

SUPPLEMENTAL ONLINE APPENDICES

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APPENDIX A: DATA

This appendix describes the data underlying the empirical analysis.

Table A1: Top bilateral aid donors, 1960-2009

Country	Bilateral aid, net disbursements (2010 US\$, Millions)		Share of total DAC bilateral aid
	Annual average	Std. Dev.	Annual average (%)
United States	13451.5	4972.1	28.5
France	6802.8	1133.8	14.6
Japan	6347.9	3181.8	12.5
Germany	4771.9	1474.8	9.8
United Kingdom	3306.2	1442.5	6.9

Notes: Data from OECD, 1960-2009.

A2. Sample of U.S. aid recipients

Albania	Gambia	Oman
Algeria	Georgia	Pakistan
Angola	Ghana	Palau
Antigua and Barbuda	Grenada	Panama
Argentina	Guatemala	Papua New Guinea
Armenia	Guinea	Paraguay
Azerbaijan	Guinea-Bissau	Peru
Bahamas, The	Guyana	Philippines
Bahrain	Haiti	Poland
Bangladesh	Honduras	Qatar
Barbados	Hungary	Romania
Belarus	India	Russia
Belize	Indonesia	Rwanda
Benin	Iran, Islamic Rep.	Samoa
Bhutan	Iraq	Saudi Arabia
Bolivia	Israel	Senegal
Bosnia and Herzegovina	Jamaica	Serbia
Botswana	Jordan	Seychelles
Brazil	Kazakhstan	Sierra Leone
Bulgaria	Kenya	Singapore
Burkina Faso	Kiribati	Slovak Republic
Burundi	Korea, Rep.	Slovenia
Cambodia	Kuwait	Solomon Islands
Cameroon	Kyrgyz Republic	South Africa
Cape Verde	Latvia	Sri Lanka
Central African Republic	Lebanon	St. Kitts and Nevis
Chad	Lesotho	St. Lucia
Chile	Liberia	St. Vincent and the Grenadines
China	Libya	Sudan
Colombia	Lithuania	Suriname
Comoros	Macedonia, FYR	Swaziland
Congo, Dem. Rep.	Madagascar	Syria
Congo, Rep.	Malawi	Tajikistan
Costa Rica	Malaysia	Tanzania
Cote d'Ivoire	Maldives	Thailand
Croatia	Mali	Togo
Cyprus	Malta	Tonga
Czech Republic	Mauritania	Trinidad and Tobago
Djibouti	Mauritius	Tunisia
Dominica	Mexico	Turkey
Dominican Republic	Moldova	Turkmenistan
Ecuador	Mongolia	Uganda
Egypt, Arab Rep.	Montenegro	Ukraine
El Salvador	Morocco	United Arab Emirates
Equatorial Guinea	Mozambique	Uruguay
Eritrea	Namibia	Uzbekistan
Estonia	Nepal	Vanuatu
Ethiopia	Nicaragua	Venezuela, RB
Fiji	Niger	Vietnam
Gabon	Nigeria	Zambia
		Zimbabwe

Table A3. Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Political rights	3853	4.16	2.02	1.00	7.00
Log U.S. economic aid (2000 US\$)	3853	13.43	6.25	0.00	23.27
Log U.S. military aid (2000 US\$)	3853	9.55	6.48	0.00	22.58
Log U.S. econ. and mil. aid (2000 US\$)	3853	14.44	5.21	0.00	23.39
<i>Instrumental variable</i>					
FRAG x P	3853	58.13	24.99	0.00	95.35
Legislative fragmentation (FRAG)	3853	85.73	9.57	65.74	97.93
Prob. of receiving U.S. aid (P)	3853	0.68	0.29	0.00	0.97
<i>Controls</i>					
Log GDP per capita (2000 US\$)	3853	6.94	1.26	4.13	10.75
GDP per capita growth (% annual)	3853	1.80	6.79	-50.00	90.50
Log population	3853	15.39	1.95	9.86	21.00
UN Security Council member	3853	0.05	0.22	0.00	1.00
U.S. military ally	3853	0.19	0.39	0.00	1.00
Log U.S. exports (2000 US\$)	3853	17.53	4.05	0.00	25.97

Notes: Summary statistics corresponding to baseline specification, Table 3 columns 1 and 2.

APPENDIX B: ROBUSTNESS

This appendix presents and discusses the results associated with section “III-C. Robustness checks” in the main text.

B1. Alternate specifications

Specifications that vary fixed effects. The core findings do not hinge on the inclusion of fixed effects in the baseline specification. Columns 1-3 in Table B1 show that instrumented aid harms political rights in specifications that vary the set of fixed effects. For instance, column 1 excludes country fixed effects but does appropriately control for the time-invariant component of the instrument, P_i . In this specification, more frequent U.S. aid recipients (i.e., higher value of P_i) are associated with *lower* levels of repression. This negative coefficient is consistent with the U.S. government’s stated intentions to disburse aid to less repressive countries. Furthermore, excluding year fixed effects (column 2) as well as both year and country fixed effects (column 3) does not affect the core finding. On balance, across all 3 specifications (which appropriately control the relevant constituent terms of the instrumental variable in both the first and second stage regressions), instrumented aid harms political rights and the coefficient estimate is similar in magnitude those reported in table 2.

Distributed lag specification. The baseline specification presumes that effect of U.S. aid in year t has a contemporaneous effect on political rights. Yet, given the mechanisms discussed in the article, the effect may not be immediate but reflect the effect from previous years. To investigate this possibility, I estimate distributed lag specifications that include lagged aid receipts as an additional control (table B1, columns 4 and 5). In these specifications, contemporaneous U.S. aid is instrumented. In a specification with a one year lag of aid receipts as a control (column 4), instrumented aid (in year t) continues to have a positive and significant effect on *POLITICAL RIGHTS*. And summing the lagged and contemporaneous effect yields an estimate of 0.143 (=0.39+0.247), which is similar in magnitude to the baseline estimates in table 2, columns 2 and 3. A specification with 1 and 2 year lagged aid as additional controls generates similar results for the effect of contemporaneous (instrumented) U.S. aid on repression (results not reported). In column 5, a one year lead of aid (i.e., aid in year $t+1$) is included as an additional control as means to potentially capture the full temporal dynamics of aid on repression.

