

## Aid and Democracy Redux\*

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June 8, 2013

### Abstract

This paper uses Freedom House ratings to assess the impact of foreign aid on democracy. We employ an interval regression to account for Freedom House's method of rating countries. A cross-sectional analysis examining the long run effect of aid on democracy in 122 countries between 1972 and 2011 finds a significant positive relationship that survives various tests for endogeneity. A short run annual panel analysis of 156 countries between 1985 and 2011 explores whether aid operates through leverage and conditionality. We present evidence that i) donors allocate aid in response to democratization and ii) recipient countries respond to this incentive for democratic reform. Our identification strategy relies on the reduced importance of democratization in the allocation of aid to geopolitically important countries.

Key words: Democracy; Freedom House; Foreign Aid; Political Reform

JEL code: F35

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\*An early version of this paper was presented at the Transatlantic Community and Rising Powers in the Global Economy workshop, Berlin, May 20-21, 2011. Special thanks to Axel Dreher and Andreas Fuchs for access to data on Chinese aid. E-mail: erasmus.kersting@villanova.edu; chkilby@yahoo.com

# 1 Introduction

Does foreign aid promote democracy? A growing literature examining the impact of aid on democracy has reached divergent conclusions. In the most widely cited article on the topic, Knack (2004) finds no evidence that aid promotes democracy in a long run, cross-sectional analysis and several studies report aid undermines democracy in short run, panel analyses. Yet many researchers do find evidence of a significant positive impact, both in the long run (Heckelman 2010) and the short run (Bermeo 2011).

In this paper, we revisit the aid and democracy question, looking more carefully at issues of timing, utilizing an estimator that reflects the process generating democracy ratings, and taking advantage of an expanded data set. Timing is critical since aid's role as either input or incentive depends on whether aid flows precede or follow changes in democracy. The process employed by Freedom House to generate the democracy ratings used by Knack and others starts with a finer grain scale; an interval regression that reflects this data generating process is more efficient and avoids the attenuation bias of least squares. Finally compared with early work in the field, we take advantage of much more substantial post-Cold War data.

The impact of aid on democracy is largely an empirical matter as theory suggests both positive and negative effects. By promoting economic and social development, aid may plant the seeds for democracy (Lipset 1959). Educated voters are an important prerequisite for a well-functioning democracy and also more likely to demand self-determination. Assuming civil liberties and political rights are normal goods, greater demand for democracy goes hand-in-hand with economic growth. Aid can also promote democracy directly when it funds institutional development within the government and civil society, growth in news or social media, empowerment of oppressed groups, or election administration (so-called democratic aid). Finally, donors committed to democratic reforms may be able to use aid as leverage, making the continued flow of funds conditional on political reform.

However, aid may also stymie moves toward democracy. By providing an alternative to tax collection, aid can make recipient governments less accountable to their citizens. Aid flows may also be viewed as rents, competition for which results in corruption that weakens the fundamental institutions of government and promotes clientelism, thereby narrowing the selectorate. This is particularly likely when donors cannot credibly commit to enforcing aid conditions due to other overriding priorities, be they geopolitical (*e.g.*, the U.S. during the Cold War) or commercial (*e.g.*, China today). In sum, whether the net impact of aid on democracy, like the net impact of aid on growth, is positive or negative remains fundamentally an empirical issue.

This paper explores links between aid and democracy both in the long run using cross-sectional data and in the short run using annual panel data. A cross-sectional analysis similar to Knack (2004) but

covering 122 countries between 1972 and 2011 finds a significant positive relationship that survives various tests for endogeneity. The panel analysis finds a small positive link between aid and democracy but only for aid from OECD Development Assistance Committee (DAC) members. The panel analysis also uncovers evidence of a more substantial incentive effect. When recipients can expect donors to condition future aid on democratization (*i.e.*, when conditionality is credible), more democratic reform is observed.

Section 2 reviews recent empirical research on aid and democracy, placing it in a framework that links the timing of aid to its potential role in democratization. Section 3 details the empirical model and estimation method as well as key variables. Section 4 describes cross-sectional data and results for the long run impact of aid from DAC donors on democracy. Section 5 turns to panel data and short run analysis, capitalizing on the role of democracy in aid allocation decisions in order to explore possible incentive effects. Section 6 concludes.

## 2 Previous Research

Aid potentially can play any of three roles in the promotion of democracy. Over the long run, aid may promote economic and social developments that are preconditions for democratization. Over a short time horizon, aid may provide key immediate inputs to successful elections and a transition to democracy. Finally, if conditioned on democratic reform, aid acts as an incentive to induce the government or elements of civil society to undertake or advocate for democratization. A key question in empirical analysis is how to identify aid's impact under each of these scenarios.

[Figure 1 about here]

Figure 1 illustrates the three roles: aid as general input, aid as specific input, and aid as incentive. Over the course of decades, aid may promote growth (Minoiu and Reddy 2010, Clemens *et al.* 2012), education (Michaelowa and Weber 2006, Dreher *et al.* 2008, Christensen *et al.* 2011, Birchler and Michaelowa 2013), and the expansion of civil society. If democracy is a normal good (demand increases with income) that depends on an educated selectorate and active civic associations, aid may act as a general input for democratization by laying the necessary foundation. This argument suggests a link between aid over several decades and democratization over that or subsequent periods.

Just prior to an election, certain types of aid may act as a specific input to democratization. Aid targeted to opposition parties or non-governmental organizations (NGOs), poll observers or polling stations, election media, or technical assistance for drafting legislative reforms may have a direct and immediate effect

on civil liberties and political rights (Finkel *et al.* 2007, Kalyvitis and Vlachaki 2010, Nielsen and Nielson 2010, Scott and Steele 2011, Carothers and de Gramont 2013). This pathway might explain a link between certain categories of foreign aid and improvements in democracy over a short period of time (*e.g.*, one to three years). Alternatively, in the short run aid may promote stability and thus allow for democratic reform in some circumstances (Dutta *et al.* 2013).

