Political Aid Cycles

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Web Appendix

This Appendix examines the robustness of the results presented in the main text along two dimensions. Section A.A1 explores potential mis-specification issues arising due to the granular timing of aid flows and elections, and Section A.A2 examines the sensitivity of the results to methods for limiting the influence of outliers.

A1. Timing Concerns

One limitation of our data is that we observe both elections and aid flows at the annual level. This raises a question whether the relationship between aid and UN alignment during election years is driven by aid before or after the election itself takes place. The Palestinian anecdote cited in the Introduction and our own conversations with former USAID officials suggest that donors can implement new aid packages fairly quickly before an election if they desire, but it is desirable to examine the issue more systematically.

Specifications Exploiting Election Timing. One way we can examine this issue is to exploit information on the timing of elections, which is available for 268 out of the 274 elections in our sample. If aid cycles were driven by changes in aid *following* elections we would expect to see stronger effects for earlier elections, while our influence hypothesis predicts stronger effects in later elections. Table A.1 reports results differentiated by election timing. In the first column we define an election as "early" if it took place in the first six months of the year (the median election takes place in July). We estimate larger aid cycles for elections that take place later in the year, but economically and statistically significant cycles around early elections as well. In the next column we redefine "early" to mean taking place in the first three months of the year; using that definition we again estimate a large and significant effect of later elections but now estimate a small and insignificant cycle for early elections.

Specifications Using Aid Disbursements. Our primary estimates of political aid cycles use aid commitments as the outcome variable, but we could alternatively have used aid disbursements, which are recorded separately in the DAC database. The quantitative gap between commitments and disbursements is not large in our sample (mean disbursements are $50 million, as opposed to mean commitments of $57 million), but the difference in the timing of commitments and disbursements could be important depending on the nature of political

aid cycles. If one believes that donors are signalling support for an incumbent then commitments are appropriate (especially since we know that they are not binding). If, on the other hand, one thinks that fiscal policy is the key mechanism then either commitments or disbursements might be appropriate depending on the incumbent's ability to spend future income. To examine this issue we re-ran our main estimators using aid disbursements rather than commitments as the outcome variable. The results are reported in Tables A.2 and A.3. Estimated political cycles in aid disbursements are smaller than estimated cycles in commitments, but they remain economically and statistically significant.

A2. Robustness to Outliers

Table A.4 presents summary statistics on ODA by recipient for our sample. As is well-known this distribution is skewed, with politically important countries receiving disproportionate shares of total ODA. Given this skewness, one important question is how sensitive the results are to outliers.

Dropping Individual Observations. A simple way to define "outliers" is to identify individual outlier observations. Figure A1 plots the residual values of ODA and UN alignment, separately for election and non-election years, after taking out donor-recipient means. The three data points identified in red are obvious outliers (these are US aid to Egypt in 1990 and 1991 and Japanese aid to Pakistan in 2003). To verify that our results are not driven by these observations we re-ran our main specification including a separate indicator variable for each of these three observations, effectively dropping them from the regression. Table A.5 reports the results; dropping these outliers reduces the estimated magnitude of aid cycles but they remain economically and statistically significant.

Dropping Large Recipients. An alternative notion of robustness is sensitivity to large aid recipients. Tables A.6 and A.7 report estimates of our main specifications for samples that exclude the largest 3 recipients (Egypt, Indonesia, India) and largest 5 recipients (Egypt, Indonesia, India, Israel, China) in our sample, respectively. The estimates in Table A.6 are smaller in magnitude but qualitatively similar and in fact more strongly significant than baseline estimates. Estimates in Table A.7 are again somewhat smaller in magnitude but remain qualitatively consistent with baseline estimates and statistically significant (in some cases at the 10 percent level).[1]

Estimates Using Ln(ODA) as the Dependent Variable. An alternative way to reduce the weight given to larger aid recipients is to estimates specifications using the logarithm of ODA as the dependent variable. A priori it is unclear whether estimation in logs or in levels is more appropriate. Suppose the optimal amount of aid given to recipient r during "normal" years is n_r while the magnitude of the "adjustment" in aid flows a donor wishes to make prior to an election in

[1]We also estimated models dropping each individual recipient one-at-a-time. None of the exclusions has a material effect on the results except excluding Egypt; excluding Egypt yields estimates similar to those in Table A.6.

recipient r is e_r. If cross-recipient variation in these figures is proportionate – i.e. if $e_r = kn_r$ for some constant k – then a logarithmic specification is appropriate. On the other hand if e_r is independent of n_r then a linear model is appropriate. It is thus an empirical question which specification better fits the data.