The main effect of contemporaneous aid remains positive and significant, and the “net” effect generates an estimate of 0.13, which is similar in magnitude to the baseline finding in table 2.

Averaged data. A potential concern is that the measure of political rights does not vary that much annually and as such may be better modeled at a lower temporal frequency. Columns 6 and 7 in table B1 report the effect of U.S. aid on *POLITICAL RIGHTS* where all the data is averaged over 2 and 5-year intervals. The former specification also corresponds to the frequency at which *FRAG_t* varies since elections in the House of Representatives are held every two years. Across both specifications, the core findings hold. In column 6, 2 year averaged aid has a similar effect (coefficient=0.160) to the baseline results that uses annual aid receipts. In column 7, the estimated effect for 5 year averaged aid is larger and remains statistically significant despite a much smaller estimating sample. In this specification, there are 557 observations compared to 3853 observations in the baseline specifications.

Standard errors. While the baseline specifications account for potential within-country variation in the errors (by clustering at the country level), an additional concern is the possibility of temporal correlation in the errors. This may be present since the instrumental variable leverages year “shocks” to the legislative fragmentation of the House of Representatives to identify the effect of U.S. aid on repression. A solution to this worry is to cluster the standard errors at both the year and country level (i.e., two-way standard errors). Moreover, as additional robustness checks, I re-estimate the baseline specifications with standard errors clustered at the *YEAR* and *REGION* x *YEAR* level. The results are presented in column 8. The two-way standard errors are reported in parentheses, while those clustered at the *REGION* x *YEAR* and *YEAR* levels are reported in square brackets and braces respectively. As is clear in column 8, regardless of the level of clustered standard errors, U.S. aid remains a robust determinant of a deterioration of *POLITICAL RIGHTS*. Interestingly, the standard errors do not increase when they are clustered at the year level, which mitigate concerns of negative autocorrelation in errors over time (and as such, U.S. economic aid is not mean-reverting).

Table B1. Alternate specifications

Dependent variable:	Political rights							
	Constituent terms of instrument			Lags and leads		Averaged data		(8)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
<i>Instrumented variables</i>								
Log U.S. aid (2000 US\$)	0.137 (0.067)**	0.135 (0.061)**	0.149 (0.069)**	0.39 (0.135)***	1.327 (0.640)**			0.157 (0.060)*** (0.030)*** (0.032)***
Log U.S. aid (2000 US\$), 2 year average						0.16 (0.060)**		
Log U.S. aid (2000 US\$), 5 year average							0.244 (0.116) **	
<i>Additional controls</i>								
P_i	-2.227 (1.019)**		-2.13 (1.012)**					
$FRAG_t$		-0.013 (0.005)** *	-0.048 (0.010)** *					
Log U.S. aid (2000 US\$) in year t-1				-0.247 (0.086)***	-0.537 (0.264)**			
Log U.S. aid (2000 US\$) in year t+1					-0.603 (0.296)**			
Recipient characteristics	Y	Y	Y	Y	Y	Y	Y	Y
Country fixed effects	N	Y	N	Y	Y	Y	Y	Y
Year fixed effects	Y	N	N	Y	Y	Y	Y	Y
R-squared	0.08	0.65	.	0.42	.	0.69	0.7	0.64
Number of observations	3853	3853	3853	3750	3553	1704	557	3853
Number of countries	151	151	151	151	151	151	143	151

Notes: Estimation via 2SLS. In columns 1-7, robust standard errors clustered by country reported in parentheses. *, **, *** = significant at 10%, 5%, and 1% respectively. In column 8, robust standard errors clustered at alternate levels: two-way by country and year (in parentheses), region x year (in brackets, []), and year (in braces, {}). Recipient characteristics include: log GDP per capita, GDP per capita growth, log population, UNSC member, U.S. ally, and U.S. exports. These coefficients, country fixed and year fixed effects (where relevant), and a constant are not reported.

B2. Additional controls

Additional recipient characteristics. While the baseline specifications control for a parsimonious set of variables that affect both political rights and the allocation of aid, there may be additional factors, such as a country's underlying political institutions, unearned income, and security apparatus that may mediate the effect of U.S. on political rights. *These variables are not included in the baseline specification as they are highly endogenous with political rights.*

Table B2 shows that the core finding is robust to the inclusion of these additional variables. Column 1, for instance, demonstrates that countries where the political leader faces greater constraints on her power tends to be less repressive, while column 2 shows that more “durable” or longer lasting regimes (which are typically autocratic) are more repressive.¹ These effects are consistent with existing studies (e.g., Bueno de Mesquita et al 2003). While both a country's executive constraints and regime durability tend to “mediate” the effect of aid on political rights (since the coefficient estimate on aid is smaller in magnitude than the baseline results in table 2), the effect of aid on political rights remains statistically significant. In column 1, the p-value on aid is 0.11 (just barely missing statistical significance at conventional thresholds).