Finally, aid may act as an incentive for democratic reform if key players in the recipient country expect that democratization will lead to sufficiently large increases in aid flows. Whether this takes the form of traditional conditionality (conditions stated *ex ante*) or selectivity (conditionality only applied *ex post*), it provides an incentive for political reform as long as donor conditionality is credible.

The literature on aid and democracy is active—with studies falling into each of the three categories in terms of their treatment of aid—but little consensus has emerged on methodology or findings (Wright 2009). One approach is long run cross-sectional analysis that examines the change in measures of democracy over decades. Knack (2004) finds aid as a share of GDP robustly insignificant in a global sample. In contrast, Goldsmith (2001a) and Heckelman (2010) find positive effects of aid per capita for Africa and the transition economies.

Panel studies focus on the short run impact of aid, examining either annual data or five year averages. Looking at Africa from 1975 to 1997, Goldsmith (2001b) uncovers a positive effect which Dunning (2004) demonstrates is driven entirely by the post-Cold War period. Using a range of measures and estimation methods with a global (though with a somewhat limited) sample of countries, Djankov *et al.* (2008) uncover a robust negative contemporaneous relationship between aid and domestic institutions. Dutta *et al.* (2013) report an amplification effect; aid makes autocracies more autocratic and democracies more democratic.

A number of other panel studies look at democratic transitions (roughly, large movements toward democracy). Foreshadowing the amplification effect of Dutta *et al.*, Wright (2009) finds that the impact of aid depends on the initial governance structure, with aid having a democratizing effect (stronger after the Cold War when credibility is greater) except in the case of military regimes where aid has the opposite impact. Bermeo (2011) examines the difference between aid from democracies (*i.e.*, DAC donors and the multilateral development banks (MDBs) they control) and aid from autocracies (*i.e.*, oil rich Middle East donors and the MDBs they control) in the post-Cold War period. Despite the relative paucity of democratic transitions in the data, Bermeo finds a positive link between aid from democratic donors and subsequent transitions to democracy in recipient countries but no such link for autocratic donors. Furthermore, aid from democracies only has an impact when the recipient does not also receive aid from an autocratic donor, implying that the impact of conditional aid (from democracies) is undercut by unconditional aid (from autocracies). Morrison (2009), Wright (2011), and Bermeo (2013) compare the impact of aid and other rents

(*e.g.*, oil revenue) on the probability of democratic transition. Morrison finds all rents have a similar negative impact on democracy. In Wright’s work examining times of economic crisis, aid is associated with a higher probability of democratic transition while oil rents are associated with a lower probability of democratic transition. Bermeo demonstrates that the impact of aid on democracy changed as donor motives changed at the end of the Cold War while the impact of oil did not change.

Several panel studies assume aid is not fully fungible and so focus on the aid given by donors specifically to promote democracy and improve governance. Scott and Steele (2005) use annual data on “democracy grants” from the U.S. National Endowment for Democracy (NED) during the 1990s to test whether these funds succeed in promoting democracy but their analysis finds no evidence that NED grants work. Finkel *et al.* (2007) broaden the scope to all U.S. bilateral aid obligations focused on democracy and governance in the post-Cold War period (1990-2003). Excluding countries already having good democracy ratings, the authors find a positive impact for U.S. Agency for International Development (USAID) democracy and governance assistance but not for other forms of aid. Kalyvitis and Vlachaki (2010) construct a democracy aid variable from DAC data for the period 1972-2004 for 59 countries that receive democracy aid. Again, more democracy aid is linked to a higher probably of political freedom though the impact of aid does not persist over time. Scott and Steele (2011) use USAID democracy aid disbursements for a panel of 108 developing countries from 1988 to 2001 and find a significant positive impact on democratization (in contrast to their earlier results for NED grants). Finally, Dietrich and Wright (2012) examine the separate effects of economic and governance aid on five different measures of democratic consolidation in Africa. They find a positive link only for governance aid.

[Table 1 about here]

In each of these studies, we can identify whether the focus is on aid as general input, aid as specific input, aid as incentive, or some combination thereof (see Table 1). Long run, cross-sectional studies (Goldsmith 2001a, Knack 2004, Heckelman 2010) aim to capture the impact of aid in all three roles but are more likely to pick up long run relationships (aid as general input) since the share of democracy aid is small and aid conditionality tends to be episodic. Short run panel studies using democracy or governance aid clearly focus on aid as specific input (Scott and Steele 2005, 2011, Finkel *et al.* 2007, Kalyvitis and Vlachaki 2010, Nielsen and Nielson 2010, Dietrich and Wright 2012). Short run panel analysis using all types of aid (Goldsmith 2001b, Dunning 2004, Djankov *et al.* 2008, Morrison 2009, Wright 2009, Bermeo 2011, 2013) present an interesting problem. The time horizon is too short to capture the impact of aid as general input while the broad definition of aid appears to be inconsistent with the notion of aid as specific input. The story

for incentive effects is also not clear as the aid flow is just prior to or contemporaneous with the measure of democracy (Dietrich and Wright 2012). Both Wright (2009) and Bermeo (2011) partly sidestep this problem by testing for additional implications of aid as incentive while in Djankov *et al.* (2008), Morrison (2009), and Bermeo (2013) the corrupting effects of aid may manifest themselves within a shorter period of time. More generally if aid is fully fungible, the broader definition of aid is appropriate to measure its impact as specific input. Finally, aid could play a stabilization role even in the short run and under some circumstances that could allow for democratic reform or prevent a shift toward autocracy.

Our review of these studies points to three important measurement issues. First, the limited time series variation in democracy indicators places a large premium on estimation approaches that use all available information. Previous studies based on Freedom House data have not used all available information because they do not model the rating process directly. Second, timing matters. Looking for future incentive effects of current aid is problematic; the timing is wrong. If this year's economic aid is linked to next year's democracy rating, it may simply reflect a link between current democracy and future democracy ratings. If democracy is a continuous variable, the donor may observe and respond to improvements that only later become large enough to trigger a change in the ratings.<sup>1</sup> In short, timing matters and any causal story needs to take timing into account. Third, it is not possible to identify the incentive effects of aid via instrumental variables. If the instruments are valid, they remove the impact of democracy on aid, the very conditional element of aid that could generate an incentive effect.

