Political Aid Cycles[*]

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Abstract

Researchers have scrutinized foreign aid's effects on poverty and growth, but anecdotal evidence suggests that donors often use aid for other ends. We test whether donors use bilateral aid to influence elections in developing countries. We find that recipient country administrations closely aligned with a donor receive more aid during election years, while those less aligned receive less. Consist with our interpretation, this effect holds only in competitive elections, is absent in U.S. aid flows to non-government entities, and is driven by bilateral alignment rather than incumbent characteristics.

1   Introduction

Since Boone (1994; 1996) reignited debate on the effectiveness of foreign aid, a large literature has examined its impacts on economic development.[1] This is an important agenda on normative grounds and eminently sensible: after all, Official Development Assistance (ODA) is by definition financing "with promotion of economic development and welfare as the main objective."(OECD, 2006) Might it nevertheless miss part of the story? Might donors also use aid for political change abroad?

The 2006 Palestinian elections suggest so. In these elections the U.S.-backed incumbent Palestinian Authority (P.A.) faced strong opposition from Hamas. In the weeks preceding the elections the United States Agency for International Development Assistance (USAID) funded several development programs in Palestine including the distribution of free food and water, a street-cleaning campaign, computers for community centers, and even a national youth soccer tournament. A progress report distributed to USAID and State Department officials was strikingly candid about the objective:

> "Public outreach is integrated into the design of each project to highlight the role of the P.A. in meeting citizens needs. The plan is to have events running every day of the coming week, beginning 13 January, such that there is a constant stream of announcements and public outreach about positive happenings all over Palestinian areas in the critical week before the elections."
> (Wilson and Kessler, 2006)

This paper examines whether the Palestinian episode is indicative of a broader phenomenon: do donors use foreign aid to influence elections?

If so, the political cycles literature suggests that donors might adjust aid flows immediately before elections according to whether they favor the incumbent or not. We test this hypothesis using an empirical strategy analogous to differences-in-differences estimation: we compare aid flows in election and non-election years for recipient administrations that are more or less politically aligned with a donor. If donors do attempt to help more aligned administrations (or hamper less aligned ones) then we should see differentially higher aid flows in election years for more aligned administrations. Following Alesina and Dollar (2000) we use voting patterns at the United Nations to measure alignment between administrations.[2] We include donor-recipient fixed effects in all our models, and

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[1] E.g. Burnside and Dollar (2000), Easterly et al. (2004), Rajan and Subramanian (2005), Rajan and Subramanian (2007), and Werker and Cohen (2009); see Radelet (2006) or Rajan and Subramanian (2008) for overviews.

[2] By using UN voting to measure alignment we do not necessarily mean to imply that aid is being exchanged for UN votes, though that interpretation is consistent with our results. Dreher et al. (2008)

hence estimate election effects using only time variation within donor-recipient pairs.

We find that donors give significantly more aid in election years to recipient administrations that are more closely aligned. An administration that is two standard deviations more politically aligned with the donor receives $20 million more ODA on average during an election year as opposed to a non-election year, which is 35% of mean annual ODA level in our sample. The least aligned administrations experience a decrease in aid during election years: an administration one standard deviation below mean alignment receives $8 million less on average during an election year. On average aid is no higher during election years than non-election years, which suggests that donors and not recipients are responsible for political aid cycles.

We provide three additional pieces of evidence for the influence hypothesis. First, political aid cycles are driven by donor-specific alignment rather than alignment with the "average" donor, confirming that bilateral politics is the key driver. Second, political aid cycles are concentrated in competitive elections, as one would expect if donors are more likely to intervene when the stakes are higher. Third, U.S. funding to non-governmental and opposition groups exhibits (statistically insignificant) political cycles *opposite* to those in ODA, consistent with the influence hypothesis but inconsistent with omitted variables interpretations. The Appendix shows that our results are also robust to using aid disbursements v.s. commitments, to dropping the largest aid recipients, to controlling directly for outliers, and to focusing on elections that take place later in the calendar year.

Our findings add to work on aid's determinants, which has shown that aid allocations are strongly correlated with measures of political alignment.[3] Along with Kuziemko and Werker (2006) and Dreher and Jensen (2007) this paper provides evidence that politics plays a causal role. This point has further implications for the aid effectiveness literature, which has built on the determinants literature by using political variables as instruments for aid. The assumption implicit in this strategy is that politics affects how aid is allocated but not how it is used. Our results raise the question of whether intent matters – that is, whether aid given for political ends has different impacts than aid given purely to promote development.[4] They also imply that selecting leaders may be an important channel through which aid influences development.[5]

Our analysis also fits within work on political cycles more generally, which has docu-

present evidence that US aid is used to buy UN votes without specific reference to elections.