The effect of aid on political rights is also robust in specifications that control for oil exports (column 3), arm imports (column 4), civil conflict (column 5), and trade openness (column 6). *OIL EXPORTS* measure a country's domestic rents (which can finance a more robust security apparatus), while a country's *ARMS IMPORTS* captures the capacity of a government to effectively suppress its population. While several studies find that these variables are often correlated with state repression, they are statistically insignificant in these specifications. Instrumented aid remains positive and statistically significant, despite the smaller estimating samples (due to missing country-year observations in oil exports and arms imports). The main results hold while controlling for whether a country is experiencing civil conflict (column 6). This is noteworthy, since conflict is often viewed as an intensification of repression (Besley and Persson 2011). Finally, in column 6, the inclusion of trade openness (sum of a country's exports and imports divided by GDP) accounts for the potential association between aid receipts and total trade flows (rather than exclusively U.S. exports to aid recipients) and direct effect trade

¹ The measure of executive constraints and regime durability are drawn from the POLITY data set (Marshall and Jaggers 2010).

flows may have on governance. In this specification, trade openness does not affect *POLITICAL RIGHTS*.

Foreign born population. Milner and Tingley (2010) argue that the percentage of foreign-born voters in a legislator's district can affect her vote decisions on foreign aid bills. And to the extent that immigration to the United States is correlated with a country's quality of political rights, failure to account for emigration may bias the results. Using data from Milner and Tingley (2010), I account for this by controlling for the average House district share (percentage) of foreign-born voters in each legislative session across all districts (column 7) and separately for Democratic and Republican districts (column 8).² In both specifications, while the average measure of district 'migrant voter-stock' is negatively correlated with repression, instrumented aid continues to exhibit a robust, positive effect on *POLITICAL RIGHTS*. Excluding this measure of migrant voter-stock therefore does not seem to constitute omitted variable bias.

Moreover, there is scant evidence to suggest that emigration to the United States is correlated with a "sending" country's quality of political rights. Using bilateral migration data from the World Bank (based on detailed analysis of census data), there is a low correlation between a sending country's migrant stock in the US and its level of *POLITICAL RIGHTS*. For example, using data from the 1990 and 2000 census, the correlation is -0.08. This low correlation suggests that political rights are unlikely to be a key determinant of a country's outward flow of migrants to the US in any meaningful way.

² Milner and Tingley (2010) provide data for the period 1976-2004. The measures of "percent foreign born" vary across years (but by recipient countries), thus specifications do not include year fixed effects but do control for $FRAG_t$.

Table B2: Impact of U.S. economic aid on political rights, with additional controls

Dependent variable:	Political rights							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log U.S. aid (2000 US\$)	0.101 (0.063) [^]	0.132 (0.072)*	0.165 (0.099)*	0.269 (0.130)**	0.157 (0.059)***	0.14 (0.055)**	0.139 (0.058)**	0.14 (0.058)**
<i>Additional controls</i>								
Exec. constraints	-0.599 (0.044)***							
Regime durability		0.029 (0.010)***						
Oil exports			-0.077 (0.145)					
Arms imports				0.021 (0.015)				
Civil conflict					0.296 (0.172)*			
Trade openness						-0.001 (0.003)		
Pct foreign born							-18.027 (5.116)***	
Pct foreign born in Rep. districts								-11.188 (6.654)*
Pct foreign born in Dem. districts								-7.802 (4.653)*
<i>FRAG_t</i>							-0.003 (0.004)	-0.001 (0.004)
Recip. Characteristics	Y	Y	Y	Y	Y	Y	Y	Y
Country FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	N	N
No. observations	3201	3352	2686	2090	3853	3739	2887	2887
R-squared	0.8	0.64	0.65	0.51	0.64	0.66	0.69	0.69

Notes: Estimation via 2SLS. Robust standard errors, clustered by country reported in parentheses. [^], *, **, *** = significant at 11%, 10%, 5%, and 1% respectively. Recipient characteristics include: log GDP per capita, GDP per capita growth, log population, UNSC member, U.S. ally, and U.S. exports. These coefficients, country and year fixed effects, and a constant are not reported. “Pct foreign born” is the average percentage of foreign born residents in each U.S. Congressional district (for each legislative year).

B3. Alternate instruments using different measures of P_i and $FRAG_t$

Alternate measure of legislative fragmentation. Table B3 provides evidence that the baseline results also hold in specifications that instrument for U.S. aid using the legislative fragmentation from the U.S. Senate derived from the absolute difference in number of Democratic and Republican senators. Specifically, Senate fragmentation is equal to $\left(1 - \frac{|DEMOCRATS-REPUBLICAN|}{100}\right) \times 100$.

Panel A in Table B3 shows the 2nd stage regression. The results in panel A provide evidence from a variety of specifications that U.S. aid (instrumented with Senate $FRAG_t$) has a positive and significant effect on *POLITICAL RIGHTS*. All the specifications include the baseline recipient characteristics, but vary the fixed effects. The specification in column 1, for instance, does not include any country and year fixed effects, but does appropriately include the constituent terms of the instrumental variable in both the first and second stage regressions (i.e., *Senate $FRAG_t$* and P_i).

Panel B reports first stage regression corresponding to each specification in Panel A. Similar to the effects in table 2 the coefficient on the instrument is positive and highly statistically significant. Across all the specifications, the instrumental variable is “strong” as the F -statistic exceeds the threshold for weak instruments of 9.6 as suggested by Stock et al (2002). These instruments, however, tend to be less powerful (i.e., lower F -statistic) than those using the measure of legislative fragmentation from the U.S. House of Representatives (see table 2) which exhibits greater temporal variation (i.e., each of the 435 House members face elections every year, compared to one-third of Senators) and thus yields greater precision. As a consequence, I use the measure of legislative fragmentation associated with the House of Representatives as the preferred variable in constructing the instrumental variable throughout this article.