The analysis below follows up on each of these observations. The next section introduces an estimation method that more accurately models the Freedom House rating process in order to exploit all the information in the data. Subsequent estimations take into account the timing of aid flows and changes in democracy, looking at aid as general input, aid as specific input, and aid as incentive. Rather than looking at current aid or (clearly endogenous) future aid to identify incentive effects, we explore the response of recipient governments to expected future aid.

### 3 Estimation Method and Data

Our choice of estimation method (interval regression) is driven largely by the data generating process for the dependent variable, Freedom House ratings. The overall rating is an average of two subratings, Civil Liberties (*CL rating*) and Political Rights (*PR rating*). Each is reported on a seven point scale, with 1 denoting the fewest restrictions (the best rating) and 7 denoting the most restrictions (the worst rating). The *CL rating* is based on a more fine-grained Civil Liberties score (*CL score*) generated from responses to a

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<sup>1</sup>Goldsmith(2001a) notes that ratings based on expert judgment tend to lag behind actual changes and our own work reveals a bias against changing ratings.

check-list of 15 questions in four categories (Freedom of Expression & Belief; Associational & Organizational Rights; Rule of Law; and Personal Autonomy & Individual Rights). Answers to each question assign a score from 0 (less free) to 4 (more free) so that the *CL score* ranges from 0 to 60. The *PR rating* is based on a Political Rights score (*PR score*) generated from responses to a 10 question check-list so *PR score* ranges from 0 to 40. Each Freedom House rating corresponds to a range or interval of these scores; to make comparisons between scores and ratings more intuitive, we invert the ratings so that 1 corresponds to least free and 7 to most free. Country-level annual data are available for *CL rating* and *PR rating* from 1972 to 2011 while *CL score* and *PR score* data are publicly available only from 2005 to 2008 (Freedom House 2012, Armstrong 2011).

Our approach to working with these data is to treat *CL rating* and *PR rating* as interval data reflecting the underlying (generally unobserved) values of *CL score* and *PR score*. For expositional simplicity, we focus on Civil Liberties in what follows; the analysis is parallel for Political Rights or the average of the two, *FH score* (scaled to fall between 0 and 100).<sup>2</sup> The *CL score* intervals for each *CL rating* are known.<sup>3</sup> For example, a *CL rating* of 2 corresponds to a *CL score* in the interval 8 to 16. Figure 2 depicts this mapping. Note that for the years 2005 to 2008 the actual *CL score* values are available.

[Figure 2 about here]

To present the link between ratings and score intervals formally we define interval  $j$  for *CL score* as  $[a_j, b_j]$  for  $j = 1, 2, \dots, 7$  so that

$$CL\ rating_{it} = j \rightarrow CL\ score_{it} \in [a_j, b_j] \quad (1)$$

Based on these intervals, we can construct a second set of intervals for changes in civil liberties. For  $k = 1, 2, \dots, 7$ :

$$CL\ rating_{it} = j, CL\ rating_{it-1} = k \rightarrow \Delta CL\ score_{it} \in [a_j - b_k, b_j - a_k] \quad (2)$$

Assuming a linear model for changes in scores

$$\Delta CL\ score_{it} = \beta X_{it} + \varepsilon_{it} \quad (3)$$

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<sup>2</sup>Freedom House gives equal weight to civil liberties and political rights in its overall rating so  $FH\ score = 50 * (CL\ score/60 + PR\ score/40)$ .

<sup>3</sup>Based on data from Armstrong (2011), the ranges of *CL score* corresponding to the 7 ratings are 0-7, 8-16, 17-25, 26-34, 35-43, 44-52, 53-60 and for *PR score* 0-5, 6-11, 12-17, 18-23, 24-29, 30-35, 36-40. Note that in a handful of cases (12 of 768), ratings do not directly correspond to the underlying scores. These are Bhutan (2007), Brunei (2005), Iraq (2006-8), Sudan (2006-8), and United Arab Emirates (2005-8). Freedom House (2012) alludes to this possibility: “The ratings were compared to the previous year’s findings, and any major proposed numerical shifts or category changes were subjected to more intensive scrutiny. The scores [ratings] from the previous survey edition are used as a benchmark for the current year under review. In general, a score [rating] is changed only if there has been a real world development during the year that warrants a change.”

with  $\varepsilon_{it}$  independently distributed  $N(0, \sigma^2)$ , interval estimation via maximum likelihood yields consistent estimates of the parameters in (3) (Cameron and Trivedi 2005, 532-535). The resulting likelihood function can incorporate both point values (where  $\Delta CL$  score is observed—from 2006 to 2008) and intervals as defined above (where only  $CL$  ratings are observed).<sup>4</sup>

Although interval estimation usually takes the form of an ordered probit model with known cut-off points, our application can more usefully be thought of as a generalization of the Tobit model (Greene and Hensher 2008). With known cut-off points for ratings, we are able to examine *changes* in the underlying scores, an approach not possible in the standard ordered probit model with unknown cut-off points. Furthermore, the change in score intervals defined by (2) may overlap, a feature not compatible with an ordered probit model. The more natural interpretation of this interval estimation is as a two-sided Tobit with upper and lower bounds that vary by observation.

Much of the literature analyzing Freedom House ratings ignores the interval nature of these data, employing instead a linear regression approach that implicitly uses interval midpoints. Unfortunately, the least squares estimator is inconsistent and reported standard errors are incorrect in this setting. The slope coefficient estimates tend to be biased toward zero (attenuation bias) because, over some range, variation in the independent variables is not matched by variation in the dependent variable (*i.e.*, it is within-interval variation; see Stewart 1983, Fielding 1999, Greene and Hensher 2008).

### 3.1 Data

As described above, we derive the dependent variable from the Freedom House measures of Civil Liberties and Political Rights. The measure of DAC aid flows is total official gross disbursements from OECD (2013) divided by the GDP of the recipient country. It includes flows from all bilateral donors who are members of the DAC. Following Bermeo (2011), the short run panel analysis also considers aid from all multilateral organizations that report to the DAC and are controlled by DAC countries, which forms a separate variable. In addition, we consider aid given by autocratic regimes, which we define in the same way as Bermeo. For completeness, the short run analysis also includes another variable with information about aid from China.