[3]E.g. Alesina and Dollar (2000), Neumayer (2002), Barro and Lee (2005), and Dreher et al. (2009).

[4]Clemens et al. (2004) make the related argument that different kinds of aid have impacts over different time horizons.

[5]See Jones and Olken (2005) on the importance of national leaders.

mented a pattern of fiscal expansion prior to elections in developing countries.[6] Several of the explanations proposed for this pattern may also explain our results – voters may naively "vote their pocketbooks" or may infer candidate quality from social spending (Rogoff, 1990). Alternatively, changes in aid flows may signal the strength of the incumbent's relationship with the donor. As for impacts, there is some evidence that fiscal policy affects voting, though valid instruments are scarce.[7]

The rest of the paper is organized as follows: Section 2 describes our data sources and Section 3 our empirical methodology. In Section 4 we present the main results along with robustness checks and extensions; the Appendix presents additional robustness checks. Section 5 concludes.

2   Data Sources

Our source for ODA figures is the OECD Development Co-operation Directorate (DAC) database. We use ODA commitments from 1975-2004, inflated to constant 2004 U.S. dollars, as our main outcome variable. (Results using aid disbursements instead are somewhat smaller but otherwise similar – see Appendix Table A.2). We consider only bilateral donors, since multilateral donors have multiple stakeholders making it difficult to define donor-recipient alignment. We focus on the five largest donors during the study period (the United States, Japan, Germany, France, and the United Kingdom) who together accounted for 77% of aid commitments. This gives us a 30-year panel with 116 recipients in total and an average of 102 recipients per donor-year.[8] It is important to note that ODA as defined by the DAC must meet three criteria: it must be "(a) undertaken by the official sector (b) with promotion of economic development and welfare as the main objective (c) at concessional financial terms [if a loan, having a Grant Element of at least 25 per cent]" (OECD, 2006). ODA does not include grants, loans and credit for military purposes or transfer payments to private individuals.

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[6]E.g. Schuknecht (1996), Alesina et al. (1999), Drazen (2000), Schuknecht (2000), Franzese (2002), and Shi and Svensson (2003). Brender and Drazen (2005) argue that cycles are concentrated in new democracies.

[7]Using neighboring districts as instruments, Levitt and Snyder (1997) find that federal spending helps incumbent Congressmen in the U.S.; Brender and Drazen (2008) estimate that larger deficits hurt rather than help incumbents internationally. In our context, incumbent political alignment is a poor instrument since it likely affects voting through channels other than aid.

[8]We exclude from the analysis recipients with gaps in key time series (elections and UN votes) as including them would cloud the interpretation of fixed-effects estimators. Such gaps generally reflect internal strife (e.g., Iraq, Somalia) or other important transformations in the country which would affect both political alliances and aid levels. Where we introduce additional variables we again drop any pairs for whom we do not observe all variables in an unbroken sequence.

To account for the possibility that some aid is specifically intended to facilitate elections we incorporate data from the DAC Creditor Reporting System (CRS), which presents ODA statistics broken down by purpose. The database is incomplete and includes only a fraction of the ODA in the DAC database; OECD representatives claim it is a random subsample of the DAC data but cannot account for specific differences. We nonetheless use the data to subtract aid directed towards "elections" and "government administration" (sector codes 15161 and 15140) from our outcome variables in order to be sure that there is nothing mechanical driving our results. Of course, since our goal is to estimate *differential* election year effects by political alignment, election-facilitating aid is a priori unlikely to affect the results. In practice, the excluded aid makes up only 1% of total CRS aid and whether we include or exclude it has essentially no impact on the results.

Data on executive elections come from the World Bank Database of Political Institutions (DPI) (Beck et al., 2005). The data include the years that elections were held between 1975 and 2004. We observe a total of 274 elections in our final sample, with 71 of the 116 aid-receiving countries holding at least one election. Most (262) executive elections take place in presidential systems, with the rest (12) in parliamentary systems. The mean probability of an election occurring in any particular recipient-year is 8.9% and increases by 0.2% per year after controlling for recipient fixed-effects (not reported). The DPI also includes information on the election outcomes and an index of electoral competitiveness which we describe in Section 4 where we differentiate our results by the competitiveness of elections.[9]

Our measure of political alignment is the fraction of UN General Assembly votes in which both countries voted (either yes or no) for which their votes coincided. Any vote for which a country was absent, abstained, or was not a member of the UN is not counted in its total. We calculate this fraction for every donor-recipient-year observation and use the average of this variable over each recipient administration's term in office as our measure of alignment. This aggregation does not distinguish between more and less important votes, but we were unable to systematically improve upon it since donors likely differ in the importance they attach to specific votes. General Assembly voting records are taken from *Documenting Votes in the UN General Assembly v2.0* (Voeten, 2005). According to the measure, in 2004 the recipient most aligned with the United States was Israel (UN alignment of .96) while the one least aligned was North Korea (.02). On the other