Table B3: Impact of U.S. aid on political rights using fragmentation in the US Senate

Panel A: Second stage regression				
Dependent variable:	Political rights			
	(1)	(2)	(3)	(4)
Log U.S. aid (2000 US\$)	0.248	0.208	0.169	0.192
	(0.132)*	(0.124)*	(0.090)*	(0.093)**
Prob. of receiving U.S aid (P_i)	-3.104	-3.04		
	(1.579)*	(1.556)*		
Senate fragmentation	-0.076		-0.013	
	(0.021)***		(0.006)*	
R-squared	.	.	0.61	0.59
Panel B: First stage regression				
Dependent variable:	Log U.S. bilateral aid (2000 US\$)			
P_i x Senate fragmentation	-0.254	-0.277	-0.289	-0.278
	(0.074)***	(0.082)***	(0.078)***	(0.082)***
F-statistic	11.87	11.56	13.42	11.60
R-squared	0.40	0.46	0.63	0.65
<i>Controls in both panels</i>				
Recipient characteristics	Y	Y	Y	Y
Country fixed effects	N	N	Y	Y
Year fixed effects	N	Y	N	Y
Number of observations	3853	3853	3853	3853
Number of countries	151	151	151	151

Notes: Estimation via 2SLS. Robust standard errors, clustered by country reported in parentheses. * = significant at 10%; ** = significant at 5%; *** = significant at 1%. Recipient characteristics include: log GDP per capita, GDP per capita growth, log population, UNSC member, U.S. ally, and U.S. exports. These coefficients, country fixed and year fixed effects (for the appropriate specification), and a constant are not reported.

Alternate measures of the “propagation” mechanism, P_i . Skeptics may still be concerned that use of P_i in the identification strategy is problematic. As discussed in the main text, one such concern is that the probability a country receives U.S. aid is not time-invariant, but does change over time. This changing probability may reflect changes in U.S. domestic politics and changes in foreign policy objectives (Fleck and Kilby 2006) due in large part to evolving geopolitical conditions. To account for this possibility, I use alternate measures of P_i in the construction of the instrumental variable.

I first create a time-varying measure of P_i that captures the changing geopolitical objectives of U.S. foreign aid (and U.S. foreign aid policy more broadly). In particular, I calculate the average probability a country receives U.S. aid over 4 distinct geopolitical periods since 1972. The first is the period of détente (1972-1980) in which the superpower rivalry between the United States and the USSR was relatively calm. The election of Ronald Reagan in November 1980 marked a transition in the superpower rivalry. This second period (1981-1990)

had two main features: an expansion of U.S. defense spending under President Reagan and Mikhail Gorbachev's move a gradual opening (glasnost) and restructuring (perestroika) of the USSR. The end of the Cold War in 1989 bought an end to the superpower rivalry and ushered a re-orientation of U.S. foreign policy. For example, under President Clinton in the 1990s U.S. defense spending declined. The third period captures the post-Cold War period until the terrorist events of September 11, 2001 (1991-2001). The fourth period is the post-9/11 period (2002-2009) in which U.S. foreign policy (and foreign aid) objectives changed once again.

Column 1 in table B4 reports the first and second stage estimates from a specification with this time-varying probability of receiving aid ($P_{time-vary}$). In panel A, instrumented U.S. aid has a positive and statistically significant effect on political rights. The estimated effect is twice as large as the baseline estimate in table 2. The corresponding first-stage regression is reported in panel B, column 1. The instrument, which interacts $FRAG_t$ with $P_{time-vary}$ has a positive and highly statistically significant effect on U.S. aid. This positive effect is consistent with the first-stage effect associated with the baseline results in table 2. Moreover, the instrument remains “strong.” Finally, and unsurprisingly, countries that receive aid more frequently (i.e., higher value of $P_{time-vary}$) tend to receive higher amounts of aid on average.

I also construct a set of alternate instruments which interact $FRAG_t$ with a dummy variables equal to 1 if a country received any U.S. aid in the past 1, 2, and 5 years (and zero otherwise). These variables are denoted as $P_{i,t-1}$, $P_{i,t-2}$, and $P_{i,t-5}$ respectively. Columns 2-4 in table B4 report the second (in panel A) and first (in panel B) stage estimates from these specifications. Across all 3 specifications, instrumented U.S. aid has a positive and significant effect on political rights. In the first stage, all 3 alternate instrumental variables are strong determinants of U.S. aid disbursements and are “strong” instruments (since the corresponding F -statistics exceed 9.6). Moreover, as expected, countries that received U.S. aid in the recent past are more likely to get higher levels of contemporaneous aid. The estimated effects on $P_{i,t-1}$, $P_{i,t-2}$, and $P_{i,t-5}$ decrease as the temporal range increases.

Table B4: Impact of U.S. aid on political rights using alternate measures of P_i

	(1)	(2)	(3)	(4)
Panel A: Second stage regression (2SLS)				
Dependent variable:	Political rights			
<i>Instrumented</i>				
Log U.S. econ. aid (2000 US\$)	0.368 (0.166)**	0.299 (0.113)***	0.242 (0.097)**	0.123 (0.057)**
<i>Non-instrumented</i>				
$P_{i,time-vary}$	-4.471 (1.944)**			
Receives aid in year $t-1$		-2.551 (0.997)**		
Receives aid in year $t-2$			-1.582 (0.667)**	
Receives aid in year $t-5$				-0.231 (0.190)
R-squared	0.41	0.53	0.57	0.68
Panel B: First stage regression (OLS)				
Dependent variable:	Log U.S. economic aid (2000 US\$)			
<i>Instrumental variables</i>				
$FRAG_t \times P_{i,time-vary}$	-0.104 (0.034)***			
$FRAG_t \times$ Rec. aid in $t-1$		-0.097 (0.025)***		
$FRAG_t \times$ Rec. aid in $t-2$			-0.12 (0.030)***	
$FRAG_t \times$ Rec. aid in $t-5$				-0.175 (0.036)***
<i>Constituent term</i>				
$P_{i,time-vary}$	19.969 (2.757)***			
Rec. aid in $t-1$		16.634 (2.110)***		
Rec. aid in $t-2$			16.412 (2.529)***	
Rec. aid in $t-5$				16.674 (3.273)***
R-squared	0.76	0.78	0.74	0.68
F-statistic on instrument	9.28	14.7	15.71	22.89
Additional controls in all both panels				
Recipient characteristics	Y	Y	Y	Y
Country and year fixed effects	Y	Y	Y	Y
Number of observations	3853	3750	3712	3587
Number of countries	151	151	151	150