Control variables include the log of PPP GDP per capita and the log of population (World Bank 2012, Heston *et al.* 2006). Another control variable is *War*, a dummy variable equal to 1 if there is a major on-going conflict with more than 1000 conflict-related deaths that year (Gleditsch *et al.*, 2002). Finally, panel specifications include year dummies to reduce the chance of spurious correlation driving results.

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<sup>4</sup>This approach also allows us to include narrower intervals where the starting point is a rating but the end point is a score (observations from the transition, 2004 to 2005) or vice versa (2008 to 2009). The estimator thus incorporates all available score/rating information. We implement the estimator via the *intreg* command in Stata.



## 4 Long Run Cross-Sectional Analysis

We estimate an interval regression model on cross-sectional data to determine the long run impact of bilateral aid from DAC donors on democracy. We examine different samples to compare results with Knack (2004)—either Knack’s time period (1975-2000) or the full time period (1972-2011) and either Knack’s country coverage or all countries with sufficient data. One complication is different lengths of coverage for some countries over the period (when countries come into existence, cease to exist, or data are missing); failing to account for this implicitly compares impacts over very different lengths of time. Time period heterogeneity is particularly problematic since we use the average aid flow over the period which does not distinguish between aid at 1% of GDP for one year or for 30 years. The simplest solution is to impose some homogeneity by restricting the sample to only those countries where data are available over a long period. We thus require that each country have data for at least 20 years of the 1975-2000 period. Alternate solutions to time period heterogeneity (using total rather than average aid or including the time span covered as an additional covariate) yield very similar results while ignoring heterogeneity entirely does impact some specifications.

[Table 2 about here]

The estimation sample described in Table 2 covers 122 countries (all available countries with at least 20 years between 1975 and 2000; countries listed in appendix). The figures given in Table 2 draw on data from the 1972 to 2011 period. As explained above, the dependent variable is an interval rather than a point; summary statistics report midpoint values.  $\Delta FH$  score averages 14.3, ranging from a low of -52.5 (Gambia) to a high of 81.5 (Bulgaria).  $\Delta FH$  rating shows the corresponding changes in rating. Over the long time periods considered in the cross-sectional analysis, the correlation of changes in civil liberties and political rights with changes in the overall Freedom House score is high (0.93 and 0.95).

Bilateral aid from DAC donors (*DAC aid*) averages 5.2% of recipient GDP, ranging from only 0.056% of GDP (Kuwait) to nearly 32% of GDP (Kiribati). The initial Freedom House score (*Initial FH score*) is also reported in terms of midpoints. The mean of 42.3 corresponds to an initial Freedom House rating of 3.5 while the minimum (6) and maximum (94.6) of the score correspond to the extremes of the standard rating (1 and 7).<sup>5</sup> The log of initial PPP GDP per capita averages 7.9 (\$2,650 in year 2000 dollars), ranging from a low of 6 (\$235 for Bhutan) to a high of 11.2 (\$73,000 for Qatar). Real GDP growth rate over the period averages 1.8%, ranging from -2.7% to +10.1%.

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<sup>5</sup>The slight asymmetry in the minimum and maximum *FH score* reflects slight asymmetries in Freedom House’s mapping from scores to ratings. See footnote above.

## 4.1 Cross-Section Estimation Results

Table 3 presents estimation results for a specification largely modeled on Knack (2004).<sup>6</sup> Column 1 restricts the data to countries and years covered by Knack though our sample is smaller by 17 countries as we still require at least 20 years of data for a country to be included. Despite these differences and different estimation methods, results in column 1 regarding aid and Freedom House rankings are consistent with Knack’s findings. There is a strong, negative relationship between the initial level of democracy and subsequent changes in democracy but no apparent relationship between aid and changes in democracy.

[Table 3 about here]

The rest of Table 3 demonstrates that the latter result only holds for Knack’s original sample. Column 2 expands the set of countries to all those with at least 20 years of data. This adds 37 countries excluded from Knack’s analysis (presumably due to missing illiteracy data). The estimated coefficient for aid increases and is now statistically significant. Column 3 expands the time period to all available years but re-imposes Knack’s country list. The estimated coefficient for aid is again positive and significant. Finally, Column 4 includes all countries that qualify and the longer time period, finding again a strong, statistically significant positive relationship between aid flows and democratization. In concrete terms, the impact is non-trivial but modest. An increase in DAC aid by one standard deviation (5.47 percent of recipient GDP, equivalent to doubling the mean level) sustained over a 20 to 25 year period is linked to an increase of about 7 points in the Freedom House score, roughly half a rating on the standard 1-to-7 scale.

## 4.2 Endogeneity Tests

Naturally, the above results are subject to concerns over endogeneity. If donors have a preference for democracies, the estimated link between aid and democracy could simply reflect aid following democratization, even in the absence of incentive effects. Table 4 addresses this concern with increasingly demanding tests. Columns 1 through 3 follow Knack’s approach. Columns 1 and 2 use variables suggested by Knack (initial population, initial rate of infant mortality, and colonial heritage dummies) in a first stage to instrument aid which is used in the second stage regression.<sup>7</sup> In both cases, the estimated coefficient is essentially unchanged and continues to be statistically significant at the 10 percent level. In addition, standard Durbin and Wu-Hausman endogeneity tests fail to reject the null hypothesis of exogeneity at all standard significance

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<sup>6</sup>We do not include initial illiteracy as publicly available data are now sparse, likely due to improved quality control standards. In any event, the illiteracy variable is uniformly insignificant in Knack (2004).

<sup>7</sup>The estimation in column 1 uses an interval regression at the second stage while the estimation in column 2 is a standard IV estimation.

levels. Column 3 uses early aid, the average DAC aid flow between 1972 and 1990, to explain the change in Freedom House scores between 1972 and 2011. The estimated coefficient is again positive and significant with only a slight reduction in magnitude relative to aid over the entire period.