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[9]We also tested for aid cycles around legislative elections (which are also documented in the DPI) and found no evidence of significant patterns. This could reflect difficulties of channeling funds to specific legislators, or it could simply mean that UN voting is a better measure of executive than of legislative preferences.

hand, France was least aligned with Israel (.51) but most aligned with Slovenia (.98). Interestingly, recipients' UN alignment with the United States does not appear to reflect public sentiment: in the 2004 cross-section it is negatively correlated with the fraction of foreigners with a favorable view of the Unites States ($\rho = -0.22, p = 0.26$) and the fraction that believe that the United States takes into account the interests of countries like their own ($\rho = -0.35, p = 0.07$), as measured by the Pew Global Attitudes Survey.

Additional demographic and economic controls are taken from the *World Development Indicators* database (World Bank, 2005). Tables 1 and 2 present summary statistics.

3 Empirical Methodology

Let d index donor countries, r index recipient countries, and t index years. We first estimate the direct relationship between bilateral aid and elections:

$$ODA_{drt} = \delta_1 ELEC_{rt} + X'_{drt}\beta + \alpha_{dr} + \epsilon_{drt} \qquad (1)$$

Here $ELEC_{rt}$ is an indicator which takes the value 1 if recipient r held an election for the chief executive office in year t, X_{drt} is a vector of time-varying donor or recipient-specific control variables such as population and GDP, and α_{dr} represents a vector of donor-recipient fixed effects. In this specification δ_1 represents the average effect of elections on aid flows. Our donor influence hypothesis does not predict anything particular about δ_1: in some cases a donor may wish to support an incumbent by increasing aid flows, while in others the donor may seek to hurt an incumbent's electoral chances by restricting aid flows, and δ_1 averages together these offsetting effects.

There are, however, several other reasons why one might expect aid to be systematically different in election years. First, elections could alter aid's effectiveness. Media scrutiny might be higher and electoral discipline stronger, leading to more effective use of funds; alternatively, incentives for corruption might be stronger because of the need to finance campaigns or because of shortened time horizons, leading to less effective use of funds. Election years might also be more prone to civil unrest and internal conflict, which could either attract or deter aid. Second, elections might be endogenously timed in some countries. Politicians with discretion over the timing of elections may schedule them to coincide with periods of strong economic performance in order to increase their chances of reelection (Ito, 1990). They might also aim to hold elections at times when aid inflows were high. If so we should see a positive relationship between elections and aid. Finally, aid might increase during election years because recipients intensify their requests for aid in hopes of boosting their electoral prospects. This view is similar to our

influence hypothesis in that aid is used to influence elections, but distinct in that incumbent leaders rather than donors have agency. A key distinguishing feature of this view is that aid should only increase and not decrease during election years, since incumbents would never request aid reductions. We estimate Equation 1 as an omnibus test of these hypotheses.

One statistical concern is that elections may be correlated with unobserved recipient characteristics – for example, well-governed countries might hold elections more frequently and also receive more aid. We address this possibility by including donor-recipient fixed effects (the α_{dr}s) in Equation 1. This eliminates any time-invariant attributes of recipients and of their bilateral relationships with donors, leaving only time variation within donor-recipient pairs with which to estimate election effects. Another concern is that trends in aid have mirrored trends in election frequency. To control for this possibility we include, alternately, year fixed effects, donor-year fixed effects, and time-varying controls such as population and GDP.

After estimating (1) we move to our main question: do donors provide differentially more aid in election years to recipient administrations with which they are more politically aligned? Donors might engage in this behavior if changes in aid flows could affect election results. There are several channels through which this effect could operate. It could be that increases in aid allow a government to increase visible social spending (e.g., distribution of free food, employment and public works programs) as in the Palestinian anecdote in the Introduction. Social spending might directly influence voters beliefs about the candidate, or indirectly by signaling something about candidate quality (Rogoff, 1990). Incumbents may also be able to use public funds to directly finance campaign activities, or inflows of aid might signal levels of foreign support for the incumbent government. Regardless of the exact mechanism, we expect increased aid to improve an incumbent's chances of winning re-election, while reductions in aid would harm these chances.[10]

To test the influence hypothesis we estimate

$$ODA_{drt} = \delta_1 UN_{drt} + \delta_2 ELEC_{rt} + \delta_3 ELEC_{rt} * UN_{drt} + X'_{drt}\beta + \alpha_{dr} + \epsilon_{drt} \qquad (2)$$