Notes: Estimation via 2SLS in panel A and OLS in panel B. Robust standard errors, clustered at the country level reported in parentheses. *, ***, *** = signif at 10%, 5%, and 1% respectively. $P_{i,time-vary}$ is the probability a country receives U.S. aid during the following periods: detente (1972-1980), Reagan (1981-1990), post-Cold War (1991-2001), post-9/11 (2002-2009). Rec. aid in $t-j$ is an indicator variable to 1 if a country received any U.S. aid in year $t-j$ ($j=1,2,5$) and zero otherwise. Recipient characteristics include: log GDP per capita (2000 US\$), GDP per capita growth (% annual), log population, UN Security Council membership, US military ally, log US exports (2000 US\$). These coefficients, country fixed effects, year fixed effects and a constant are not reported.

Appendix B4. Alternate samples and the potential “crowding out” of aid

Accounting for potential outliers. Table B5 shows that neither countries that receive high levels of U.S. aid nor are the most frequent aid recipients unduly influence the main findings. In column 1, the estimating sample *excludes* country-year observations from the top decile of U.S. receipts (in aggregate dollars), while the specification in column 2 is restricted to a sample of countries that receive U.S. aid more than 90 percent of the time, such as Egypt and Israel (i.e., $P_i > 0.90$). In these “trimmed” samples the estimated effect of instrumented U.S. aid on political rights is quite similar in magnitude (and statistical significance) to the effect reported in table 2.

The confluence of country size and aid receipts introduces an additional potential source of “influential” observations. Specifically, the costs of repression are often lower in less populated countries and these countries tend to receive disproportionately higher amounts of aid (Alesina and Dollar 2000). Thus, the findings may be overly influenced by the presence of small countries in the estimating sample. To allay this worry, I estimate the effect of instrumented U.S. aid on political rights in samples that exclude countries with populations less than 2 and 5 million people respectively. Doing so significantly reduces the estimating sample. In column 3, countries with populations less than 2 million people are excluded, resulting in a sample that is 30 percent smaller than the full sample in table 3, columns 1 and 2. In column 4, nearly 50 percent of the full sample is dropped as it excludes countries with a population less than 5 million people. Despite these smaller estimating samples, the substantive effect that U.S. aid worsens political rights remains unchanged. Moreover, the coefficient estimates are quite larger than the baseline findings, suggesting that the inclusion of smaller countries actually attenuates the effect of U.S. aid on political rights.

Table B5: Impact of U.S. economic aid on political rights with alternate samples

Dependent variable:	Political rights			
	(1)	(2)	(3)	(4)
	<u>Outliers</u>		<u>Excl. low populations</u>	
Sample:	Excl. top decile of aid recipients	$P_i < 0.90$	Pop. < 2M	Pop. < 5M
Log U.S. bilateral aid (2000 US\$)	0.148 (0.069)**	0.188 (0.071)***	0.302 (0.152)**	0.345 (0.168)**
Recipient characteristics	Y	Y	Y	Y
Country fixed effects	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
R-squared	0.65	0.64	0.41	0.32
Number of observations	3463	2551	2734	1902
Number of countries	150	114	104	71

Notes: Estimation via 2SLS specifications. Robust standard errors, clustered at the country level. * = significant at 10%; ** = significant at 5%; *** = significant at 1%. All specifications include the baseline recipient characteristics, country, and year effects. These coefficients and a constant are not reported. In column 1, the top decile of U.S. bilateral aid recipients (by dollar amount) are excluded. In column 2, countries that receive U.S. bilateral aid more than 90 percent of the time are excluded. In columns 3 and 4, countries with populations less than 2 million and 5 million are excluded respectively.

Crowding out non-US aid. The interpretation of the main results is that U.S. foreign aid has a direct causal impact on *POLITICAL RIGHTS* in recipient countries. However, a possible alternative explanation is that U.S. foreign aid affects political rights by crowding out aid from other donors. For example, other donors may respond to an increase in U.S. foreign aid by reducing their own provisions. If these other forms of aid *reduce* political rights, then this form of “crowding out” can explain why U.S. aid deteriorates political rights. Conversely, U.S. aid could be negatively correlated with aid from other sources. And, the results could, instead, be explained by crowding out if U.S. aid reduces *POLITICAL RIGHTS*. In either instance, it is important to note that this does not undermine the validity of the estimated causal effects of U.S. aid on political rights; but the mechanism of crowding-out offers a potentially different channel than those discussed in section I.

To explore whether crowding out exists, I regress U.S. aid on total bilateral economic aid from *other* donors. The results are presented in table B6. In both the OLS and IV specifications, the results show that U.S. aid does not crowd out other donors. In particular, instrumented U.S. aid is uncorrelated with total non-US aid (i.e., the coefficient estimate of -0.004 is practically equal to zero). This null finding is reassuring as it validates the paper's underlying identification strategy that the legislative fragmentation of the U.S. Congress should influence U.S. aid, but should be uncorrelated with aid from other countries.

Table B6: Evaluating the existence of crowding out

Dependent variable:	Log non-U.S. economic aid (2000 US\$)	
	(1)	(2)
Method of estimation:	OLS	2SLS
Log U.S. economic aid (2000US\$)	0.021 (0.011)**	-0.004 (0.044)
Recipient characteristics	Y	Y
Country fixed effects	Y	Y
Year fixed effects	Y	Y
Number of observations	3801	3801
R-squared	0.77	0.77

Notes: Robust standard errors, clustered by country reported in parentheses. * = significant at 10%; ** = significant at 5%; *** = significant at 1%. Recipient characteristics include: log GDP per capita (2000 US\$), GDP per capita growth (% annual), log population, UNSC member, U.S. ally, and log U.S. exports (2000 US\$). These coefficients, country fixed effects, year fixed effects, and a constant are not reported.

