion over outstanding bilateral issues.⁷

Immediately after seizing power, the military had nullified an agreement reached between Belaúnde and the IPC and proceeded to expropriate the company's refinery. A deadline of April 9 had been established under Johnson for automatic cuts to bilateral assistance and sugar import quotas to go into effect, and Nixon showed no inclination toward revising that deadline.⁸ The Peruvians determined that an acceptable and timely resolution would require the involvement of a US negotiator who was an "important man in US government circles," "clearly stipulated" to be the personal representative of the president, with "broad discretionary

³Van Evera (1997) puts "data richness" at the top of his list of case selection criteria (p.77); Goemans and Spaniel (2016) recommend that "the ideal case study has a detailed enough historical record that the researcher can evaluate the counterfactual" (p.30).

⁴ADST: John Wesley Jones (p.32)

⁵Contemporaneous diplomatic cables confirm that the Peruvians were anticipating the turnover; see Walter (2010, p.170)

⁶FRUS: 1964-68v31/d521

⁷Walter (2010, p.171)

⁸Ibid. p.150 and 173

powers of negotiation”⁹—a role that the lame-duck Ambassador Jones was unable to fulfill.

On February 13, the Peruvian navy fired on and seized two US fishing vessels in contested waters. This escalation seems to have won the Peruvians the high-level attention they sought: the following day Kissinger suggested that Nixon consider sending a presidential emissary to Peru,¹⁰ and Nixon soon announced the appointment of John Irwin, an influential Republican attorney, as his personal representative to negotiate the expropriation and fisheries issues in tandem. Velasco received this announcement with “apparent enthusiasm.”¹¹ Through a continued strategy of “brinkmanship”, as one US official described it, the Peruvians ultimately brought Irwin, and consequently Nixon, to an understanding that Jones had reached months prior: that a protracted fight and punitive sanctions would strengthen, not weaken, the Peruvian leadership’s hold on power along with its nationalist impulses. (Jones reported his assessment to this effect on January 19,¹² but the advice went unheeded until it was reiterated in a nearly identical assessment by Irwin on April 4.¹³) The Nixon administration “blinked”, accepting a far worse deal than it originally demanded and deferring the application of sanctions.

One can only speculate as to how events would have unfolded had the turnover in US presidential administration not been accompanied by a turnover in the ambassadorial post in Lima. In particular, the counterfactual comparison one would have to consider is the scenario in which Nixon enters office, and makes clear his intention to both keep Jones at post and empower Jones to represent him as he would empower his own appointee. (The potentially confounding influence of the US presidential turnover in this case highlights the importance of accounting for time effects in the statistical analyses: this allows for within-year comparisons of countries that do and do not experience an ambassadorial turnover, thus holding fixed whether the US presidential administration is experiencing an election or turnover.) It seems eminently reasonable to posit that, had the incoming Nixon administration accepted Ambassador Jones’ assessment and followed his policy recommendations, the militarized dispute would have been avoided; whether there exists a plausible counterfactual world in which Nixon would have heeded the advice of a Johnson appointee is the more difficult question.

Whatever the counterfactual relationship between Nixon and Ambassador Jones, the historical evidence does indicate that the Peruvians were keenly attentive to the status of the US agents charged with overseeing bilateral issues, and the influence those agents wielded to shape policy internally. The final point that Velasco raised in his farewell meeting with Jones was his apprehension over the fact that the US had not yet appointed a new ambassador to Lima; Jones’s assurances as to the competence of his deputy, who would stay on as chargé in the

⁹North American Congress for Latin America (1969)

¹⁰FRUS: 1969-76ve10/d579

¹¹Walter (2010), p.175

¹²Ibid. p.166

¹³Ibid. p.182

interregnum, did not seem to dispel Velasco's concerns.¹⁴ In seeking a resolution of the fisheries and expropriation issues, the Peruvian leadership recurrently adjusted their negotiating tactics (including the tactic of militarized escalation) in response to or in anticipation of changes in US diplomatic personnel, even absent any accompanying change in the White House's stated positions on the issues themselves. The same issues that prompted militarized confrontation during the turnover, Jones had managed quietly throughout his tenure up to that point, and his successors did the same for the remainder of the Nixon administration.

As it happens, throughout the postwar period, the Peruvians—like the Canadians—engaged in four MIDs with the US over territorial waters; and like the US-Canadian disputes, three of the four coincided with an ambassadorial turnover.

As a final consideration, it is worth noting the interrelationship between trade and conflict outcomes that is highlighted in this case. In addition to the militarized confrontation over the fisheries issues, the year 1969 saw the single lowest annual volume of US exports to Peru over a thirty-year window. Insofar as “trade follows the flag”,¹⁵ we can posit that this case is illustrative of a general pattern: by working to maintain harmonious relations more broadly, a chief of mission's diplomatic efforts can have the indirect effect of preventing these sorts of downturns in trading relations which follow from seemingly unrelated bilateral disputes.

¹⁴Ibid., p.191

¹⁵Keshk et al. (2004)

B.2 Career vs. Political Appointees

Here I elaborate on two points regarding the distinction between career and political ambassadors:¹⁶ first, the differential electoral cycles in ambassadorial appointments by appointee type; and second, the heterogeneous effects of turnovers by appointee type.

B.2.1 Electoral cycles

As mentioned in the main text, all analyses include year fixed effects interacted with appointee status, along with the component terms of the interaction. Specifically, $PoliticalAppointee_{i,t}$ is an indicator for whether the appointee in office at the start of year t (or the most recent prior appointee, in the case of vacancy at the start of year t) was a political appointee; the OLS models interact year FE with $PoliticalAppointee_{i,t}$, and the 2SLS models interact year FE with $PoliticalAppointee_{i,t-3}$. Thus the models effectively include two fixed effects per year: a careerist-year FE, and a non-careerist-year FE. This is to address the possibility of a heterogeneously confounding influence of US electoral cycles on both appointment patterns and foreign policy behavior: intuitively, countries that receive political appointees are likely to have an ambassadorial appointment schedule that more closely aligns with the presidential election cycle, and those same countries may be differentially affected by electoral cycles in their broader bilateral relations with the US, as compared to countries that receive career appointees.

We can observe the differential electoral cycles by appointee type in Table [A3](#) below.

Table A3: Annual turnover rate, by appointee type and election cycle year

	Election cycle year			
	1	2	3	4
Non-careerist in office Jan. 1	75.63%	17.78%	21.93%	27.46%
Careerist in office Jan. 1	42.31%	30.64%	32.57%	35.37%

The first row represents country-years with a non-career ambassador in office at the start of the year, while the bottom row represents country-years with a career ambassador in office at the start of the year. We see that both appointee types experience an electoral cycle, in that both experience the highest rate of turnover in the first year of a presidential term. However, the cycle is far more pronounced for non-career appointments than for career appointments.

The pattern appears even more starkly if we just compare years in which the presidency

¹⁶Note that this terminology, though common, is misleading: all ambassadors are political appointees, in that they are principal officers of the State Department whose appointment requires Presidential nomination and Senate confirmation. I follow the convention of using the term “political ambassador” to refer to ambassadors who are not career Foreign Service Officers.

changes parties (1961, 1969, 1977, 1981, 1993, 2001, and 2009) to all other years, as seen in Table [A4](#).