Column 4 takes this approach one step further: Aid is again averaged over the 1972 to 1990 period but all other variables are based on the subsequent period. The initial Freedom House score is now based on the 1991 rating, initial GDP is the 1991 value, and GDP growth is averaged over the 1991 to 2011 period. Most importantly, the dependent variable is now the change in the Freedom House score from 1991 to 2011. This approach precludes measuring incentive effects (since aid is strictly before reform) but, by the same token, avoids the reverse causation critique. It is a hard test of aid as general input because: 1) it requires that the effects of aid on future democratization persist over long periods of time; 2) it includes transition economies that, for well-known structural reasons, received little DAC aid during the initial period but made dramatic strides following the fall of the Soviet Union in the subsequent period; and 3) it examines the impact of much-maligned Cold War aid (Bearce and Tirone 2010, Morrison 2013). For all these reasons, the estimate can be considered a lower bound. Although the coefficient estimate is reduced by a third, it remains statistically significant and non-trivial in magnitude.

[Table 4 about here]

### 4.3 Robustness Checks

Table 5 presents robustness checks of the long-run result. Columns 1 and 2 explore alternative measures of democracy, replacing the overall Freedom House variables ( $\Delta FH$  score and *Initial FH score*) with their constituent parts, Civil Liberties ( $\Delta CL$  score and *Initial CL score*) and Political Rights ( $\Delta PR$  score and *Initial PR score*). The estimated coefficient on *DAC aid* is about half the previous magnitude (as the range of the dependent variable is also halved) but statistically significant in both cases, demonstrating that the link between aid and democracy is not driven just by one component. Column 3 investigates whether the results are robust to a different measure of aid intensity. Following Goldsmith (2001a) and Heckelman (2010), we now measure aid in per capita terms rather than as a share of recipient GDP. Aid again enters with a positive and significant coefficient. Considering the magnitude of the coefficient and one standard deviation in aid per capita, the size of the estimated effect is consistent with previous estimates.

Column 4 uses an ordered probit to relax an assumption implicit in the interval regression approach. As discussed earlier, using the change in an index as the dependent variable is inconsistent with the standard ordered probit model. However, an ordered probit (or logit) approach can be justified under a different set

of assumptions. If applied to an estimation in terms of changes in a discrete variable, the ordered probit assumes evenly spaced cut points in terms of levels but allows the estimated model to locate the cut points of changes.<sup>8</sup> This may seem arbitrary (assuming a rigid cardinal structure for levels but only an ordinal structure for changes in levels) but may be justified if, for example, Freedom House experts are hesitant to change ratings. In that case, the range of changes in the latent score variable that maps into changes in the observed rating may be larger for 0 (no rating change) than for other rating changes. Because of the nature of the ordered probit model, the coefficient estimates in Column 4 are much smaller but the statistical significance of the link between aid and democracy is robust to this alternate assumption.

Our final robustness check again examines the link between scores and democracy in terms of a latent variable model (LVM). Consider the Civil Liberties variable. The format of the surveys generating *CL score* does not allow values to exceed 60 or drop below 0. However, it is conceivable that further movements toward or away from democracy occur after these limits are reached. In other words, the relevant intervals for *CL score* corresponding to *CL ratings* of 1 and 7 can be considered to be  $(-\infty, 7]$  and  $[53, \infty)$ , respectively. In that case, the interval containing underlying score changes for countries that maintain a *CL rating* of 1 or 7 would be  $(-\infty, \infty)$ ; those observations thus contain no information for our estimation. For countries that move from or to a rating of 1 or 7 the interval would have one well-defined boundary. For example, a move from a *CL rating* of 6 (*CL score* interval  $[44, 52]$ ) to 7 (*CL score* interval  $[53, \infty)$ ) would generate an interval for  $\Delta CL score$  of  $[1, \infty)$ . A parallel critique applies to the Political Rights rating. Column 5 takes this into account by adjusting intervals accordingly. Ten countries drop from the sample because their *FH score* interval becomes  $(-\infty, \infty)$  but the estimation results are essentially unchanged.<sup>9</sup>

Taking into account the evidence from Tables 3 to 5, the estimated link between DAC aid and democratization does not appear to be driven by endogeneity or our choice of specification but rather to reflect a positive long run impact of aid on democracy.

[Table 5 about here]

## 5 Short Run Panel Analysis

As discussed in Section 3, short run panel analysis presents clear challenges regarding questions of timing and endogeneity. As such, results presented in this section must be interpreted with some degree of cau-

<sup>8</sup>The assumption of evenly spaced level cut points is required to treat, for example, a change from 2 to 3 the same as a change from 5 to 6. This assumption does not quite hold throughout the range of our data (the interval size for 1 and 7 is different than for 2 through 6) but it is closely approximated. Note that the ordered probit is less efficient since it estimates "known" parameters and—for the panel analysis in the next section—cannot incorporate actual score values when they are observed (between 2005 and 2008).

<sup>9</sup>Results are also robust to dropping individual countries or regions.

tion. Nonetheless, a short run analysis does allow investigation of a number of important issues including the impact of aid as specific input and as incentive, *i.e.*, whether conditional aid is effective at promoting democratic reform. As noted above in Section 2, it is not possible to identify the incentive effects of aid via instrumental variables because, if the instruments are valid, they remove the impact of democracy on aid, *i.e.*, they remove the incentive effect. Instead, to identify an incentive effect we need to find conditions under which the expected benefit of movement toward democracy (in terms of aid gained) or equivalently the expected cost of movement away from democracy (in terms of aid lost) varies. To explore this, we start by estimating aid allocation equations for individual donors and then use differences between donors' responses to democratization to construct a variable that plausibly captures variation in the recipient country's expected return to democratization. This is one of the explanatory variables in the annual-level equation for change in democracy.