Appendix B5. Accounting for unobserved temporal, spatial, and country-specific factors

The core finding is also robust in specifications that control for a number of potentially unobservable temporal (e.g. geopolitics), geographic (spatial), and country-specific “contextual” factors (e.g., democratization). These results are discussed below and presented in tables B7 and B8.

Accounting for geopolitics. As discussed in the main text, *time-varying* geopolitical considerations often shape the US government’s aid allocation decisions and its broader foreign policy goals. These considerations are largely “unobservable” but can be associated with distinct historic periods (e.g., Cold War period, post-9/11 period). The baseline results (from table 2) also hold in specifications that capture unobserved temporal trends, such as the superpower rivalry between the United States and the USSR during the Cold War, which frequently shaped each state’s foreign policies and aid disbursements with developing countries (e.g., Dunning 2004).³ More recently, concerns with global terrorism have prompted the U.S. to increase aid to certain countries and regions.

Table B7 presents results that account for these geopolitical “temporal factors” in a number of ways.⁴ For instance, the specification in column 1 controls for the specific effects of the Cold War and post-9/11 “epochs.” The Cold War epoch dummy is equal to 1 prior to 1990 and zero thereafter, while the post-9/11 epoch dummy is equal to 0 prior to 2001 and coded as 1 from 2002 onwards. In this specification, instrumented U.S. aid continues to harm political rights, as demonstrated with the positive and statistically significant effect. The coefficient estimate (=0.157) is nearly identical in magnitude to the main results in table 2. The coefficient on the Cold War dummy is positive and statistically significant (but only at the 10% level). This finding is consistent with the general trend that countries tended to be more repressive during the Cold War period and that many started to democratize in the post Cold War period. The negative

³ The Cold War rivalry often meant the U.S. supported highly repressive political regimes (e.g., dictatorships in Greece, Spain, Iran, Cuba, Haiti, Guatemala, Pakistan) and the United States often pursued different foreign policies in different geographic regions (e.g., promoting democracy in Western Europe, stalling liberalization in the Middle East).

⁴ While the core results (in tables 2 and 3) include year fixed effects (and thus account for observable and unobservable factors that *vary annually*, but *affect all aid recipients*), the specifications in table D7 control for temporal factors associated with specific time periods and their effects that may vary across region and/or by individual countries.

coefficient and statistically significant on the post-9/11 dummy partially identifies this post Cold War democratization trend.

The positive coefficient on the Cold War dummy suggests that conditions during the Cold War may have made countries more conducive to inferior political rights. To investigate and account for this more systematically, I estimate specifications that interact region and country fixed effects with a dummy for the Cold War period (columns 2 and 3). These interaction terms control for the differential effect of Cold War politics across each region and for *each* country.

In the specification that controls for the interaction of *COLD WAR_t* with *REGION_i* fixed effects (column 2), instrumented aid continues to exhibit a robust, positive effect on *POLITICAL RIGHTS*. While these regional interaction terms tend to be statistically significant in isolation, their combined effect with the “main term” tends not to be.

In column 3, the core result for instrumented aid also holds in a specification that controls for the interaction of the Cold War dummy with each country fixed effect (so 151 additional *COUNTRY* x *COLD WAR* interaction terms). These country trends are a means to account for how specific countries were “treated” by the superpowers between the Cold War and post-Cold War periods. Finally, in a sample restricted to the post-Cold War period, U.S. aid continues to exhibit a robust, positive effect on *POLITICAL RIGHTS* (column 4). This specification *directly* purges concerns that Cold War dynamics may be unduly driving the results.⁵

⁵ Note, specification (4) in table B6 controls for region fixed effects, while specification (2a) in table 2 does not.

Table B7: Accounting for temporal factors

Dependent variable:	Political rights							
	(1)		Region FE x CW		Country FE x CW		Post CW sample	
	Coeff.	SE	Coeff.	SE	Coeff.	SE	Coeff.	SE
Log U.S. aid	0.157	(0.059)***	0.114	(0.042)***	0.081	(0.038)**	0.081	(0.041)**
Cold War (CW)	0.808	(0.476)*	5.150	(0.779)***	3.551	(0.372)***		
Post 9/11 period	-1.436	(0.406)***						
Asia x CW			-4.895	(0.729)***				
Africa x CW			-4.014	(0.595)***				
Mid.East x CW			-5.339	(0.729)***				
N.Amer x CW			-4.762	(0.652)***				
S.Amer x CW			-4.124	(0.661)***				
Europe x CW			-4.681	(0.734)***				
Asia			-1.492	(3.942)			-0.579	(1.856)
Africa			-3.269	(2.172)			-1.499	(3.400)
Mid.East			2.784	(1.603)*			4.618	(3.911)
N.Amer			-2.160	(2.860)			0	
S.Amer			2.383	(2.820)			-1.384	(1.474)
Europe			-0.975	(0.595)			0.217	(3.985)
Rec. characteristics	Y		Y		-	Y	Y	
Country and year FE	Y		Y			Y	Y	
Country FE x Cold War						Y		
Number of observations	3853		3853		3853		2325	
Number of countries	151		151		151		150	
R-squared	0.64		0.69		0.81		0.83	

Notes: Estimation via 2SLS. Robust standard errors, reported in parentheses. * = significant at 10%; ** = significant at 5%; *** = significant at 1%. Log US aid measured in 2000 US\$. All specifications control for the following recipient characteristics: log GDP per capita (% annual), GDP per capita growth (% annual), log population, log US exports, UNSC membership, US ally, country and year fixed effects. These coefficients and a constant are not reported. In column 4, the sample is restricted to country-year observations in the post-Cold War period (1991-2008).