Here UN_{drt} is our measure of political alignment between the donor d and recipient r at time t, which captures average alignment over the recipient administration's previous

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[10]Levitt and Snyder (1997) show that election-year spending helps incumbent U.S. Congressmen. Brender and Drazen (2008) show that larger deficits are associated with lower re-election probabilities in a large sample of countries; this supports our hypothesis to the extent that aid substitutes for deficits, but of course aid can also be turned into spending without affecting deficits. More broadly, a growing literature has documented that voters do not perfectly disentangle politician performance from exogenous shocks (Achen and Bartels, 2004; Wolfers, 2007; Cole et al., 2008).

term in office. Intuitively, δ_3 estimates the differences-in-differences between election year effects for more and less politically aligned regimes. This identification strategy allows for unobserved factors that covary with both aid and elections (e.g., conflict and efficiency of aid spending, as discussed above) provided they do not do so differentially by political alignment. It also directly addresses concerns about election endogeneity: in order for endogenous election timing to bias estimates of this specification, more closely aligned governments would need to be more (or less) likely to shift elections toward such years. We include the same battery of time-varying controls as above.

A final methodological issue concerns inference. Even after removing donor-recipient and time fixed effects the error terms in (1) and (2) are unlikely to be conditionally uncorrelated within dimensions of the panel, as is necessary for consistency of the usual OLS standard errors (Bertrand et al., 2004; Petersen, 2005). Since there are several possible dimensions on which to cluster we use the most general approach, non-nested multi-way clustering along each of the three dimensions of the panel (donors, recipients, and years) as proposed by Cameron et al. (2006). The only restriction this imposes is that $\mathrm{Cov}(\epsilon_{drt}, \epsilon_{esu}) = 0$ for $d \neq e, r \neq s, t \neq u$. While we prefer this approach for its robustness, our results are also significant when we use more restrictive approaches, e.g. clustering on donor-recipient pairs to allow for autocorrelation.

4   Results

4.1   Is There an Election Year Effect?

We first test whether there are election-year aid shocks in recipient countries. Columns I-III of Table 3 report estimates of Equation 1, in which the election year indicator enters uninteracted as a predictor of bilateral aid. The results suggest a very small positive direct relationship between aid and elections that is statistically indistinguishable from zero. This is true whether we control for time-varying influences using year fixed effects (Column I), donor-year fixed effects (Column II), or macroeconomic controls (Column III). Moreover, this does not seem to be due to a lack of power: the magnitudes within a 95% confidence interval are still only on the order of $10 million.

The absence of a relationship between mean aid flows and election years increases our confidence that the differential effects we document below are not driven by omitted variable bias. Any variables correlated with election timing that affect aid decisions, such as domestic conflict, would have shown up in these regressions. The non-result is also inconsistent with the hypothesis that politicians systematically time elections to coincide with large inflows of aid; this accords with the existing literature, which has not found

strong evidence of endogenous timing (Alesina et al., 1999). Finally, it is inconsistent with the view that recipients proactively request more aid during election years and donors passively respond.

4.2 Is There a Differential Election Year Effect by Political Alignment?

We now turn to our main question: do ODA flows change differentially in election years for recipient administrations that are more and less aligned with the donor. Columns IV-VI of Table 3 report estimates of Equation 2, which are our central results. Starting with Column IV, we see that an administration that is two standard deviations more politically aligned with the donor can expect to receive $20 million more in ODA flows during an election year than the less aligned administration. This figure represents 35% of the average annual ODA level in our sample. Moreover, this large positive interaction term combined with the negative direct effect of an election year implies that while the more politically aligned recipient administrations receive an increase in ODA during election years, the less politically aligned administrations experience a decrease in ODA. For example, an administration one standard deviation below the mean level of donor-alignment receives $8 million less on average during an election year. In contrast, an administration one standard deviation above the mean receives $12 million more during an election year.

The fact that less aligned administrations receive less aid during election years is particularly important as it is incompatible with the idea that recipients themselves drive the observed patterns: one would have to believe that less aligned administrations request a decrease in aid during election years. Similarly, endogenous timing is an unlikely explanation: one would have to believe that less-aligned governments systematically choose low-aid years in which to hold elections.

Regarding robustness, Columns V and VI show that the results hold up when we include donor-year fixed effects or macroeconomic controls; they are also robust to recipient-specific trends (not reported). The Appendix shows that political cycles are more pronounced for elections that take place in the latter half of the calendar year and insignificant for elections that take place in the first three months of the year, confirming that our results are driven by aid before rather than after elections. The Appendix also reports checks for the influence of outliers. In brief, the results are robust to (a) controlling for individual outlier observations, (b) dropping the larger aid recipients, and (c) using aid disbursements rather than commitments as the dependent variable. The point estimates are smaller in some of these specifications but consistently statistically significant and

economically relevant. Estimates using the logarithm of ODA as the dependent variable are less precise, but consistent in sign and magnitude with our base results and significant for elections after the first three months of the year (as is the case when estimating in levels).

