

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.

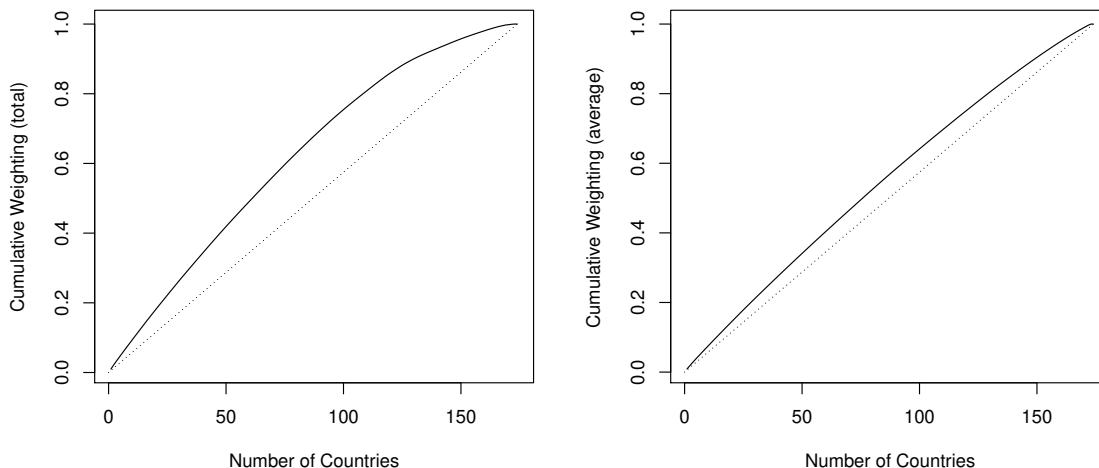
Figure A1 reports a similar analysis, aggregating regression weights by country. In both figures, countries are aligned on the horizontal axis according to their regression weighting; each country’s aggregate weighting is used in the left panel, and its average weighting (aggregate weighting divided 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 $_{i,t}$	0.427	0	1	0.495	Office of the Historian (2018a); indicator for any ambassadorial vacancy or turnover
Pct. Vacant $_{i,t}$	0.149	0	1	0.253	Office of the Historian (2018a); portion of the year that the ambassadorial post was vacant
Enter $_{i,t-3}$	0.313	0	1	0.464	Office of the Historian (2018a); indicator for any ambassador entrance into office in $t-3$
MID Onset $_{i,t}$	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 $_{i,t}$ (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.

	Mean (Nominal Sample)	Mean (Effective Sample)	Mean (Compliers)	Source and Notes			
Pre-treatment Covariates	Min	Max	SD				
Prior Vac $_{i,t-6;t-4}$	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 $_{i,t-4}$ (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 <i>Amelia II</i> R package (Honaker et al., 2011)
Population $_{i,t-4}$ (log)	15.763	9.88	22.68	1.65	15.717	15.786	World Bank (2018); missing values imputed
Exports to US $_{i,t-4}$ (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 $_{i,t-4}$	0.712	-10	10.815	7.336	0.535	0.577	Marshall and Jaggers (2002)'s Polity 2 score; missing values imputed
Δ Polity $_{i,t-4}$	0.086	-15	16	1.861	0.138	0.222	Change in polity from $t-5$ to $t-4$
Capabilities $_{i,t-4}$	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 $_{i,t-4}$	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 $_{i,t-4}$	0.018	0	1	0.132	0.018	0.007	Dür et al. (2014); indicator for whether US and country i are party to a free trade agreement
GATT or WTO $_{i,t-4}$	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 $_{i,t-4}$	12.961	0	22.96	7.482	13.355	15.933	USAID (2018); negative values smoothed across adjacent years
Mil. Aid $_{i,t-4}$	9.031	0	23.12	7.666	9.231	10.942	USAID (2018)
Political Appointee $_{i,t-4}$	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 $_{i,t-4}$	0.067	0	1	0.249	0.063	0.039	Benoit et al. (2018)'s <i>sotu</i> corpus; indicator for whether country i was mentioned in the US President's State of the Union Address
Pres. Visit $_{i,t-4}$	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.)