Table A4: Annual turnover rate, by appointee type and inauguration year

	New party inaugural year	All other years
Non-careerist in office Jan. 1	92.04%	27.87%
Careerist in office Jan. 1	46.68%	33.47%

Simply including year fixed effects in the regressions would not account for this potential source of confounding, insofar as the countries that receive different types of appointees also experience differential electoral cycles in their broader relations with the US (due to factors other than ambassadorial appointments). This is the reason that all models also include the year FE interacted with a political appointee indicator. Note that the inclusion of year FE that vary by appointee type serves only to address concerns of omitted variable bias, but says nothing of heterogeneous treatment effects, which is what the analyses reported in Tables [A5](#) and [A6](#) below seek to estimate.

B.2.2 Heterogeneous effects

The main empirical analyses in the main text pool together career and non-career appointees in the operationalization of the main independent variables: $Enter_{i,t-3}$ indicates the entrance of either type of ambassador into office, $Turnover_{i,t}$ indicates the absence of either type, and the appointment of either type in year $t - 3$ is a strongly significant predictor of turnover in year t . The main text offered some theoretical discussion as to why we may or may not expect non-career ambassadors to underperform their careerist counterparts (and thus expect a turnover in a non-career appointment to have less detrimental effects for conflict and trade).

Tables [A5](#) and [A6](#) report a set of analyses aimed at differentiating between the effects of turnovers in career versus political ambassadors. A parallel set of analyses is reported in both tables, for the two separate outcomes. Columns 1 and 2 of Table [A5](#) replicate Columns 13 and 15 of Table 3, and Columns 1 and 2 of Table [A6](#) replicate Columns 16 and 18 of Table 3, with the entrance instrumented separated into $Career\ Enter_{i,t-3}$ and $Political\ Enter_{i,t-3}$. Columns 3 through 6 use the same separated instruments, in separate models. (In column 5 we see that the political entrance instrument alone does not provide a significant first-stage relationship, and as such, the 2SLS using this instrument alone is uninformative.) Columns 7 and 8 interact the treatment assignment and uptake (that is, $Enter_{i,t-3}$ and $Turnover_{i,t}$) with $PoliticalAppointee_{i,t-3}$ (an indicator for whether the ambassador in office at the start of $t - 3$ was a non-career appointee, or the most recent prior ambassador in the case of a vacancy).

Columns 9 and 10 subset to the observations for which $Political\ Appointee_{i,t-3}=0$, and columns 11 and 12 subset to the $Political\ Appointee_{i,t-3}=1$ observations.

Across these different specifications, a fairly consistent pattern emerges. The effect of turnovers on MID onsets appears to be driven primarily by turnovers in career ambassadors. The point estimate and precision of the coefficient on $Turnover_{i,t}$ are similar when the treatment is interacted with the political appointee indicator, when the sample is restricted to countries receiving career ambassadors, and when the instrument is recoded to only include entrances of career ambassadors. This is not the case for trade outcomes. The effect of turnovers on US exports is weaker and less precisely estimated when restricting attention to turnovers in career appointees, across all specifications; we cannot conclude that the aggregate effect estimated using the pooled entrance instrument is driven solely by the career ambassadors.

This heterogeneity by ambassador types and outcomes is intuitively reasonable, and comports with much of the common justification for (and criticism of) the appointment of non-career ambassadors: they may be perfectly competent to promote and support US firms doing business abroad, but are inferior to career diplomats in the more sensitive aspects of negotiating and managing crises. The empirical patterns observed would be consistent with this reasoning; however, as noted in the main text, these tests cannot tell us whether career ambassadors actually perform better or worse (or at all differently) than non-career ambassadors, as opposed to the alternative explanation that career and non-career ambassadors are assigned to countries with systematically different prospects for conflict and cooperation (and systematically different sensitivities to changes in diplomatic personnel). In other words, this research design provides no causal leverage to estimate the effect on conflict and cooperation of appointing a non-career ambassador to a given country, as compared to the counterfactual of appointing a career ambassador to that same country. Finally, a more technical explanation for these heterogeneous effects has to do with the strength of the first-stage relationship: because non-career ambassadorial appointments adhere less tightly to the three-year rotation norm, they exhibit a weaker first-stage relationship between a $t - 3$ entrance and a t turnover, which increases the variance of the second-stage estimation.

Table A5: Heterogeneous Effects of Turnover on MID Onset, by Appointee Type

	Full Sample						Split Sample					
	Separate Instruments			Interaction			Prior Political, $t-3 = 0$			Prior Political, $t-3 = 1$		
	(FS) (1)	(RF) (2)	(FS) (3)	(2SLS) (4)	(FS) (5)	(2SLS) (6)	(FS) (7)	(2SLS) (8)	(FS) (9)	(2SLS) (10)	(FS) (11)	(2SLS) (12)
Turnover $_{i,t}$				0.022 (0.011) p = 0.043		0.035 (0.192) p = 0.855		0.023 (0.011) p = 0.031		0.022 (0.011) p = 0.044		0.024 (0.049) p = 0.617
Enter $_{i,t-3}$						0.289 (0.022) p = 0.000			0.290 (0.022) p = 0.000			0.125 (0.030) p = 0.000
Career Enter $_{i,t-3}$	0.309 (0.020) p = 0.000	0.007 (0.003) p = 0.035	0.300 (0.020) p = 0.000									
Political Enter $_{i,t-3}$	0.089 (0.024) p = 0.000	0.002 (0.005) p = 0.626		0.024 (0.024) p = 0.313								
Enter $_{i,t-3}$ × Pol. Appointee $_{i,t-3}$						-0.165 (0.035) p = 0.000						
Turnover $_{i,t}$ × Pol. Appointee $_{i,t-3}$										-0.007 (0.046) p = 0.886		
Instrument				Career		Political		Pooled		Pooled		Pooled
Observations	6,279	6,279	6,279	6,279	6,279	6,279	6,279	6,279	4,414	4,414	1,865	1,865

Note: First two columns reproduce columns 13 and 15 from Table 3 in main text. All columns labeled FS (first stage) have $Turnover_{i,t}$ as outcome; all columns labeled 2SLS or RF (reduced form) have $MIDOnset_{i,t}$ as outcome. All covariates from main text 2SLS specifications included in all models. SE clustered by country.