Our aid allocation estimation examines the geographic distribution of aid between recipient countries, including separate equations for bilateral aid from each of the G7 donors, multilateral aid from agencies controlled by the G7, and autocratic aid as identified by Bermeo (2011).<sup>10</sup> The dependent variable is the log of total official gross disbursements of aid over recipient GDP; we first add \$1000 to aid to avoid log of zero. All specifications include year dummies and recipient country fixed effects (as well as clustering standard errors by recipient country). Control variables include log of PPP GDP per capita (*GDP per capita*) and log of population (*Population*) as well as the *War* dummy which indicates more than 1000 conflict-related deaths in the year. The key explanatory variables are the Freedom House score in the previous year (*FH score*<sub>*t*-1</sub>) as a measure of democracy and a dummy variable indicating whether the country received significant U.S. military aid in the previous year (*US military aid*<sub>*t*-1</sub>) to capture the country's geopolitical importance.<sup>11</sup> To explore differences in the response to democracy, we interact *FH score*<sub>*t*-1</sub> and *US military aid*<sub>*t*-1</sub>. This allows for the possibility that for some donors geopolitics may trump democracy, *i.e.*, that aid depends less on democracy when a recipient country is geopolitically important.

[Table 6 about here]

Table 6 presents results using panel data for 156 recipient countries from 1974 to 2011 (5090 obser-

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<sup>10</sup>Autocratic aid includes bilateral aid from Saudi Arabia, Kuwait and the United Arab Emirates as well as multilateral aid from the Arab Fund (AFESD), the Arab Bank for Economic Development in Africa (BADEA), the Islamic Development Bank, and the OPEC Fund for International Development (OFID). These are a subset of the non-democratic international organizations as defined by Libman and Obydenkova (2013).

<sup>11</sup>*FH score* reflects the interval midpoint when precise scores are not observed. We define significant U.S. military aid as more than \$2 million in real terms using 2011 dollars. Results are not sensitive to the threshold, e.g., lowering the amount to \$1 million or raising it to \$3 million.

vations).<sup>12</sup> The negative coefficient estimates for *GDP per capita* are consistent with donors responding to recipient need (allocating more aid when a country’s income is lower). Interpretation of the negative coefficients on *Population* is not straightforward since population generally increases steadily over time and other elements of the estimated equation (country fixed effects and year dummies) already account for persistent differences between countries and trends across countries over time. With country fixed effects and year dummies, the residual variation in population might capture the impact of catastrophic events such as man-made or natural disasters. The war dummy enters negatively, indicating that conflict generally hampers the distribution of development aid. All donors except France provide significantly more aid to a country when it receives substantial military aid from the U.S., *i.e.*, when it is strategically important to the United States and its allies.

Turning to the key aspects of Table 6, we find that all donors reward democratization. When a country’s Freedom House score is above the country’s norm, the country receives significantly more than normal levels of aid from all donors (though the effect is smallest and only marginally significant for autocratic donors). For some donors—Canada, Italy, Japan, the U.K., and the U.S.—this effect is conditional on the recipient country’s geopolitical importance. For these *conditional donors*, the estimated coefficient for the interaction term ( $FH\ score_{t-1} \times US\ military\ aid_{t-1}$ ) is negative and significant. In the case of Canada, Italy, the U.K. and the U.S., the impact of democracy on aid flows is statistically insignificant when countries are geopolitically important; for Japan, the role of democracy is reduced but not eliminated. For the remaining donors—France, Germany, autocratic donors, and multilateral aid organizations—the role of democracy in aid allocation is not conditional on geopolitical importance as measured by U.S. military aid. For these *unconditional donors*, a reduction in the Freedom House score is associated with a reduction in aid regardless of whether the country received significant U.S. military aid or not. We return to this finding shortly, looking at the percentages of aid each country typically receives from conditional versus unconditional donors.

We turn now to the short run impact of aid on democracy. Table 7 provides descriptive statistics. The sample for the short run analysis differs substantially from that of the long run analysis for several reasons. First, the long run analysis required a minimum of 20 years of data so that averaged variables would be comparable between countries; this restriction is no longer necessary or useful and the sample expands to 156 countries. Second, our short run analysis differentiates between aid from different donor groups (G7, multilateral, autocratic, China). Because of substantial unmeasured aid from the Soviet Union during the 1970s and early 1980s, we limit the time period to 1985-2011 when Soviet aid was no longer a

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<sup>12</sup>Data availability (chiefly Freedom House ratings) determines the sample. We exclude Barbados and Somalia because they drop from the subsequent democracy estimation due to lack of meaningful data on change in democracy.

significant factor. Finally as compared to the long run analysis, the annual analysis has many more cases—928 cases—where either the Civil Liberties or Political Rights rating is unchanged at 1 or 7. Following the latent variable model approach of Table 5, Column 5, we adjust intervals to reflect the limits of the score range. In addition to dropping the 928 cases with no meaningful change-in-score information (*i.e.*, intervals that run from  $-\infty$  to  $\infty$ ), this approach generates 245 one-sided intervals which are easily accommodated by the interval regression. Combining these restrictions with data availability limitations results in a final sample of 2886 annual observations.

[Table 7 about here]

The dependent variable ( $\Delta FH$  score) is an interval for the annual change in the Freedom House score; descriptive statistics again report the midpoint of the underlying interval. This averages 0.6 (reflecting the spread of democracy over the period) and ranges from -67.5 (Gambia 1994) to 52.5 (Panama 1990).<sup>13</sup> Mirroring the long run analysis, we include the initial Freedom House score (midpoint).  $DAC\ aid_{t-1}$  reflects total official gross disbursements of bilateral aid from all DAC members (the G7 countries investigated above account for the vast majority of this aid) lagged one year and averages 5.9% of recipient GDP, ranging from no aid to 153% of GDP (Democratic Republic of the Congo 2003).  $Multilateral\ aid_{t-1}$  includes lagged total official gross disbursements from all multilateral aid agencies controlled by DAC countries (as in Table 6) and averages about 4% of recipient GDP, ranging as high as 146% of GDP (Sao Tome and Principe 2007).  $Autocratic\ aid_{t-1}$  again follows Bermeo’s definition and averages just under 0.3% of recipient GDP with a maximum of 23% of GDP (Djibouti 1990).