Accounting for unobserved spatial heterogeneity (“diffusion”). In addition to unobservable temporal factors, such as the role of geopolitics, unobserved spatial factors may affect a country’s level of *POLITICAL RIGHTS*. For instance, the existing literature on “political diffusion” suggests that rights-related behaviors may diffuse spatially through a number of channels. Governments may learn from their geographic or economic peers. Relatedly, prevailing regional economic (e.g., growth) and/or political (e.g., *POLITY* level) conditions may ‘spillover’ and affect politics.

I account for these forms of unobserved spatial heterogeneity by controlling for each geographic *region’s average* *POLITY* score, growth rate, and level of trade openness (all in the

prevailing year, t). In such a specification (reported in column 1 of table B8), the causal impact of instrumented U.S. aid on *POLITICAL RIGHTS* remains positive and statistically significant. In fact, the coefficient estimate ($=0.157$) is nearly identical to the core results in table 2. Countries in *regions* experiencing economic growth and improvements in democratic governance exhibit improvements in *POLITICAL RIGHTS*. This is evident from the negative (but statistically insignificant) coefficient estimates on *REGIONAL GROWTH* and *REGIONAL POLITY*. Surprisingly, greater regional economic integration (measured with the region's average level of trade openness) tends to harm a country's political rights.

Country-specific factors. Skeptics may worry further that the effect of aid on political rights is “conditional” on prevailing country-specific factors. For example, perhaps the effect of aid on political rights is conditional on whether the recipient is in the midst of a democratization process (as in Central Europe in the 1990s) or on whether the US relies on the country as an important regional ally (as in the Middle East and North Africa). I account for these country-specific factors in two ways.

First, in specification (2) in table B8, I control for a country's change in POLITY score (from the previous year). A positive value of this variable implies that the country is in the midst of a democratization process. In this specification, instrumented aid continues to exhibit a robust, positive effect on *POLITICAL RIGHTS*. The coefficient estimate ($=0.19$) is slightly larger than the core results in table 2 due to the slightly smaller sample size (due to missing country observations in the POLITY variable, i.e., only 128 countries).

Second, to account for a country's importance as a regional ally, I interact whether a country is a US military ally with that country's relevant region fixed effect. The coefficient estimates from this regression are reported in specification (3). While some of these interaction terms are statistically significant, instrumented U.S. continues to exhibit a robust and positive effect of *POLITICAL RIGHTS*. The coefficient estimate ($=0.157$) is near identical to the core results in table 2 (e.g., column 3)

Table B8: Accounting for spatial heterogeneity and country-specific factors

Dependent variable	Political rights					
	(1)		(2)		(3)	
	Coeff.	SE	Coeff.	SE	Coeff.	SE
Log US aid	0.157	(0.059)***	0.191	(0.093)**	0.157	(0.060)***
Regional POLITY	-0.617	(0.356)*				
Regional growth	-0.627	(3.713)				
Regional trade	0.046	(0.090)				
Annual change in POLITY			-0.001	(0.003)		
US ally	0.162	(0.214)	0.042	(0.259)	-0.794	(0.225)***
Asia x US ally					0.56	(0.674)
Africa x US ally					0	
Mid.East x US ally					0	
N.Amer x US ally					1.548	(0.117)***
S. Amer x US ally					0.944	(0.276)***
Europe x US ally					1.43	(0.533)***
Asia					-0.905	(4.125)
Africa					-4.910	(2.265)**
Mid.East					3.980	(1.375)***
N. America					0	
S. America					2.845	(2.984)
Europe					-1.672	(0.643)**
Recipient characteristics	Y		Y		Y	
Country and year FE	Y		Y		Y	
Number of observations	3853		3307		3853	
Number of countries	151		128		151	
R-squared	0.64		0.56		0.64	

Notes: Estimation via 2SLS. Robust standard errors, reported in parentheses. * = significant at 10%; ** = significant at 5%; *** = significant at 1%. Log US aid measured in 2000 US\$. All specifications control for the following recipient characteristics: log GDP per capita (% annual), GDP per capita growth (% annual), log population, log US exports, UNSC membership, US ally, country and year fixed effects. These coefficients and a constant are not reported.

APPENDIX C: ALTERNATE DEPENDENT VARIABLES

This appendix describes the alternate dependent variables corresponding to the empirical analysis in panel B of Table 3.

Alternative measures of POLITICAL RIGHTS. As an alternative measure of political rights, I also use the “competitiveness of political participation” (*PARTICIPATION*) index from POLITY (Marshall and Jaggers 2010). This 5-point index (1-5) gauges the “extent to which alternative preference for policy and leadership can be pursued in the political arena.” *Lower* index values of *PARTICIPATION* correspond to lower competitiveness. For example, *PARTICIPATION* values of 1 and 2 correspond to “repressed” and “suppressed” levels of political participation. “Repressed”, for instance, implies that the presence of “no significant oppositional activity.” “Suppressed” implies that the presence of “some” organized competition, but with “sharp limits in ways that exclude substantial groups (20% or more of the adult population) from participation.”

In addition to political participation, repression can be assessed on an individual’s autonomy from the state. To capture such autonomy, I use the *CIVIL LIBERTIES* index created by Freedom House. Freedom House defines civil liberties to entail the “freedom to develop views, institutions, and personal autonomy from the state.” The country experts at Freedom House also consider the protection of minorities and the protection of freedom of expression in their annual assessments of each country’s civil liberties. Like *POLITICAL RIGHTS*, *CIVIL LIBERTIES* is a 7-point index (1-7) where *higher* values correspond to *less* autonomy from the state.