Table A6: Heterogeneous Effects of Turnover on Exports, by Appointee Type

	Full Sample						Split Sample					
	Separate Instruments			Interaction			Prior Political, $t-3 = 0$		Prior Political, $t-3 = 1$			
	(FS) (1)	(RF) (2)	(FS) (3)	(2SLS) (4)	(FS) (5)	(2SLS) (6)	(FS) (7)	(2SLS) (8)	(FS) (9)	(2SLS) (10)	(FS) (11)	(2SLS) (12)
Turnover $_{i,t}$				-0.085 (0.042) p = 0.044		-0.686 (1.476) p = 0.642	0.308 (0.021) p = 0.000	-0.079 (0.046) p = 0.084	0.310 (0.021) p = 0.000	-0.076 (0.046) p = 0.099	0.115 (0.029) p = 0.000	-0.256 (0.181) p = 0.157
Enter $_{i,t-3}$												
Career Enter $_{i,t-3}$	0.326 (0.020) p = 0.000	-0.029 (0.013) p = 0.032	0.318 (0.020) p = 0.000									
Political Enter $_{i,t-3}$	0.088 (0.024) p = 0.000	-0.020 (0.025) p = 0.428			0.020 (0.023) p = 0.391							
Enter $_{i,t-3}$ × Pol. Appointee $_{i,t-3}$							-0.191 (0.034) p = 0.000					
Turnover $_{i,t}$ × Pol. Appointee $_{i,t-3}$												
Instrument				Career	Political	Pooled						
Observations	6,768	6,768	6,768	6,768	6,768	6,768	6,768	6,768	4,763	4,763	2,005	2,005

Note: First two columns reproduce columns 16 and 18 from Table 3 in main text. All columns labeled FS (first stage) have $Turnover_{i,t}$ as outcome; all columns labeled 2SLS or RF (reduced form) have $USExports_{i,t}$ as outcome. All covariates from main text 2SLS specifications included in all models. SE clustered by country.

B.3 Foreign diplomatic representation in US

The main text’s theoretical and empirical analyses focus exclusively on the US’s diplomatic representation abroad. Most countries in which the US has an embassy operating, however, also have their own embassies in the US. A thorough examination of two-way diplomatic representation, and of variation in the influence of diplomatic agents on both sides of the exchange, is beyond the scope of this study. Theoretically, I justify restricting attention to one-way diplomatic representation on the grounds that the two channels of diplomacy are simply imperfect substitutes for one another. That is, during a turnover in the US ambassadorial post, the foreign country’s diplomatic representative in the US will be unable to influence US policy in the way that the US ambassador would, so the hypothesized effects of US ambassadorial turnovers will still hold despite the foreign ambassador’s best efforts.

Empirically, I consider here whether the paper’s main results are robust to accounting for foreign countries’ diplomatic representation in the US. Data on foreign diplomatic representation are available on the State Department’s website, though I could only find them on the archived pages from the Obama administration’s State Department Office of the Chief of Protocol.¹⁷ Each country page lists each chief of mission from that country, with their date of appointment, date of presentation of credentials, and rank (Envoy, Ambassador, or Chargé). Unlike the data on US representation abroad, these data do not systematically list each chief of mission’s date of departure from post (or equivalently, the start dates of chargés d’affaires as interim who serve temporarily between ambassadors).

I scraped these data, and created the following variables at the country-year level:

- *foreign COM appoint*_{*i,t*}: indicator for whether a chief of mission from country *i* (either an ambassador or chargé) was appointed to the US in year *t*
- *foreign COM status*_{*i,t*}: categorial variable indicating whether the chief of mission for country *i*’s embassy at the end of year *t* is an ambassador/envoy (pooling the two together), chargé, or if the embassy is not operational
- *foreign COM tenure*_{*i,t*}: years since the chief of mission serving at the end of this year entered office

My goals in collecting the data were, first, to incorporate them into robustness checks (to ensure that the estimated effect of US ambassadorial turnovers persists when we account for the foreign country’s diplomatic representation in the US); and second, to make the data available for other

¹⁷The country pages linked on the main directory page, however, seem to be all broken links (<https://2009-2017.state.gov/s/cpr/rls/c23721.htm>), so I found the individual country pages by scanning through numbers in the urls. See, eg, <https://2009-2017.state.gov/s/cpr/rls/91549.htm>. I have posted the collected data on my website: [withheld for peer review]

Table A7: Relationship between foreign and US ambassadorial turnovers

	DV: Foreign COM Appointment $_{i,t}$					
	(1)	(2)	(3)	(4)	(5)	(6)
US Amb. Turnover $_{i,t}$	0.011 (0.011) p = 0.339	0.009 (0.011) p = 0.416				
US Amb. Entrance $_{i,t}$			0.007 (0.013) p = 0.608	0.0002 (0.013) p = 0.990		
US Amb. Entrance $_{i,t-3}$					-0.012 (0.012) p = 0.309	-0.013 (0.012) p = 0.285
Country and Year FE	No	Yes	No	Yes	No	Yes
Observations	6,279	6,279	6,279	6,279	6,279	6,279

Note: Sample of analysis from main text IV specifications, with no covariates, with standard errors clustered by country.

researchers and facilitate future work that can extend the present analyses to examine more directly the reciprocal nature of diplomatic representation.

As a first pass on examining the foreign ambassador data, I consider whether the timing of foreign ambassadorial turnovers appears to correspond with the timing of US ambassadorial turnovers. Table [A7](#) indicates that this is not the case. The models regress an indicator for for the appointment of a chief of mission from country i in year t , separately, over a US ambassadorial turnover in country i (that is, whether there is a non-zero length of vacancy), an entrance of a US ambassador in country i , or the $t - 3$ entrance instrument, with and without country and year fixed effects. In none of these models do we observe any systematic relationship in the timing of US ambassadorial appointments and foreign countries' ambassadorial appointments in the US. The foreign diplomatic representation variables are incorporated more systematically into the robustness checks below.

B.4 Robustness Tests

Here I report a series of robustness checks for each outcome measure, in Tables [A8](#) and [A9](#). Each table reports the following:

- Column 1: the main text IV specification (column 5 from Table 1, and column 11 from Table 2). These each include a set of “main controls” specific to each outcome, along with three lags of the dependent variables.

- Column 2: main text specification, without the lagged DVs.
- Column 3: main text specification, without the main controls.
- Column 4: main text specification, plus additional controls. For each outcome, the additional controls are the controls included in the other outcome’s specifications, plus bilateral US economic and military aid.
- Column 5: main text specification, with controls for the status of the foreign ambassador in the US, as discussed above. Specifically, these include interactions of *foreign COM status* with both *foreign COM tenure* and *foreign COM appoint* (to allow for the possibility that the effect of appointment or tenure of a chief of mission differs depending on the rank of that chief of mission), all lagged to $t - 4$.
- Column 6: main text specification, but with the countries represented via side accreditation (that is, countries for which the assigned US ambassador is resident in another country) included in the sample, along with a control variable indicating side accreditation.¹⁸
- Column 7 (for MID model only): main text specification, with the sample restricted to countries which, at some point in the timeframe of analysis, engaged in a MID against the US. Intuitively, this is meant to ensure that the main analyses are not “inflating” the sample (and thus overstating the precision of the estimates) by including countries which we could not reasonably expect to ever engage in a MID with the US.¹⁹

The coefficients remain stable across specifications, with the obvious exception of column 7 for the MID models, for which the effect increases in magnitude, by construction.

Finally, given that the MID outcome is binary and highly imbalanced, I further test the robustness of the main result reported in Columns 5 and 8 of Table 2 to alternate specifications. First, Table [A10](#) reports results from a set of IV probit analyses. The four columns report the four combinations of including or excluding country and year fixed effects, with the same controls from the main text specifications. Note that for countries (alternatively, years) which experience no MID onset with the US throughout the sample, their observations are automatically dropped from the IV probit estimation when we include country (alternatively, year) fixed effects.²⁰ Across all four models, the estimated effect of turnover remains positive and highly statistically significant. (The magnitude of the second-stage estimates varies somewhat, as would be expected due to changes in the effective sample.)