To capture all major sources of foreign aid, we also include a dummy variable indicating whether the country was eligible for Chinese aid. Data on aid from China are not available from the OECD database so we rely on two other sources. Rich (2009) provides panel data on diplomatic recognition of Taiwan, a decision that has generally made countries ineligible for Chinese aid (Bräutigam 2009, Zhang 2011). In addition, Dreher and Fuchs (2011) compile data on Chinese aid projects from Bartke (1989) and Chinese Commerce Yearbooks (Ministry of Commerce 1984-2007). Dreher and Fuchs report the number of projects funded by China in each recipient country for five periods (1956-1969, 1970-1978, 1979-1988, 1989-1995, 1996-2005). We use these two data sources to construct a Chinese aid dummy, equal to 0 if a country was ineligible for Chinese aid and equal to 1 otherwise. Eighty-three percent of the sample are cases where countries are eligible for aid from China.

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<sup>13</sup>For simplicity, the reported midpoint is calculated before imposing the restrictions of the latent variable model, *i.e.*, before defining the extremes of the latent  $CL$  score and  $PR$  score as  $\pm\infty$ . Note that for 2006 to 2008,  $\Delta FH$  score is known precisely because we have the underlying score values from Armstrong (2011). For 2005 and 2009, we again have interval data but with a narrower range since one of the two  $FH$  score values is known precisely.

*US military aid*<sub>*t-1*</sub> is defined as above in Table 6; in about 30% of the cases, countries received substantial U.S. military aid in the previous year. The remaining control variables are log of PPP GDP per capita, log of population, and the conflict dummy. All specifications include unreported year dummies and base z-statistics on country-clustered standard errors.

[Table 8 about here]

Across all specifications, the initial Freedom House rating is highly significant with a coefficient estimate of -0.08 indicating that countries which start with higher scores are more likely to see a decrease. The initial Freedom House score is an interval midpoint but has not been adjusted for the open-ended limits of the latent variable model. Doing so reduces the sample and shrinks the estimated coefficient to -0.04, but it continues to be statistically significant. Key results are not sensitive to whether the initial rating is dropped or adjusted as above.

The first specification (Column 1) includes aid from each donor group, intended to capture the impact of aid as a specific input or as a (de)stabilizing factor. All aid variables are lagged one year to reduce the potential for reverse causation. *DAC aid* enters with a very small but positive and significant coefficient, indicating that an increase in bilateral aid from DAC donors of one percent of recipient GDP is associated with about a 0.06 point increase in Freedom House score (on a 100 point scale). An increase in aid by one standard deviation implies about a 0.5 point increase in the democracy score. *Multilateral aid* enters with a positive but even smaller and statistically insignificant coefficient. There is a statistically significant but negative link between democracy and aid from autocratic donors (other than China) and the multilateral agencies they control. An increase in *Autocratic aid* of one percent of recipient GDP (which also happens to be one standard deviation) is associated with a decrease in democracy of slightly less than 0.5 points. Finally, eligibility for aid from China is associated with a 1.5 point decrease in the Freedom House score from one year to the next. These results are consistent with Bermeo (2011) where aid from democratic donors increases the probability of a democratic transition while aid from autocratic donors decreases the probability. The finding for Chinese aid also provides some support for Naím's (2007) claim that China is a rogue donor, in contrast to Dreher and Fuchs (2011).

We now turn to a more direct examination of the role of incentives. As demonstrated in the aid allocation equations (Table 6), strategic importance can impact a country's expected return to democratizing (or the expected cost of restricting civil liberties and political rights). This suggests that, if aid has an incentive effect, strategically important countries should be less likely to democratize because the reward for doing so (or the punishment for not doing so) is smaller. To investigate this, Column 1 includes the *US*



*military aid* dummy to capture a country’s geopolitical importance to the U.S. and its allies. Consistent with this argument, *US military aid* enters the estimation with a negative coefficient, significant at the 90% confidence level. For the thirty percent of country-years with significant U.S. military aid, *FH score* is predicted to drop by 0.8 points.

One can, of course, imagine other reasons why geopolitical importance might be linked with movement away from democracy. To buttress our aid-as-incentive explanation, we introduce a new geopolitical variable more tightly linked to the story of conditional aid. Recall that the aid allocation equations in Table 6 found that Canada, Italy, Japan, the U.K., and the U.S. are conditional donors that vary the weight given to democracy in their aid allocation decisions based on the geopolitical importance of the recipient country. In contrast, France, Germany, autocratic donors, and multilateral aid organizations are unconditional donors that do not vary the weight given to democracy in their aid allocation decisions based on geopolitics (as measured by U.S. military aid). Thus, if geopolitical importance operates through the hypothesized incentive mechanism, it should only hold for countries that typically receive a substantial portion of their aid from the conditional donors. Column 2 investigates this by replacing *US military aid* dummy variable with a version of the dummy variable weighted by the percentage of the recipient country’s aid from the conditional donors over a four year period. The estimated coefficient on this weighted variable is four times as large and significant at the 95% confidence level. Column 3 pushes this one step further, adding the complementary variable weighted by the percent of the recipient country’s aid from unconditional donors over the same period. Consistent with the incentives story, the estimated coefficient for unconditional donors is small and statistically insignificant.

Read in reverse, these estimates imply a large incentive effect. If a recipient country is geopolitically important and receives all its aid from conditional donors—the case where there is no incentive effect—its Freedom House score is predicted to drop by nearly 4 points (in a single year) relative to a country that does face an incentive effect. Turning this around, the incentive effect of additional future aid as a reward for democratization (or the loss of future aid as punishment for democratic reversals) is nearly 4 points per year. Comparing this effect to other results suggests that the impact of aid as incentive—when it exists—is substantially larger than aid as general input or aid as specific input.

## 6 Conclusion

This paper reexamines the link between development aid and democratization in aid-receiving countries. In principle, aid can promote democratic reform over the long run by helping to bring about the necessary

preconditions (aid as general input), over the short run by supporting competitive elections (aid as specific input), or through leverage and conditionality (aid as incentive). Thus, timing matters when considering the causal pathways through which aid might influence democracy. To explore these pathways, we apply an interval regression that exploits knowledge about the Freedom House rating process to make full use of the information available in these democracy ratings. We also take advantage of the great length and breadth of data available.