It is difficult to accurately quantify the costs of political campaigning in developing countries, but what rough figures are available suggest that our estimates are large. For example, the Washington Post estimates that the main candidates in Kenya's 2007 presidential election spent $6 million to $10 million each on their campaigns (McCrummen, 2007). Vicente (2007) studies vote-buying in São Tomé and Príncipe and estimates that the median price of a presidential vote is $4.20.

4.3 Are Aid Cycles Really Bilateral?

As a first interpretation check we examine in more detail what exactly "donor-recipient political alignment" measures. Although it is natural to think of UN voting measuring international relations, it could also be that high UN alignment captures some other characteristic of an incumbent leader that makes him attractive to donors. It could even be that donors use political cycles to support the leaders that are best for their countries, in which case aid cycles would still be consistent with the definition of ODA. This seems unlikely given the variation in UN voting across donors – recall that in 2004 Israel was the U.S.'s most-aligned recipient and also France's least-aligned – but a systematic test is desirable.

We implement a test as follows: we decompose UN_{drt} into the component $\overline{UN}_{rt} \equiv \sum_d \frac{UN_{drt}}{D}$ which is common to all donors and the residual $\widetilde{UN}_{drt} = UN_{drt} - \overline{UN}_{rt}$ which is donor-specific. Intuitively, if political aid cycles are driven by recipient-level characteristics then we should find that they are predicted by the common component of UN votes, while if bilateral politics are key then the donor-specific component should drive the results. We estimate

$$ODA_{drt} = \delta_1 \widetilde{UN}_{drt} + \delta_2 \overline{UN}_{rt} + \delta_3 ELEC_{rt} + \delta_4 ELEC_{rt} * \widetilde{UN}_{drt} + \delta_5 ELEC_{rt} * \overline{UN}_{rt}$$
$$+ X'_{drt}\beta + \alpha_{dr} + \epsilon_{drt} \quad (3)$$

Columns VII-IX of Table 3 present the results. We find that aid cycles are driven entirely by the idiosyncratic, donor-specific component of UN alignment; the election-year coefficient on the common component is small and statistically insignificant. This suggests that aid cycles are indeed about bilateral politics and not a concerted effort by donors to select particular kinds of leaders for recipient countries.

4.4 Are The Effects Stronger in Closer Elections?

We next differentiate elections by how readily a donor could influence them, proxying for ease of influence with measures of competitiveness. The motivating idea is that changes in aid flows are more likely to influence an election that is hanging in the balance that one that is a foregone conclusion. To test this formally we allow for distinct political cycles around non-competitive elections (indicated by $NONCOMP_{rt}$) and competitive ones (indicated by $1 - NONCOMP_{rt}$):

$$ODA_{drt} = \delta_1 UN_{drt} + \delta_2 NONCOMP_{rt} + \delta_3 UN_{drt} * NONCOMP_{rt}$$
$$+ \delta_4 ELEC_{rt} * (1 - NONCOMP_{rt}) + \delta_5 UN_{drt} * ELEC_{rt} * (1 - NONCOMP_{rt})$$
$$+ \delta_6 ELEC_{rt} * NONCOMP_{rt} + \delta_7 UN_{drt} * ELEC_{rt} * NONCOMP_{rt} + \alpha_{dr} + \epsilon_{drt} \quad (4)$$

Here δ_4 and δ_5 measure political cycles around competitive elections, while δ_6 and δ_7 measure those around non-competitive ones. We predict the former pair to be large and the latter pair small.

We use two complementary proxies for the competitiveness of an election. The first characterizes an election as noncompetitive if the winner received more than 75% of the votes. We observe this measure for 225 elections, of which 73 are defined as noncompetitive. For example, the 1996 presidential election in Zimbabwe in which Robert Mugabe received a commanding 93% of the vote is characterized as noncompetitive. This outcome might reflect either overwhelming support for Mugabe or electoral fraud; in either case we expect that this election would have been difficult for a donor to influence.