¹⁸See discussion in Section [D.4](#)

¹⁹Note that standard definitions of “politically relevant dyads” have no bite here, as all US dyadic relations

Table A8: Robustness checks: MID Onset, 2SLS

	<i>Dependent variable: MID Onset_{i,t}</i>						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Turnover _{i,t}	0.022 (0.011) p = 0.040	0.023 (0.011) p = 0.036	0.022 (0.011) p = 0.044	0.021 (0.011) p = 0.049	0.023 (0.011) p = 0.039	0.022 (0.011) p = 0.042	0.146 (0.068) p = 0.032
Main Controls	Yes	Yes	No	Yes	Yes	Yes	Yes
Lagged DV	Yes	No	Yes	Yes	Yes	Yes	Yes
Additional Controls	No	No	No	Yes	No	No	No
Foreign Ambassadors	No	No	No	No	Yes	No	No
Side Accreditation	No	No	No	No	No	Yes	No
Sample	Full	Full	Full	Full	Full	Full	Disputants
Observations	6,279	6,279	6,279	6,279	6,279	6,698	1,180

Note: Replications of Column 5 of Table 1 from main text, with variations as described in text above.

Table A9: Robustness checks: US Exports, 2SLS

	<i>Dependent variable: ln(US Exports_{i,t})</i>					
	(1)	(2)	(3)	(4)	(5)	(6)
Turnover _{i,t}	-0.101 (0.043) p = 0.018	-0.100 (0.041) p = 0.015	-0.093 (0.044) p = 0.036	-0.102 (0.042) p = 0.017	-0.098 (0.042) p = 0.020	-0.092 (0.042) p = 0.031
Main Controls	Yes	Yes	No	Yes	Yes	Yes
Lagged DV	Yes	No	Yes	Yes	Yes	Yes
Additional Controls	No	No	No	Yes	No	No
Foreign Ambassadors	No	No	No	No	Yes	No
Side Accreditation	No	No	No	No	No	Yes
Observations	6,768	6,768	6,768	6,768	6,768	7,248

Note: Replications of Column 11 of Table 2 from main text, with variations as described in text above.

Table A10: Effect of Turnover on MID Onset, IV Probit

Second Stage: DV is MID Onset $_{i,t}$				
Turnover $_{i,t}$	0.977 (0.321) p=0.002	1.337 (0.419) p= 0.001	0.955 (0.305) p=0.002	1.384 (0.474) p=0.004
First Stage: DV is Turnover $_{i,t}$				
Enter $_{i,t-3}$	0.240 (0.018) p=0.000	0.214 (0.043) p=0.000	0.260 (0.021) p=0.000	0.227 (0.038) p=0.000
Controls	Yes	Yes	Yes	Yes
Country FE	No	Yes	No	Yes
Year FE \times Political Appointee $_{i,t-3}$	No	No	Yes	Yes
N	6279	1180	4330	921

Note: Replication of Column 5 of Table 1, using an IV Probit model rather than 2SLS, including all covariates. SE clustered by country. $+p < 0.1, *p < 0.05, **p < 0.01, ***p < 0.001$

I also consider a range of other limited dependent variable models in Table [A11](#), focusing on the reduced-form relationship between MID outbreaks and the $Enter_{i,t-3}$ instrument. The first two columns report logit models, with and without fixed effects. For columns 3 through 6, the MID outcome is recoded as a count variable, capturing the number of militarized disputes initiated between the US and a given country in a given year.^{[21](#)} With the count measure, I estimate a poisson, a zero-inflated poisson, a negative binomial, and a zero-inflated negative binomial model.^{[22](#)} Each of these models yields results consistent with the linear models in the main text.

satisfy the criteria.

²⁰On the general non-comparability of these estimates with the linear estimates reported in the main text, see [Beck \(2020\)](#).

²¹The count measure is distributed as follows: $6,193 \times 0, 74 \times 1, 8 \times 2, 2 \times 3, 2 \times 4$.

²²For the zero-inflated models, the same set of independent variables is used in the inflation model and in the count model, with the exception that $Enter_{i,t-3}$ is included only in the count model and not in the inflation model.

Table A11: Reduced-form Effect of $\text{Enter}_{i,t-3}$ on MID Onset, Binary and Count

DV:	MID Onset $_{i,t}$ (Binary)		MID Onset $_{i,t}$ (Count)			
	Logit		Poisson	Zero-Inflated Poisson	Negative Binomial	Zero-Inflated Negative Binomial
	(1)	(2)	(3)	(4)	(5)	(6)
Enter $_{i,t-3}$	0.520 (0.222) p = 0.020	0.635 (0.294) p = 0.031	0.397 (0.153) p = 0.010	0.402 (0.145) p = 0.005	0.405 (0.171) p = 0.018	0.355 (0.144) p=0.014
Observations	6,279	6,279	6,279	6,279	6,279	6,279
FE	No	Yes	No	No	No	No

Note: All models include all covariates from Column 5 of Table 1 from main text. FE denotes country FE, year FE, and year \times political appointee FE. Standard errors clustered by country in parentheses.

B.5 Missing Data

As is common in analyses using country-year data, there is a non-negligible degree of missingness in some of the variables included in this study. In particular:

- For the MID IV analyses in the main text, there are 565 observations (out of 6,279) with missing covariate values.
- For the trade IV analyses in the main text, there are 1,352 observations (out of 6,768) with missing covariate values, and 149 observations with missing outcome values.
- There is no missingness in the MID outcome measure.
- There is no missingness in the “treatment” variables or instruments (turnover, vacancy, and appointment).

There are strengths and weaknesses to listwise deletion versus multiple imputation of missing values, and the two approaches can yield non-trivially different results.²³ There is an ongoing debate as to when and how multiple imputation should be used. This study seeks to follow current best practices (King et al., 2001; Honaker and King, 2010; Arel-Bundock and Pelc, 2018) with an acknowledgement that these practices are likely to evolve over time.

The main text analyses use imputed values for covariates, but not for the trade outcome. This follows the recommendation of Arel-Bundock and Pelc (2018, p.243) that “there are good reasons to expect that MI will be most effective where missingness affects auxiliary (or control)

²³For a recent meta-analysis, see Lall (2016)

variables, rather than the main independent or dependent variables of interest”. As it turns out, because of the small degree of missingness in the trade outcome, results are nearly identical when the trade outcome is imputed as well.

Technically, the analyses reported in the main text take the following approach: using [Honaker et al. \(2011\)](#)’s `Amelia II` package in R, create 10 imputed datasets; when an observation is missing a covariate value in the original data, replace it with the average value for that observation across the ten imputed datasets; and run the analyses using those values. This is a slight departure from the algorithm presented in [King et al. \(2001\)](#). I could not find a pre-programmed implementation of that full algorithm that would allow for two-way fixed effects and cluster-robust standard errors in a 2SLS estimation, so for a robustness check, I programmed the algorithm manually. This makes for replication code which is rather unwieldy and inaccessible, but the results are negligibly different from those reported in the main text. (It should be unsurprising that the particular approach to handling missing covariates proves inconsequential in this analysis, since, as we see in Tables [A8](#) and [A9](#), the inclusion or exclusion of the covariates altogether proves inconsequential for the main results.)