An additional issue that arises when fitting models for log ODA is that aid flows are strictly positive for only 90 percent of the donor-recipient-year observations in our sample. Following Kuziemko & Werker (2006) we set ODA to \$1 for the remaining observations and then take natural logarithms. Table A.8 presents results, replicating the specifications used in Tables A.6 and A.7. In general the estimates are consistent in sign and order of magnitude with those in levels. While they are less precisely estimated, we do obtain significant results when we isolate the role of bilateral UN alignment in Column V and perhaps most importantly when we focus on elections after the 3rd month of the year in Column IX. (Recall from Table A.1 that we obtain significant results in levels only for these elections.)

Table A.1—Political Aid Cycles Differentiated by Election Timing

Regressor	Early = First 6 Months	Early = First 3 Months
Early Election	-13.189	-1.771
	(1.582)***	(8.673)
UN * Early Election	25.946	3.316
	(0.000)***	(14.401)
Late Election	-41.154	-34.746
	(14.828)***	(10.593)***
UN * Late Election	66.942	58.400
	(29.595)**	(20.865)***
UN Alignment	-31.402	-31.396
	(21.384)	(21.493)
N	14435	14435
R^2	0.001	0.001

Robust standard errors in parenthesis are multi-way clustered by donor, recipient and year. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.2—Effect of Recipient Country Executive Elections on ODA (Disbursements)

Regressor	I	II	III	IV	V	VI	VII	VIII	IX
Exec. Election	3.827	3.827	2.732	-15.473	-8.704	-12.397	9.105	9.105	12.685
	(2.109)*	(2.088)*	(2.490)	(7.867)**	(5.539)	(5.832)**	(12.079)	(13.347)	(11.555)
UN * Election				29.729	19.353	23.750			
				(13.659)**	(10.071)*	(11.019)**			
UN Avg. * Election							-8.226	-8.226	-14.650
							(21.039)	(21.523)	(19.079)
UN Residual * Election							37.036	22.601	30.374
							(17.290)**	(13.504)*	(14.894)**
UN Agreement				-13.427	-16.185	-67.478			
				(12.817)	(20.814)	(37.874)*			
UN Donor Avg.							17.403	17.403	-44.025
							(17.037)	(20.984)	(29.385)
UN Residual							-39.030	-94.853	-116.768
							(10.246)***	(37.553)**	(55.327)**
Population			-0.310			-0.316			-0.305
			(0.286)			(0.291)			(0.287)
GDP			0.218			0.225			0.224
			(0.000)***			(0.000)***			(0.000)***
Population (Donor)			-2.947			-3.301			-3.548
			(0.782)***			(0.783)***			(0.762)***
GDP (Donor)			0.034			0.037			0.039
			(0.009)***			(0.008)***			(0.008)***
Fixed Effects	DR,Y	DR,DY	DR	DR,Y	DR,DY	DR	DR,Y	DR,DY	DR
N	15315	15315	13495	15315	15315	13495	15315	15315	13495
R^2	0.008	0.028	0.012	0.009	0.028	0.014	0.009	0.029	0.015

Robust standard errors in parenthesis are multi-way clustered by donor, recipient and year. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

TABLE A.3—EFFECT OF THE COMPETITIVENESS OF THE ELECTION ON POLITICAL AID CYCLES (DISBURSEMENTS)