The vote-share approach has several limitations. One is availability: we observe voting outcomes for only 64 of the 71 aid-receiving countries that held elections, and of course for none of those that did not. A second is potential endogeneity: if variation in ODA affects which elections have a winner with more than 75% of the vote then our estimates would be biased (though the sign of the bias is unclear). Though we believe that setting the threshold at 75% minimizes this possibility, we cannot rule it out. To compensate for these limitations we also introduce a second proxy for competitiveness: an indicator for whether multiple parties are illegal at the time of the election. We derive this from the DPI's Executive Index of Electoral Competitiveness (EIEC) (Beck et al., 2005). This is an objective measure of political competition, available for all 274 elections in our sample (105 of which it categorizes as non-competitive) as well as for for aid-receiving countries that did not hold executive elections, and not subject to the endogeneity bias discussed above. The obvious drawback is that it captures only one aspect of competitiveness – there are surely elections in our sample in which multiple parties were legal and yet the outcome was never in doubt. The EIEC measure and the voting measure of noncompetitiveness

are positively correlated but the magnitude of the correlation is only 0.50, suggesting that they do contain different information.

The first three columns of Table 4 present results using the vote-based measure of competitiveness. For competitive elections our point estimates again indicate a political aid cycle, smaller in magnitude but highly significant.[11] For non-competitive elections, however, there is no evidence of a cycle at all. This contrast becomes even sharper when we use the EIEC-based measure of competitiveness in Columns IV-VI. We find a large and strongly significant aid cycle around competitive elections, but no cycle around non-competitive ones. These results support the joint hypothesis that donors use aid to influence elections and that the expected returns to doing so are greater in more competitive elections.

4.5   Are there Cycles in Aid Flows to NGOs?

Our analysis thus far has focused exclusively on ODA flows, which originate with donor governments and benefit recipient governments. But some donors also give money to non-governmental entities in developing countries. Testing for political cycles in these aid flows is interesting for two reasons. First, it serves as a placebo test: if our ODA results were driven by some omitted variable affecting the desirability of sending aid then we would expect to see analogous cycles in aid to non-governmental groups. Second, if some of the non-governmental groups who receive foreign aid are in fact part of the political opposition then our influence hypothesis predicts an *opposite* political cycle in their funding.

Unfortunately we do not have systematic data on support from donors to non-governmental groups in developing countries. However, we did obtain data on funds channeled through the U.S. National Endowment for Democracy. Congress established the NED in 1983 at President Reagan's request to promote democracy abroad, and it continues to receive over 99% of its $110 million in annual revenue from the federal government (NED, 2006). The NED funds many non-governmental entities (e.g. trade unions, civic and social organizations, policy institutes, media outlets, and political parties) which potentially represent a mixture of political opposition groups and non-political organizations. While the NED's stated policy is "not [to] pick and choose among the democratic competitors in countries where such competition is possible" (NED, 2007), observers have accused it of doing so. For example, in the 1980s it funded groups opposing Costa Rican president and 1987

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[11]Estimated standard errors are 0 due to an inherent limitation of multi-way clustering, namely that in finite samples it may yield negative estimated variances. These are the only estimates in the paper for which this is an issue.

Nobel Peace Laureate Oscar Arias, whose peace proposals for Central America conflicted with Reagan's support for the Contras in Nicaragua (Greenberger, 1989). Suggestively, U.S. ODA to Costa Rica fell from \$213M in 1986 when Arias won the presidency to \$124 in 1990 when his party's nominee lost to Rafael Fournier (Arias was constitutionally prohibited from seeking re-election).

We obtained data on all NED grants from 1990 to 2005 including amounts, years, and recipient countries. In Table 5 we report estimates of Equation 2 using NED flows as our new outcome variable, with results for U.S. ODA over the same time period in Column IV for comparison (note that ODA flows greatly exceed NED grantmaking, explaining the difference in magnitude of the coefficients). While the NED estimates are not significant they point in the opposite direction as those for ODA flows: NED aid is higher in election years when UN votes is low, and lower when UN votes is high. This is consistent with the influence hypothesis but inconsistent with omitted variables interpretations of our earlier results.

5 Conclusion

This paper argues that donor countries use ODA to influence the outcome of foreign elections. During election years, donor countries increase ODA to recipient administrations that are politically aligned with them and reduce ODA to less aligned administrations. Auxiliary tests support our interpretation of this pattern. First, it is driven by alignment between specific donors and recipients, not alignment between recipients and donors in general. Second, it holds only for competitive elections. Third, U.S. funding to non-governmental entities follows an opposite pattern, albeit insignificantly.

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Table 1: Overall Summary Statistics

Variable	N	Mean	S.D.
Donor/Recipient/Year Level			
ODA	15315	57.19	232.56
UN Alignment	15315	0.64	0.22
Recipient/Year Level			
Election	3063	0.09	0.29
GDP	2760	42.16	115.48
Population	3049	36.35	135.91
Donor/Year Level			
GDP (Donor)	150	2910.67	2510.21
Population (Donor)	150	113.56	74.18

"Total ODA" is ODA commitments in millions of 2004 US dollars. "UN Agreement" is the average sample probability of agreement between donor and recipient votes at the United Nations. Population is in millions and "Corruption" is a linear transformation of the ICRG index ranging from 0 (least corrupt) to 1 (most corrupt).