Comparing the results from multiple imputation to listwise deletion, we find different patterns across the different models.²⁴ For the MID analyses, the treatment effect estimates are very slightly larger and more precise when using listwise deletion. For the trade analyses, the treatment effect estimates are smaller and less precise when using listwise deletion. This is likely explained by the fact that (i) the covariates in the MID analyses have less missingness, leading to a smaller loss in statistical power due to listwise deletion, and (ii) listwise deletion for the trade analyses drops observations for which commercial diplomacy would have the greatest impact: that is to say, countries with poor practices of reporting economic data are likely to be countries in which US firms will be more reliant on diplomatic support and intervention in order to do business effectively. Finally we should note that repeating the trade analyses with only the fixed effects and lagged dependent and independent variables (and without the covariates that have substantial missingness) yields results nearly identical to those reported in the main text; and as reported in Table [A12](#) below, none of the covariates is correlated with treatment assignment, and thus do not seem to be necessary for achieving unconfoundedness. As such, listwise deletion due to missing covariate values does not seem to be a defensible approach in this case.

B.6 Rotation vs. Vacancy

The instrumental variable design used in this paper examines exogenous variation in the timing of ambassadorial turnover, but not in the length of the vacancy. The IV estimates that use the

²⁴Results are not reported here but can be easily reproduced in the replication code.

continuous measure of vacancy are essentially estimating the effect of the average increase in annual vacancy that is predicted by a $t - 3$ entrance (which, as per Tables 1 and 2, is approximately 5.8% of a year, or 21 days), but does not provide any leverage to differentiate between the impacts of shorter and longer vacancies. Determining whether the length of vacancy matters, beyond just the occurrence of a turnover, is of course an important question with meaningful policy implications, and one that I hope will be taken up in future work. One possible empirical approach would be to leverage other exogenous sources of variation in vacancy length, such as proximity to a US presidential or congressional election, or the composition of the US Senate and/or the committee responsible for holding hearings on ambassadorial appointments; the challenge with using these variables as instruments, however, is that they are unlikely to satisfy an exclusion restriction, as they may affect conflict and cooperation through means other than ambassadorial vacancies. Another approach would involve interacting the aforementioned cross-sectionally-invariant domestic conditions with the $Enter_{i,t-3}$ instrument; preliminary tests of this approach yielded mixed results, which were sensitive to model specification.

C Instrumental Variable Design

This section provides a more thorough justification of two assumptions justifying the IV design: independent assignment of the $Enter_{i,t-3}$ instrument, and the exclusion restriction.

C.1 Independent Assignment

Consistency of the IV estimation requires that, conditional on covariates, the instrument, $Enter_{i,t-3}$, be assigned as-if-randomly with respect to the endogenous regressor, $Turnover_{i,t}$, and with respect to the outcomes of interest. It seems reasonable to assume that any strategic manipulation of the instrument’s assignment would not be manipulation with respect to anticipated turnover per se, but rather manipulation with respect to the outcomes which are expected to be affected by turnovers; so demonstrating independent assignment of the instrument with respect to outcomes (a causally identified reduced form) should be sufficient to show independent assignment with respect to turnover (a causally identified first stage).

As one piece of evidence to justify the plausibility of the assumption of conditionally independent assignment, I consider pairwise correlations of the instrument over each pre-treatment covariate, after residualizing over country and year fixed effects. Results are reported in Table [A12](#). We see that each covariate shows near-zero correlation with the instrument, with the exception of Prior Vacancy $_{i,t-6:t-4}$. In all specifications reported throughout the main text and appendix, I flexibly control for Prior Vacancy $_{i,t-6:t-4}$ by including quadratic and cubic terms. The fact that no covariate other than prior vacancy is a predictor of treatment assignment should increase our confidence that the instrument is not endogenously assigned with respect to potential outcomes. I keep the other covariates in the models to improve precision of the estimated treatment effects, even if they are not needed to justify independent assignment.

We should further note the OLS and IV estimates can serve a sort of bracketing function for estimating the true effect of turnover. Intuitively, we should expect that if endogenous assignment gives rise to bias in the OLS estimate of the effect of *creating* a vacancy (as indicated by $Turnover_{i,t}$), or to bias in the reduced-form estimate of the effect of *filling* a vacancy (that is, the relationship between $Enter_{i,t-3}$ and $Y_{i,t}$), the two biases would point in opposite directions. So if both OLS and IV yield estimates that have the same sign, and the estimates are sufficiently precise, then it is unlikely that the true effect falls outside of the range of the two estimates.

C.2 Exclusion Restriction

The main IV results pertaining to US exports and MID onsets invoked the assumption that the impact of the $Enter_{i,t-3}$ instrument on outcomes was channeled exclusively through the single endogenous regressor, $Turnover_{i,t}$. As is the case in any IV analysis, this exclusion restriction

Table A12: Correlations of covariates with $Enter_{i,t-3}$ instrument

Covariate	ρ
Prior Vacancy $_{i,t-6:t-4}$	-0.0632
log GDP $_{i,t-4}$	0.0128
log Population $_{i,t-4}$	-0.0055
Polity $_{i,t-4}$	0.0111
Δ Polity $_{i,t-4}$	0.0175
US Ally $_{i,t-4}$	0.0007
log Imports from US $_{i,t-4}$	0.0017
log Exports to US $_{i,t-4}$	-0.0043
UNGA Ideal Diff. $_{i,t-4}$	-0.0084
MID Onset $_{i,t-4}$	0.0051
Capabilities $_{i,t-4}$	-0.0030
FTA $_{i,t-4}$	0.0102
GATT/WTO $_{i,t-4}$	-0.0088
log Econ. Aid $_{i,t-4}$	-0.0028
log Mil. Aid $_{i,t-4}$	0.0074

Note: Correlation of each covariate with with $Enter_{i,t-3}$, after residualizing over country and year fixed effects.

can be supported theoretically but not tested empirically. Here I consider how my findings are altered when this restriction is relaxed. This sensitivity analysis follows the framework presented in [Conley et al. \(2012\)](#), making use of the “union of confidence intervals” method that specifies the support of the coefficient representing the exclusion restriction violation. The discussion here focuses on the US Exports outcome; the same analysis can be applied to the MID Onset outcome, yielding the same conclusions.

Formally, the exclusion restriction justifying the $Enter_{i,t-3}$ instrument is the claim that, for any fixed $\tau \in \{0, 1\}$ and conditional on covariates, we have:

$$Y_{it}(Turnover_{it} = \tau, Enter_{i,t-3} = 0) = Y_{it}(Turnover_{it} = \tau, Enter_{i,t-3} = 1) = Y_{it}(Turnover_{it} = \tau),$$

where $Y_{it}(\tau, e)$ is the potential outcome of Y_{it} given that $Turnover_{it} = \tau$ and $Enter_{i,t-3} = e$. In [Conley et al. \(2012\)](#)’s framework, this can be expressed as the “dogmatic prior belief that γ is identically 0” in the following system of equations:

$$Y_{it} = \beta Turnover_{it} + \gamma Enter_{i,t-3} + X'_{i,t-4} \theta + \epsilon_{it} \quad (1)$$

$$Turnover_{it} = \pi Enter_{i,t-3} + X'_{i,t-4} \phi + \eta_{it}, \quad (2)$$

where $X_{i,t-4}$ includes all regressors (controls, fixed effects, and lagged DV) from the main text specifications. If the value of γ were not zero, and assuming we knew it to be the value $\gamma_0 \in \mathcal{G}$,

we could simply subtract $\gamma_0 \text{Enter}_{i,t-3}$ from both sides and proceed with two-stage least squares estimation of β in the new equation:

$$(Y_{it} - \gamma_0 \text{Enter}_{i,t-3}) = \beta \text{Turnover}_{it} + X'_{i,t-4} \theta + \epsilon_{it} \quad (3)$$

The key to this inference strategy is to specify \mathcal{G} , the support of the unobservable exclusion restriction violation represented by γ .