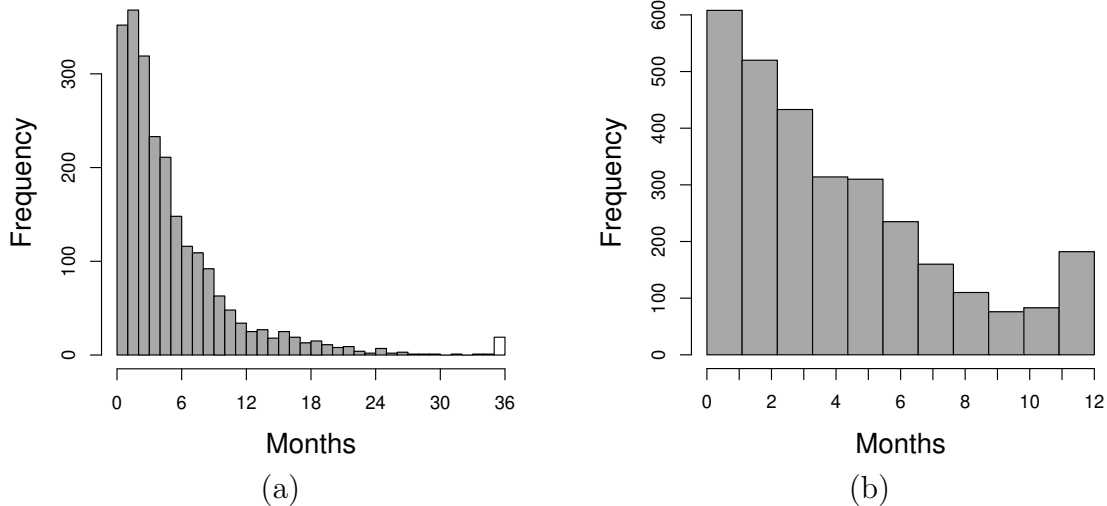
- Compliers are slightly more likely to receive career ambassadors, rather than politically appointed ambassadors. Results in Tables A5 and A6 suggest that career ambassadors are the ones driving the aggregate effect of turnovers on conflict (though see caveats below on interpretation of those analyses).

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 replicate models 7 and 9 (for MID outcomes) and models 16 and 18 (for trade outcomes) from the main text analyses, which separate the instrument into *Career* $Enter_{i,t-3}$ and *Political* $Enter_{i,t-3}$, and includes both in the same first-stage and reduced-form models. 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 $PoliticalAppointee_{i,t-3}=0$, and columns 11 and 12 subset to the $PoliticalAppointee_{i,t-3}=1$ 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 $_{i,t-3}$												
Instrument				Career	Political	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 7 and 9 from Table 2 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 Table 2 included. 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 4 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 Table 4 included. 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/r1s/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/r1s/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 Table 2, 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 2, and column 14 from Table 4). 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 2 from main text, with variations as described 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 14 of Table 4 from main text, with variations as described 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 2, 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 Columns 5 and 8 of Table 2. 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 $_{i,t-2:t-1}$	Amb. Jan 1 Tenure $_{i,t}$
	(1)	(2)
Enter $_{i,t-3}$	-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 Table 4 models. 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 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 of 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 2008-2014	Ambassador recalled as part of broader sanctioning strategy Diplomatic dispute resulting from US criticism of Lukashenko government
Bolivia	1980-1981 2008-2014	Ambassador withdrawn following coup US ambassador expelled for accusation of backing opposition groups
Burundi	1966-1968	US ambassador expelled for accusation of conspiring against government
Cambodia	1964-1970 1975-1994	Ended diplomatic relations with US in response to US bombing campaign in Cambodia 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 2011-2012	Ambassador expelled for criticizing Ecuadorian president 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 1995-2006	US diplomats declared persona non grata 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 1992-1993	Diplomatic relations ended as part of strategy to overthrow Duvalier government 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
Nicaragua	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
Panama	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
Peru	1989	Ambassador recalled as part of strategy to pressure Noriega into resignation
	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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