Table 2: Summary Statistics by Donor

Donor	Total ODA	Mean UN	Fraction Positive
United States	285220	0.86	0.97
Japan	259400	0.28	0.82
Germany	151744	0.74	0.98
France	115161	0.69	0.86
United Kingdom	71050	0.62	0.87
Other	270591	0.84	0.46

"Total ODA" is ODA commitments in millions of 2004 US dollars. "Fraction Positive" is the fraction of donor-recipient-year observations for which bilateral ODA is positive. "Mean UN" is the average sample probability of agreement between donor and recipient votes at the United Nations.

Table 3: Effect of Recipient Country Executive Elections on ODA

Regressor	I	II	III	IV	V	VI	VII	VIII	IX
Exec. Election	2.183	2.183	0.428	-27.346	-19.861	-23.102	-5.853	-5.853	-1.525
	(3.290)	(3.287)	(3.275)	(9.848)***	(7.428)***	(7.407)***	(16.089)	(17.217)	(11.139)
UN * Election				45.458	33.990	36.734			
				(17.687)**	(13.956)**	(14.387)**			
UN Avg. * Election							12.165	12.165	3.771
							(28.920)	(28.777)	(21.320)
UN Residual * Election							51.590	34.971	40.753
							(19.323)***	(15.522)**	(17.131)**
UN Agreement				-16.450	-20.244	-81.142			
				(21.303)	(23.978)	(40.862)**			
UN Donor Avg.							25.292	25.292	-39.641
							(23.867)	(33.212)	(36.098)
UN Residual							-53.023	-132.273	-179.880
							(25.197)**	(43.164)***	(66.551)***
Population			-0.439			-0.447			-0.424
			(0.374)			(0.379)			(0.374)
GDP			0.220			0.228			0.226
			(0.000)***			(0.000)***			(0.000)***
Population (Donor)			-4.018			-4.430			-4.941
			(0.830)***			(0.852)***			(0.760)***
GDP (Donor)			0.045			0.048			0.051
			(0.010)***			(0.009)***			(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	0.003	0.017	0.006	0.004	0.017	0.007	0.004	0.017	0.008

Each column reports a separate regression. The dependent variable in all columns is bilateral ODA. Fixed effects are denoted DR for donor-recipient pair, Y for year, and DY for donor-year pair. 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 4: Effect of the Competitiveness of the Election on Political Aid Cycles

Regressor	I	II	III	IV	V	VI
UN Agreement	-6.744	19.338	-40.689	-18.687	-49.965	-107.690
	(13.585)	(17.599)	(27.763)	(20.080)	(24.209)**	(45.997)**
Noncompetitive	-2.846	-9.551	-6.259	-7.570	-47.781	-39.873
	(6.948)	(6.947)	(3.705)*	(15.908)	(1.461)***	(7.766)***
UN * Noncompetitive	1.744	12.367	2.359	-4.212	59.927	51.410
	(8.844)	(9.802)	(5.373)	(16.483)	(0.000)***	(9.470)***
Competitive Election	-16.519	-8.528	-13.531	-43.488	-35.549	-38.683
	(0.000)***	(0.000)***	(0.000)***	(14.639)***	(12.745)***	(12.393)***
UN * Competitive Election	22.641	10.841	14.875	65.564	53.961	56.523
	(0.000)***	(0.000)***	(0.000)***	(24.683)***	(21.564)**	(21.078)***
Noncompetitive Election	-0.847	2.259	0.507	-3.541	5.429	4.490
	(6.854)	(4.245)	(8.410)	(5.449)	(3.552)	(5.176)
UN * Noncompetitive Election	1.671	-3.306	-3.125	14.484	-0.338	-0.667
	(8.135)	(2.796)	(11.159)	(10.697)	(8.389)	(10.830)
FEs	DR,Y	DR,DY	DR	DR,Y	DR,DY	DR
Controls	-	-	Yes	-	-	Yes
N	3310	3310	3005	15305	15305	13490
R^2	0.025	0.082	0.019	0.004	0.018	0.007