What is a reasonable specification of \mathcal{G} in this context? The most plausible violation of the exclusion restriction would be the effect of a $t - 3$ entrance on the functioning of the embassy in the years between $t - 3$ and t . That is to say, $Y_{it}(\tau, 0)$ may not equal $Y_{it}(\tau, 1)$ because of differences in the amount and timing of vacancy in the intervening years.²⁵ Intuitively, it is easy to see why, for fixed v and all else held constant, bilateral relations in t would exhibit better outcomes if there were an entrance in $t - 3$ than if there were not. We should expect that an entrance in $t - 3$ would mean less total vacancy in the time between $t - 3$ and t (because the alternative to an entrance in $t - 3$ is, most likely, an entrance in $t - 2$ or $t - 1$); and, if there is an ambassador in place at the start of year t , that she would have been more experienced on the job if she entered in $t - 3$ than if she had not.

Both patterns of intervening vacancy and ambassador tenure are borne out in the data. The first column of Table [A13](#) shows the effect of $\text{Enter}_{i,t-3}$ on $\text{Vacancy}_{i,t-2:t-1}$, the total vacancy of the ambassadorial post in country i in years $t - 2$ and $t - 1$, including all pre-treatment covariates and fixed effects used in the first stage regression. We see that $\text{Enter}_{i,t-3}$ is a consistently negative predictor of the total vacancy in the years $t - 2$ and $t - 1$. Under the basic assumption that less vacancy in these years is better (or at least no worse) for exports in t , this violation of the exclusion restriction implies a value of $\gamma \geq 0$. The second column of Table [A13](#) reports results of a similar analysis with an outcome measure of Tenure_{it} , the number of days that the ambassador serving on January 1st of year t has been in office (restricting the sample to observations for which there is an ambassador serving on January 1st). We see that a $t - 3$ entrance is a strong positive predictor of ambassador experience at the beginning of year t ; and under the similar assumption that more experienced ambassadors are no worse than less experienced ambassadors, this again implies a value of $\gamma \geq 0$.

If all plausible exclusion restriction violations fit this pattern, the implication for the IV estimation is straightforward: assuming a non-negative γ , the lefthand side of Equation [\(3\)](#) can be no higher than the lefthand side in Equation [\(1\)](#), so the 2SLS estimate of β that follows from the assumption that γ is precisely zero provides an upper bound on the (negative) impact of turnovers (or in other words, a lower bound on the magnitude of lost exports resulting from

²⁵Note that third-party responses to turnovers would not constitute exclusion restriction violations, as any such responses would instead constitute mediators in the causal relationship between turnovers and outcomes.

Table A13: Exclusion Restriction Probe: Intervening Vacancy and Ambassadorial Tenure

DV:	Vacancy _{<i>i,t-2:t-1</i>}	Amb. Jan 1 Tenure _{<i>i,t</i>}
	(1)	(2)
Enter _{<i>i,t-3</i>}	-0.202 (0.010) p = 0.000	187.321 (14.383) p = 0.000
Observations	6,768	6,005

Note: Models include the same set of regressors from all main text IV specifications. SE clustered by country.

turnovers). IV estimation conducted as if the “dogmatic” exclusion restriction were perfectly valid will thus bias the results towards zero, if at all.

C.3 Other IV Considerations

An article by [Sovey and Green \(2011\)](#) provides a useful guide to presenting and interpreting results of an instrumental variables analysis. Following their checklist (Table 3), previous discussion in this appendix has considered the LATE estimand and the generalizability of effects local to the compliers (Section [A.2](#)); the independence of treatment assignment (Section [C.1](#)); the exclusion restriction (Section [C.2](#)); and the strength of the instrument (F-statistics reported in Tables 2 and 4). Two other issues remain to be addressed: monotonicity, and SUTVA.

An important assumption in any IV design is monotonicity, or the absence of “defiers”—observations whose treatment uptake (both observed and counterfactual) is opposite its treatment assignment. What would a violation of this condition mean in the present context? It would mean that for a given country-year, the ambassadorial post is (1) experiencing a turnover this year and did not have an appointment in $t - 3$, *and* (2) if there had (counterfactually) been an appointment in $t - 3$, the post would not currently be experiencing a turnover; or alternatively, that the post is (1) not currently experiencing a turnover and did have an appointment in year $t - 3$, *and* (2) if there had (counterfactually) not been an appointment in $t - 3$, the post would currently be experiencing a turnover. Put simply, this would characterize conditions in which an ambassadorial term is intended to be shorter or longer than three years.

[Jett \(2014\)](#) identifies two conditions under which we might expect the convention of a three-year term to be violated. First is a non-career appointment that occurs in a president’s second term, when there are more or less than three years remaining in the term and the appointee is expected to serve until the president leaves office (p.48 and p.65). Second, as Jett writes: “Occasionally, conditions in a country might be so difficult and dangerous, such

as those in present-day Iraq and Afghanistan, that the tour of duty is reduced to two years, but those exceptions are rare” (Jett 2014, p.48). For the first condition, analyses in Section B.2 demonstrated that results are robust when restricting attention to career appointees (and excluding political appointees). The second condition is less straightforward to assess. Because ambassadorial term lengths arise from a strong but informal norm, rather than a formal rule, there is no direct evidence as to precisely which ambassadorial appointments were intended to last two years rather than three. Given the conditions referenced by Jett, I conduct the following robustness check: I consider the country-years experiencing either a civil war or an interstate war, as coded by the Correlates of War dataset (Sarkees and Wayman, 2010), as constituting a set of plausible “defiers”, and repeat the analyses without these observations. Results are robust to this exclusion. There may be other ways of identifying possible defiers which I am unaware of, but this seemed reasonable as a first approximation.

The final consideration mentioned by Sovey and Green (2011) is the stable unit treatment value assumption, or SUTVA. As is the case in any analysis using cross-national time-series data, violations of this condition are difficult to rule out conclusively. The most natural SUTVA concern in this case would be the possibility of within-country temporal “spillovers”; these concerns are partly addressed in Section C.2, and should be largely mitigated by the inclusion of country fixed effects as well as standard errors being clustered by country.