Regressor	I	II	III	IV	V	VI
UN Agreement	-3.797	11.001	-24.943	-13.988	-38.042	-87.674
	(19.746)	(24.045)	(25.793)	(13.841)	(22.882)*	(41.445)**
Noncompetitive	1.481	-2.260	-0.037	-4.929	-36.378	-27.382
	(5.598)	(7.256)	(7.047)	(14.613)	(2.576)***	(8.334)***
UN * Noncompetitive	-5.888	0.058	-7.563	-8.907	41.302	26.534
	(9.752)	(11.710)	(11.128)	(18.690)	(7.508)***	(14.180)*
Competitive Election	-14.416	-8.738	-11.079	-20.086	-10.725	-17.909
	(7.159)**	(5.179)*	(7.083)	(11.227)*	(8.329)	(8.864)**
UN * Competitive Election	23.775	15.318	17.672	38.174	24.418	32.921
	(11.403)**	(9.127)*	(10.702)*	(19.102)**	(14.145)*	(15.646)**
Noncompetitive Election	-4.799	-3.629	-6.935	-5.314	-1.016	0.782
	(5.955)	(4.548)	(6.859)	(4.314)	(3.169)	(5.533)
UN * Noncompetitive Election	7.336	5.419	6.241	11.829	4.636	1.950
	(7.912)	(6.876)	(8.603)	(6.094)*	(4.486)	(7.776)
FEs	DR,Y	DR,DY	DR	DR,Y	DR,DY	DR
Controls	-	-	Yes	-	-	Yes
N	3310	3310	3005	15305	15305	13490
R^2	0.039	0.102	0.017	0.010	0.030	0.016

All columns include recipient fixed effects. Columns I-III use a non-competitiveness indicator derived from vote shares; Columns IV-VI use an EIEC-based indicator. Columns II and V include year fixed effects; Columns III and VI include recipient specific trends. Robust standard errors in parenthesis are multi-way clustered by donor, recipient and year. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.4—ODA by Recipient

Recip.	Total ODA	Mean ODA	Recip.	Total ODA	Mean ODA	Recip.	Total ODA	Mean ODA
EGY	88,791	592	NIC	5,800	39	GUY	1,417	9
IDN	62,303	415	BFA	5,583	37	PNG	1,404	9
IND	55,391	369	UGA	5,274	35	ALB	1,260	15
ISR	46,502	423	ZWE	5,164	41	AZE	1,050	17
CHN	45,945	353	HTI	5,019	33	SWZ	1,046	7
PAK	33,704	225	YEM	4,610	31	NAM	998	14
PHL	32,842	219	JAM	4,481	30	CPV	948	7
BGD	29,857	199	SYR	4,390	29	MKD	898	15
THA	26,370	176	GTM	4,373	29	VEN	861	6
TUR	20,039	134	GIN	4,192	28	SGP	854	8
MAR	17,360	116	COG	4,066	27	URY	809	5
LKA	13,931	93	RWA	4,065	27	FJI	806	5
CIV	13,820	92	NGA	3,988	27	PRK	712	10
VNM	13,247	95	ECU	3,920	26	CYP	706	6
KEN	12,810	85	CRI	3,829	26	ERI	677	12
TZA	12,241	82	GAB	3,809	25	SAU	622	4
PER	12,195	81	BEN	3,516	23	HRV	573	9
MYS	12,171	81	CHL	3,377	23	OMN	553	4
CMR	11,701	78	TGO	3,374	22	BLZ	537	5
JOR	11,634	78	PRY	3,014	20	SLB	530	4
SEN	11,271	75	IRN	2,835	19	MDA	496	12
BOL	10,878	73	ZAF	2,610	47	WSM	398	3
TUN	10,832	72	SOM	2,550	30	BTN	332	2
GHA	9,933	66	LBN	2,445	16	MDV	320	2
BRA	9,852	66	AGO	2,175	15	ARE	314	3
SDN	9,621	64	BWA	2,166	14	TKM	235	4
ZMB	9,381	63	LAO	2,126	14	LCA	213	2
SLV	8,194	55	SLE	2,095	14	CUB	212	1
MMR	8,067	54	ARG	2,081	14	MLT	211	2
MOZ	8,016	55	DJI	1,977	15	SUR	119	1
ETH	7,747	52	KOR	1,966	44	KWT	100	1
COL	7,675	51	KAZ	1,839	28	BRB	81	1
MLI	7,215	48	MNG	1,796	18	BHR	68	0
MEX	7,179	48	PAN	1,747	12	SVN	68	1
MDG	7,022	47	LSO	1,665	11	LBY	62	0
HND	6,592	44	MUS	1,602	11	BRN	50	1
DZA	6,490	43	GEO	1,540	26	QAT	30	0
NPL	6,214	41	ARM	1,518	25	BHS	13	0
MWI	6,180	41	AFG	1,510	12			

ODA is in millions of 2004 U.S. dollars. Observations are ordered by Total ODA.