Actual violations of human rights. A potential drawback to gauging repression using *POLITICAL RIGHTS*, *CIVIL LIBERTIES*, and *PARTICIPATION* is that these assessments are based on *perceptions* by third-party country experts, rather than on actual, quantifiable measures of state repression. To mitigate this concern, I leverage the CIRI Human Rights data set, which draws on annual reports from U.S. State Department and Amnesty International that describe *actual* violations of human rights (Cingranelli and Richards 2008). (As discussed in the main text, the CIRI data may be biased in favor of giving U.S. allies more favorable evaluations, see Qian and Yanagizawa 2009). This data contains standard-based quantitative information on government respect for 15 internationally recognized human rights for up to 195 countries, on an annual basis

since 1981. As Cingranelli and Richards note, the data set is designed for use by scholars and students who seek to test theories about the causes and consequences of human rights violations, as well as policy makers and analysts who seek to estimate the human rights effects of a wide variety of institutional changes and public policies including democratization, economic aid, military aid, structural adjustment, and humanitarian intervention. In particular, I examine three different dimensions of government repression: religious freedom, empowerment rights, and political imprisonment.

Religious freedom indicates the extent to which the freedom of citizens to exercise and practice their religious beliefs is subject to actual government restrictions. Rather than providing raw counts of actual violations of religious freedoms, the data is grouped into a 3-point categorical variable: widespread violation (0), some violations (1), and no violations (2).⁶ A lower value, therefore, implies greater restrictions on religious freedom.

Empowerment rights, in contrast, measures whether a government respects seven distinct human rights, such as freedom of speech, workers' rights, and freedom of movement. More specifically, empowerment rights adds the Foreign Movement, Domestic Movement, Freedom of Speech, Freedom of Assembly and Association, Workers' Rights, Electoral Self-Determination, and Freedom of Religion indicators in CIRI (see CIRI for documentation). Empowerment right ranges from 0 (no government respect for these seven rights) to 14 (full government respect for these seven rights).

Finally, political imprisonment refers to the incarceration of people by government officials because of their beliefs. In particular, political imprisonment refers to the incarceration of people because of their speech; their non-violent opposition to government policies or leaders; their religious beliefs; their non-violent religious practices including proselytizing; or their membership in a group, including an ethnic or racial group. Like religious freedom, political imprisonment is coded on a 3 point scale, where a lower value implies a higher number of imprisoned individuals. Specifically, a score of 0 indicates that there were many people (more than 50) imprisoned because of their religious, political, or other beliefs in a given year; a score of 1 indicates that a few people were imprisoned (1-49); and a score of 2 indicates that no persons (0) were imprisoned for any of the above reasons in a given year.

⁶ See Cingranelli and Richards (2008) for a justification/explanation why the counts are clustered into 3 categories.

APPENDIX D: U.S. AID FOSTERS AUTHORITARIAN DURABILITY

The ability and willingness for governments to deteriorate political rights is feasible (and rational) if the aid inflows help make the country's underlying political regime, institutions, and rules more stable. The analysis presented in this appendix shows that U.S. aid tends to strengthen authoritarian politics.

To evaluate whether U.S. aid strengthens authoritarianism, I use 3 well-known variables from the POLITY data set (Marshall and Jaggers 2010). The first is the executive constraints index (*XCONST*) which measures the extent of institutionalized constraints on the decision-making powers of chief executives, whether are individuals or collectives. *XCONST* lies on a 7-point (1-7) scale, where lower values correspond to a reduction in constraints (i.e., an aggrandizement of executive control) and usually implies a decline in a country's democratic governance. The second is the broader *POLITY* index (-10 to +10) where higher values correspond to higher quality democratic institutions. The third, *DURABLE*, is a measure of regime durability. If a country experiences at least a 3-point change in its *POLITY* score from the previous year, *DURABLE* is reset to zero. If there is no such 3-point change, *DURABLE* increases by one point.

Table D1 presents evidence that U.S. aid strengthens authoritarian politics. In column 1, a one standard deviation increase in U.S. aid lowers *XCONST* by 0.75 index point, which is equal to about a one-third standard deviation change in *XCONST*. The specification in column 2 substantiates this, as a one standard deviation increase in U.S. aid lowers *POLITY* by 3 index points (or roughly 0.43 standard deviation of *POLITY*). Instrumented U.S. also tends to make a country's existing political regime (institutions and rules) less prone to rapid changes and overall more durable in both authoritarian and democratic regimes (column 3). Yet the effects are not uniform across regime type. In countries that tend to have autocratic politics over the sample period, the effect of instrumented U.S. aid on *DURABLE* is more than twice as large as the effect in democratic polities (columns 4 and 5). This differential effect is consistent with the notion that repression can be a prudent strategy for regime stability in autocracies (Smith 2008).

Table D1: U.S. aid fosters authoritarian stability

Dependent variable:	<i>XCONST</i>	<i>POLITY</i>	<i>DURABLE</i>		
			<u>All countries</u>	<u>Autocracies</u>	<u>Democracies</u>
	(1)	(2)	(3)	(4)	(5)
Log U.S. aid (2000 US\$)	-0.126 (0.073)*	-0.503 (0.253)**	1.833 (0.908)**	2.627 (1.490)*	1.236 (0.554)**
Recipient characteristics	Y	Y	Y	Y	Y
Country fixed effects	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y
Number of observations	3225	3341	3376	2050	1326
R-squared	0.66	0.68	0.56	0.39	0.80
Number of countries	129	129	130	74	56

Notes: Estimation via 2SLS. Robust standard errors, clustered by country reported in parentheses. * = significant at 10%; ** = significant at 5%; *** = significant at 1%. Recipient characteristics include: log GDP per capita, GDP per capita growth, log population, UNSC member, U.S. ally, and U.S. exports. These coefficients, country fixed and year fixed effects, and a constant are not reported. In columns (4) and (5) the sample is restricted to autocracies and democracies respectively.