D Definitions

D.1 Embassies and Ambassadors

For the early years of the sample, the Office of the Historian data distinguishes between embassies, overseen by ambassadors, and legations, overseen by envoys. Prior to World War II, the distinction between embassy and legation denoted a country’s status in the international hierarchy; after World War II, the distinction was gradually eroded, as all legations were eventually formally elevated to embassies. This process of diplomatic “inflation” was well under way by the timeframe of my study:²⁶ only eleven chiefs of mission in the data appointed after 1960 held the title of “Envoy”, as compared to 2,709 appointed in that period with the title “Ambassador”. I elide this distinction, and use the term “ambassador” to refer collectively to ambassadors and envoys. Likewise, in constructing the $\text{Eligible}_{i,t}$ variable, I consider embassies and legations to be on equal footing, and refer to them collectively as “embassies”.

D.2 Chargés d’affaires

In contemporary US diplomatic practice, there are two kinds of chargés d’affaires. As defined by the US State Department:

Formerly, a chargé d’affaires was the title of a chief of mission, inferior in rank to an ambassador or a minister. It is still used as the title of the head of a US mission where the US and other nation do not have full diplomatic relations. Today with the a.i. (ad interim) added, it designates the senior officer taking charge for the interval when a chief of mission is absent from his/her post or the position is vacant.²⁷

In diplomatic practice more broadly, what the State Department calls a chargé d’affaires (*not* ad interim) is alternatively referred to as “chargé d’affaires et pied”.

Whenever the term “chargé” or “chargés d’affaires” appears in the present study, it is referring to a chargés d’affaires ad interim. A consequence of restricting the sample to conditions of “normal” diplomatic relations (as defined below) is that it removes conditions in which the US is represented by a chargés d’affaires (et pied) (or by other chiefs of mission holding titles such as Principal Officer, Chief, Director, or Representative, under non-normal diplomatic relations). Whenever such a representative does remain in the data (because “eligibility” in the sample is determined at the start of year $t - 3$, rather than concurrently), the observation is coded as vacant (ie. there is not an ambassador present).

²⁶See, for instance, Table 1 in Small and Singer (1973)

²⁷<https://diplomacy.state.gov/discover-diplomacy/diplomatic-dictionary/>

D.3 Career and Political Appointees

At various points in the main text and appendix, I differentiate between “career” and “politically appointed” US ambassadors. This terminology, though commonplace, is misleading: all ambassadors are “political” appointees, in the legalistic sense, in that they are principal officers of the State Department whose appointment requires Presidential nomination and Senate confirmation. I follow the convention of using the term “political ambassador” to refer to ambassadors who are not career Foreign Service Officers. It is worth noting that, although an appointee’s status—career Foreign Service Officer (FSO) or not—does allow for a binary classification, the difference between the two classes of ambassador may in reality be more a matter of degree than of kind: many non-career ambassadors have held positions in government, either in elected office or in other positions in the foreign policy bureaucracy; and the Foreign Service Officers who go on to become ambassadors tend to be the most “politically”-minded, frequently holding positions in the White House or other executive agencies in between their foreign service tours.

D.4 Normal Diplomatic Relations

For the purposes of this study, I employ a definition of “normal” diplomatic relations which is meant to capture, intuitively, conditions in which we can plausibly claim that the appointment (or not) of an ambassador is as-if-random. Normal diplomatic relations in this context should be understood as a necessary but not sufficient condition for as-if-randomness; the “IV Design” section of the main text explains the endogeneity concerns that remain even when restricting the sample to these conditions, and the study’s empirical strategy to address those concerns.

Normal diplomatic relations for country i in year t are coded by the variable $Eligible_{it}$. This variable takes on the value of zero if any of the following conditions hold:

- the US has not yet recognized country i , or has never sent an ambassador to country i as an independent country;
- the US and country i have severed diplomatic relations for some portion of year t , as recorded in the State Department Office of the Historian’s “Guide to to the United States’ History of Recognition, Diplomatic, and Consular Relations, by Country, since 1776”;²⁸
- there is not a US embassy operating in country i for all of year t , or the ambassador assigned to the country is resident in another country’s embassy (a practice known as “side accreditation”);²⁹ or

²⁸history.state.gov/countries/all

²⁹See Tables [A8](#) and [A9](#) for robustness checks which keep these cases in the sample, and include side accreditation as a control variable.

- a diplomatic dispute occurs between the US and country i during or within a year prior to a vacancy spell which spans part of year t (e.g. if a dispute occurs in year t , and a vacancy begins later that year, and the vacancy spell extends into year $t + 1$, then $Eligible_{i,t+1} = 0$).

Otherwise, $Eligible_{it} = 1$. Diplomatic disputes were identified through a combination of the “diplomatic sanctions” variable in the Threat and Imposition of Economic Sanctions (TIES) dataset (Morgan et al., 2014), and searches through the New York Times archives for cases in which diplomats were deliberately expelled or withdrawn. From the original sample of 8,679 country-year observations (from 1960-2014), 1,640 are coded as $Eligible_{i,t-3} = 0$. Table A14 reports the cases for which $Eligible_{it} = 0$, omitting those cases that were defined to be ineligible by virtue of the US and host country having not yet exchanged ambassadors at any point in that country’s history.

The decision to exclude the ineligible cases is driven by a number of methodological and substantive considerations. First, observations of non-normal diplomatic relations will generally violate the “positivity condition” necessary for the estimation of average treatment effects. Aronow and Samii (2016) provide the following definition and explanation:

Positivity, loosely speaking, requires that, for all values of [covariate vector] X_i that appear in the target population, there is some probability of observing different values of [treatment condition] D_i . If, for example, all units with a given covariate profile always have the same treatment condition, then one cannot estimate causal effects for these units. When positivity fails, then the best that one can do without introducing more assumptions (that provide a basis for extrapolation and interpolation) is to estimate a representative causal effect for the subset of the target population for which positivity does hold (Petersen et al. 2011). Formally, the positivity assumption is as follows:

$$Pr[D_i = d|X_i = x] > 0, Pr[D_i = d'|X_i = x] > 0,$$

for all values of x in the target population represented by the nominal sample.

Applied to the present study, this condition tells us that we cannot estimate treatment effects for observations that are ineligible to receive an ambassador in $t - 3$. Recall that the sample for the IV estimation (and, for comparability, the OLS sample as well) is limited to observations of $Eligible_{i,t-3} = 1$, rather than to observations of $Eligible_{i,t}$, so as to avoid selecting the sample on a post-treatment variable.

In the context of the IV design, including observations of non-normal relations poses an additional problem of weakening the first-stage relationship: when diplomatic relations ex-

perience an extended interruption, the fact that an ambassador did not enter office in $t - 3$ ($Enter_{i,t-3} = 0$) is a poor predictor of a lack of vacancy in year t ($Turnover_{i,t} = 0$), whereas we would expect a positive correlation between the two under normal diplomatic relations. Similarly, for non-normal observations, the reduced-form relationship should not have the same effect as in the normal-relation sample: that is, under conditions of normal relations, the lack of an entrance in $t - 3$ ($Enter_{i,t-3} = 0$) should predict higher volumes of US exports in year t ; but we expect no such effect in, for example, post-revolutionary Iran. Finally, in the rare case that an ambassador does enter office under non-normal relations, we might expect her treatment effect (or the treatment effect of her subsequent turnover) to differ substantially from the rest of the sample: she may be severely constrained in her ability to advance US interests vis-a-vis her host government; or she may pursue different priorities, for instance, forgoing export promotion in favor of addressing the particular conflict that led relations to interrupted; or the situation may warrant the attention of higher authorities within the State Department or the White House, diminishing the ambassador's role in the bilateral relationship.