FIGURE A1. OUTLIERS

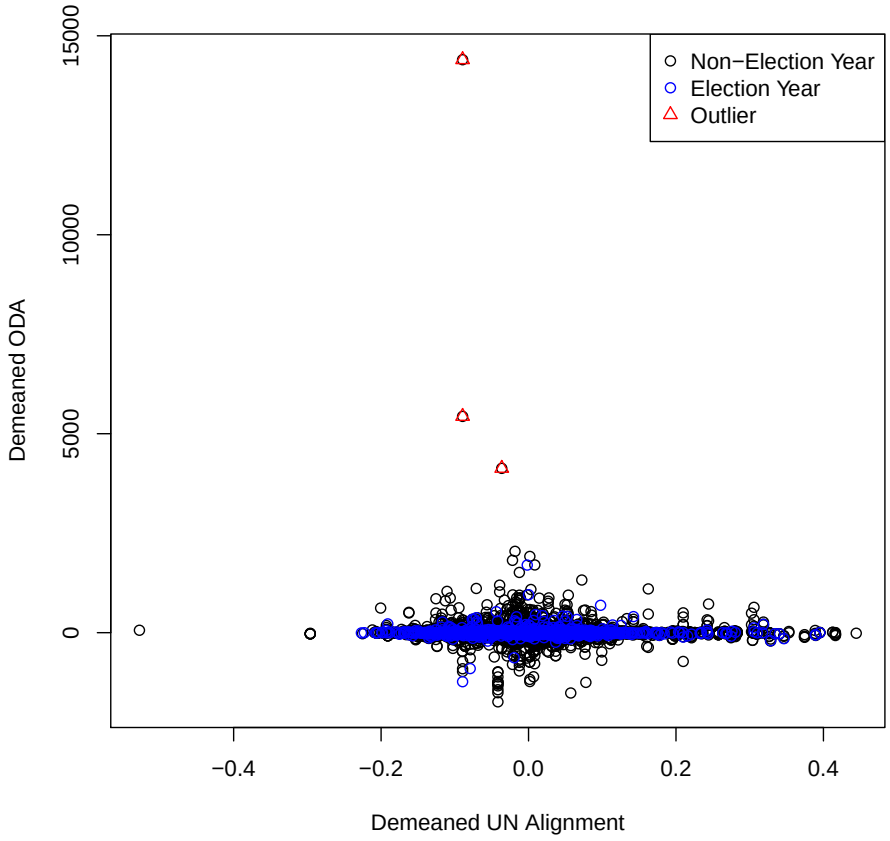

TABLE A.5—POLITICAL AID CYCLES CONTROLLING FOR OUTLIERS

Regressor	I
Exec. Election	-13.390
	(7.256)*
UN * Election	26.250
	(12.394)**
UN Agreement	8.886
	(17.268)
Fixed Effects	DR,Y,Outliers
N	15315
R^2	0.661

Robust standard errors in parenthesis are multi-way clustered by donor, recipient and year. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.6—Estimates Excluding 3 Largest Recipients (EGY, IDN, IND))

Regressor	I	II	III	IV	V	VI	VII	VIII	IX
Exec. Election	2.391	1.345	-10.765	-8.853	9.138	7.225			
	(2.342)	(2.254)	(4.431)**	(2.608)***	(10.662)	(6.167)			
Competitive Election							-16.469	-13.676	
							(5.495)***	(2.697)***	
Noncompetitive Election							-3.410	-1.185	
							(4.147)	(3.497)	
Early Election									-1.961
									(9.917)
Late Election									-14.307
									(3.812)***
UN * Election			20.338	16.118					
			(7.005)***	(4.145)***					
UN Avg. * Election					-10.483	-8.423			
					(15.500)	(7.572)			
UN Residual * Election					26.001	18.996			
					(8.780)***	(5.582)***			
UN * Competitive Election							26.218	21.503	
							(6.059)***	(0.000)***	
UN * Noncompetitive Election							13.254	7.095	
							(9.409)	(8.990)	
UN * Early Election									1.571
									(14.763)
UN * Late Election									27.864
									(7.093)***
UN Agreement			-22.962	-56.972			-22.283	-64.929	-18.583
			(14.228)	(24.298)**			(14.013)	(23.845)***	(15.416)
UN Donor Avg.					19.759	-24.216			
					(18.600)	(24.590)			
UN Residual					-60.960	-136.366			
					(16.613)***	(33.827)***			
Fixed Effects	DR,Y	DR	DR,Y	DR	DR,Y	DR	DR,Y	DR	DR,Y
Macro Controls	N	Y	N	Y	N	Y	N	Y	N
N	14865	13045	14865	13045	14865	13045	14855	13040	13985
R^2	0.004	0.017	0.005	0.019	0.006	0.021	0.005	0.019	0.004

Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.7—Estimates Excluding 5 Largest Recipients (EGY, IDN, IND, ISR, CHN))

Regressor	I	II	III	IV	V	VI	VII	VIII	IX
Exec. Election	2.104	1.139	-7.433	-6.519	11.835	7.082			
	(2.605)	(2.545)	(4.763)	(2.739)**	(10.357)	(5.025)			
Competitive Election							-11.882	-10.629	
							(5.871)**	(2.550)***	
Noncompetitive Election							-2.03	-0.890	
							(3.181)	(1.800)	
Early Election									-0.265
									(9.591)
Late Election									-10.223
									(4.586)**
UN * Election			14.728	12.077					
			(8.123)*	(5.772)**					
UN Avg. * Election					-15.107	-8.744			
					(14.658)	(7.047)			
UN Residual * Election					20.245	15.450			
					(11.191)*	(7.935)*			
UN * Competitive Election							18.564	15.908	
							(8.481)**	(3.911)***	
UN * Noncompetitive Election							11.271	7.495	
							(7.945)	(7.131)	
UN * Early Election									-0.224
									(14.824)
UN * Late Election									20.788
									(8.459)**
UN Agreement			-12.548	-40.946			-11.926	-45.713	-7.657
			(13.954)	(22.405)*			(15.554)	(20.605)**	(14.322)
UN Donor Avg.					24.134	-26.274			
					(14.828)	(19.233)			
UN Residual					-44.766	-74.464			
					(21.914)**	(38.866)*			
Fixed Effects	DR,Y	DR	DR,Y	DR	DR,Y	DR	DR,Y	DR	DR,Y
Macro Controls	N	Y	N	Y	N	Y	N	Y	N
N	14625	12805	14625	12805	14625	12805	14615	12800	13745
R^2	0.006	0.007	0.006	0.009	0.007	0.009	0.007	0.009	0.006

Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.8—Estimates Using Ln(ODA))

Regressor	I	II	III	IV	V	VI	VII	VIII	IX
Exec. Election	0.082	0.059	-0.070	-0.130	0.411	-0.235			
	(0.052)	(0.000)***	(0.103)	(0.172)	(0.648)	(0.000)***			
Competitive Election							0.062	-0.025	
							(0.215)	(0.214)	
Noncompetitive Election							-0.216	-0.219	
							(0.380)	(0.310)	
Early Election									0.400
									(0.568)
Late Election									-0.308
									(0.180)*
UN * Election			0.212	0.288					
			(0.184)	(0.264)					
UN Avg. * Election					-0.532	0.470			
					(1.007)	(0.000)***			
UN Residual * Election					0.353	0.509			
					(0.129)***	(0.321)			
UN * Competitive Election							-0.005	0.148	
							(0.369)	(0.335)	
UN * Noncompetitive Election							0.461	0.400	
							(0.512)	(0.417)	
UN * Early Election									-0.488
									(0.770)
UN * Late Election									0.548
									(0.328)*
UN Agreement			3.137	0.343			2.981	0.519	3.152
			(2.015)	(2.283)			(2.156)	(1.942)	(2.048)
UN Donor Avg.					3.854	-2.951			
					(1.692)**	(5.002)			
UN Residual					2.528	9.274			
					(4.554)	(7.376)			
Fixed Effects	DR,Y	DR	DR,Y	DR	DR,Y	DR	DR,Y	DR	DR,Y
Macro Controls	N	Y	N	Y	N	Y	N	Y	N
N	15315	13495	15315	13495	15315	13495	15305	13490	14435
R^2	0.061	0.036	0.064	0.036	0.064	0.049	0.066	0.036	0.061

Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$