Each column reports a separate regression. The dependent variable in all columns is bilateral ODA. Columns I-III use a non-competitiveness indicator derived from vote shares; Columns IV-VI use an EIEC-based indicator. Competitiveness for non-election years is set equal to the competitiveness of the next upcoming election. Fixed effects are denoted DR for donor-recipient pair, Y for year, and DY for donor-year pair. Robust standard errors in parenthesis are multi-way clustered by donor, recipient, and year. The estimated multi-way clustered variances for "Competitive Election" and "UN * Competitive Election" in Columns I-III were negative and have been censored at 0. Statistical significance is denoted as: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 5: Election Year Effects of Aid to Non-governmental Entities

Regressor	I	II	III	IV (ODA)
Exec. Election	0.054	0.063	0.067	-140.143
	(0.058)	(0.053)	(0.054)	(71.982)*
UN * Election	-0.137	-0.179	-0.191	484.226
	(0.159)	(0.140)	(0.142)	(256.728)*
UN Agreement	-0.070	0.052	-0.023	-354.479
	(0.110)	(0.054)	(0.068)	(150.970)**
Population			-0.001	
			(0.002)	
GDP			0.002	
			(0.000)***	
Population (Donor)			0.002	
			(0.002)	
GDP (Donor)			0.000	
			(0.000)	
Fixed Effects	R	R,Y	R	R
N	1601	1601	1511	1601
R^2	0.002	0.025	0.201	0.004

Each column reports a separate regression. The dependent variable in Columns I-III is total NED flows in millions of 2004 US dollars; Column IV displays the results for U.S. ODA over the same time period for comparison purposes. Fixed effects are denoted R for recipient and Y for year. Robust standard errors are multi-way clustered by recipient and year. Statistical significance is denoted as: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

A Appendix (For Online Publication Only)

This Appendix examines the robustness of the results presented in the main text along two dimensions. Section A.1 explores potential mis-specification issues arising due to the granular timing of aid flows and elections, and Section A.2 examines the sensitivity of the results to methods for limiting the influence of outliers.

A.1 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 depend-

ing 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.

A.2 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 1 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% level).[12]

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

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[12]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.

flows a donor wishes to make prior to an election in 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% 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 1: Outliers
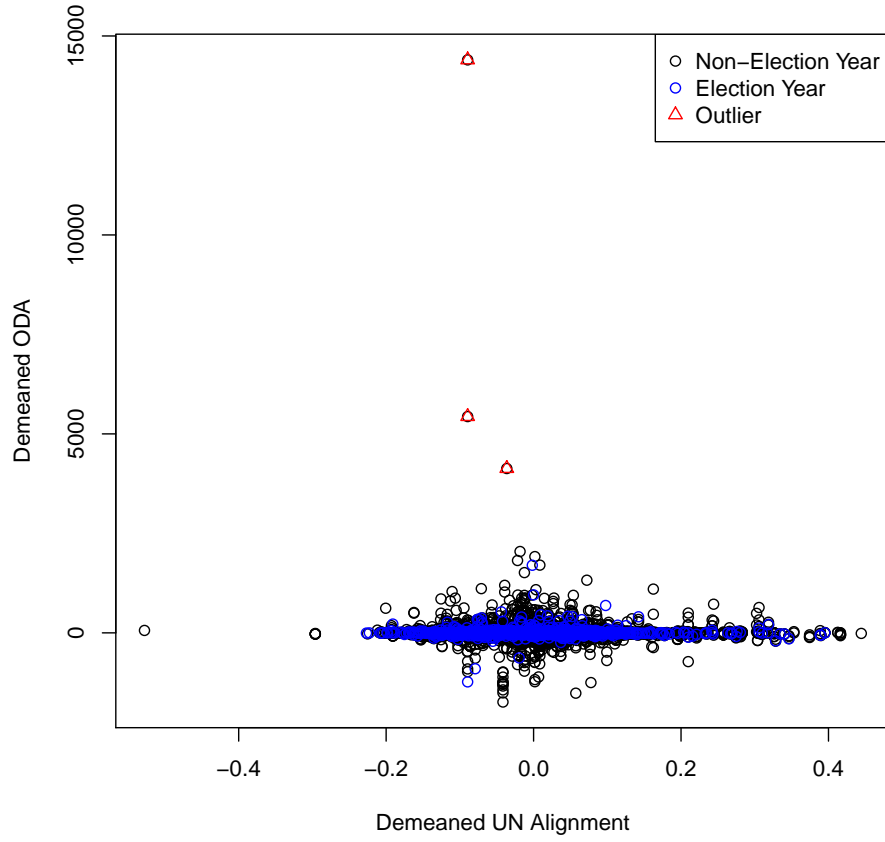
Table A.5: Political Aid Cycles Controlling for Outliers

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	3.854		2.981	0.519	3.152
			(2.015)	(2.283)	(1.692)**		(2.156)	(1.942)	(2.048)
UN Donor Avg.						-2.951			
						(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: $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$