D.5 Excluded Cases

The following section reports the cases for which $Eligible_{it} = 0$, omitting those cases that were defined to be ineligible by virtue of the US and host country having not yet established relations and exchanged ambassadors at any point in that country's history.

Table A14: Ineligible cases

Country	Ineligible Years	Description
Afghanistan	1979-2002	Ambassador assassinated; embassy later closed due to security concerns
Algeria	1967-1975	Diplomatic relations with US severed with onset of Arab-Israeli War
Antigua & Barbuda	1994-2014	Embassy closed; ambassador resident at Bridgetown, Barbados
Belarus	1997	Ambassador recalled as part of broader sanctioning strategy
	2008-2014	Diplomatic dispute resulting from US criticism of Lukashenko government
Bolivia	1980-1981	Ambassador withdrawn following coup
	2008-2014	US ambassador expelled for accusation of backing opposition groups
Burundi	1966-1968	US ambassador expelled for accusation of conspiring against government
Cambodia	1964-1970	Ended diplomatic relations with US in response to US bombing campaign in Cambodia
	1975-1994	US ended diplomatic relations following government collapse
Chad	1980-1983	Embassy closed due to civil conflict
China	1995-1996	Dispute over Taiwanese president visit to US
Comoros	1993-2014	Embassy closed; ambassador resident at Port Louis, Mauritius
Congo	1965-1979	Closed US embassy due to mistreatment of US diplomats
Cuba	1960-2014	Diplomatic relations ended following Castro government taking power
DRC	1975	US ambassador expelled for accusation of conspiring against government
Dominica	1980-2014	Ambassador resident at Bridgetown, Barbados
Dominican Republic	1960-1964	Diplomatic dispute following Trujillo government involvement in assassination attempt against Venezuelan President
Ecuador	1967-1968	Ambassador expelled for criticizing Ecuadorian president
	2011-2012	Diplomatic dispute following release of WikiLeaks cable
Egypt	1967-1974	Diplomatic relations with US severed with onset of Arab-Israeli War
Equatorial Guinea	1976-1981	US diplomats declared persona non grata
	1995-2006	Embassy closed; ambassador resident at Yaounde, Cameroon
Ethiopia	1980-1992	Stopped exchanging ambassadors as part of broader sanction strategy
Fiji	2001-2003	Diplomatic dispute following hostage crisis
Grenada	1976-2014	Ambassador resident at Bridgetown, Barbados
Guinea-Bissau	1998-2003	Embassy closed due to civil conflict
Haiti	1963-1964	Diplomatic relations ended as part of strategy to overthrow Duvalier government
	1992-1993	Ambassador recalled in response to Haitian military attacks on political opposition

Ineligible Cases (Continued)

Country	Ineligible Years	Description
Iran	1979-2014	Diplomatic relations ended following revolution
Iraq	1967-1985	Diplomatic relations with US severed with onset of Arab-Israeli War
	1988	Dispute over US contacts with Kurdish groups
	1990-2004	Diplomatic relations ended between onset of first Gulf War and overthrow of Hussein government
Kiribati	1999-2014	Ambassador resident at Suva, Fiji
Kuwait	1990-1991	Embassy closed during Iraqi invasion
Laos	1975-1992	Diplomatic relations ended with founding of Lao People's Democratic Republic
Lebanon	1989-1990	Embassy closed due to civil conflict
Libya	1972-2009	Libya designated as state sponsor of terrorism
	2011	Diplomatic relations ended as part of strategy to overthrow Gaddafi government
Liechtenstein	1998-2014	Ambassador resident at Bern, Switzerland
Luxembourg	1960-2014	Ambassador resident at Brussels
Maldives	1967-2014	Ambassador resident at Colombo, Sri Lanka
Mauritania	1967-1971	Diplomatic relations with US severed with onset of Arab-Israeli War
Myanmar	1990-2012	Stopped exchanging ambassadors as part of broader sanction strategy
Nauru	2000-2014	Ambassador resident at Suva, Fiji
	1984	Diplomats expelled for accusation of plot to assassinate foreign minister
	1988-1990	Ambassador expelled for accusation of interfering in internal affairs
Palau	1997-2004	Ambassador resident at Manila, Phillipines
	1964	Diplomatic relations with US ended due to clashes between US and Panamanian troops in the Canal Zone
	1968	Diplomatic relations ended following coup
Panama	1989	Ambassador recalled as part of strategy to pressure Noriega into resignation
	1962-1963	Diplomatic relations interrupted following coup
Peru	1962-1963	Diplomatic relations interrupted following coup
Republic of Vietnam	1965	Diplomats expelled for accusation of dealing independently with tribal groups
Poland	1983-1988	Refused to receive American ambassador

Ineligible Cases (Continued)

Country	Ineligible Years	Description
Russia	1987	Diplomats expelled for accusation of espionage
	1996-1998	Dispute over US airstrikes in Iraq
Samoa	1976-2014	Ambassador resident at Wellington, New Zealand
San Marino	2008-2014	Ambassador resident at Rome
Sao Tome and Principe	1977-2014	Ambassador resident at Libreville, Gabon
Seychelles	1996-2014	Ambassador resident at Port Louis, Mauritius
Solomon Islands	1979-1988	Ambassador resident at Port Moresby, Papua New Guinea
	1993-2014	Ambassador resident at Port Moresby, Papua New Guinea
Somalia	1991-2014	Embassy closed following government collapse
South Africa	1986	Diplomats expelled in response to South Africa aggression against neighboring countries
St. Lucia	1980-2014	Ambassador resident at Bridgetown, Barbados
St. Vincent	1982-2014	Ambassador resident at Bridgetown, Barbados
Sudan	1967-1973	Diplomatic relations with US severed with onset of Arab-Israeli War
	1996-2014	Diplomatic relations ended as part of broader sanctioning strategy, for harboring terrorists and human rights abuses
Syria	1965	Diplomats expelled for accusation of espionage
	1967-1974	Diplomatic relations with US severed with onset of Arab-Israeli War
	2005-2014	Ambassador withdrawn in response to Syrian involvement in assassination of Lebanese Prime Minister; embassy then closed due to civil conflict
Taiwan	1979-2014	Recognized government of People's Republic of China, moved embassy from Taipei to Beijing
Tanzania	1965-1966	Diplomats expelled for accusation of subversive activity
Tonga	1999-2014	Amassador resident at Suva, Fiji
Tuvalu	1980-2014	Amassador resident at Suva, Fiji
Uganda	1973-1980	Embassy closed due to security threats
Vanuatu	1988-2014	Ambassador resident at Port Moresby, Papua New Guinea
Venezuela	2010-2014	Diplomats expelled for accusation of subversive activity
Yemen Arab Republic	1962-1972	Diplomatic relations with US severed with onset of Arab-Israeli War
Yugoslavia / Serbia	1992-2002	US non-recognition of successor state to Yugoslavia following dissolution; diplomatic relations eventually re-established with Republic of Serbia

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