Our long run cross-sectional analysis finds a positive and statistically significant though modest effect of aid on democracy when examining data for 122 countries between 1972 and 2011. These results persist in estimations using instrumental variables or long lags in aid, making a convincing case that the direction of causation is from aid to democracy. The long run analysis is best suited to identifying the impact of aid as a general input. Using panel data to examine the short run impact of aid on democracy (aid as specific input) uncovers a positive relationship for bilateral aid from DAC donors but not for aid from multilateral agencies, autocratic Arab donors, or China. The positive impact of DAC aid is small while aid from Arab donors and China is associated with subsequent decreases in the level of recipient country democracy.

A clear challenge in the aid and democracy literature is to identify the impact of conditionality or selectivity, that is, the incentive effects of aid. Because donors may have a preference for democracy, independent of any impact of aid on democracy, identifying incentive effects is far from straightforward. The standard approach of using instrumental variables to identify exogenous variation cannot identify such incentive effects as they are, by definition, driven only by that part of aid which is endogenous, *i.e.*, that part of aid which recipients believe is conditional on reform. This paper takes a different approach. We first present evidence that while donors generally provide more aid to a country when its level of democracy is higher, geopolitical considerations trump concern for democracy for certain "conditional" donors. We use variation in geopolitical importance of recipient countries and in aid shares from these conditional donors to identify cases where recipients do not expect future aid to depend on democratic reforms. In such cases, we observe sizeable reductions in the recipient country's level of democracy. This implies that aid can have a substantial incentive effect when recipients do expect future aid to be conditional on democratic reform.

Overall, the evidence provides some reasons for guarded optimism about the potential impact of aid on democracy. In its role as an input to the democratization process, bilateral aid from the traditional DAC donors has a small but robustly positive impact. In cases where geopolitics do not override other considerations, expected future aid that is conditional on democratic reforms can have a substantial impact.

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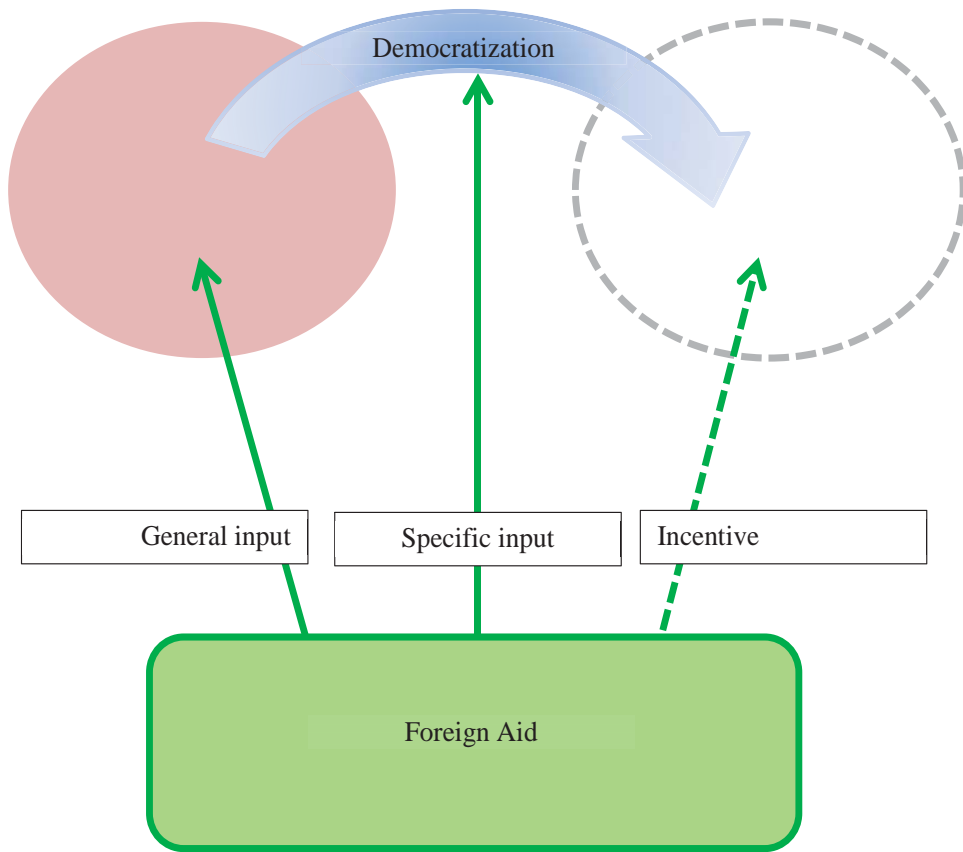


Figure 1: Roles of Aid in Democratization

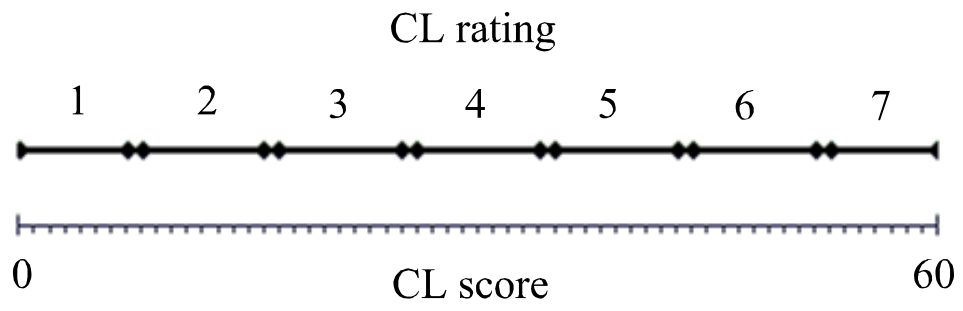


Figure 2: Mapping from rating to score





Table 2: Cross-Sectional Descriptive Statistics

Variable	Mean	StDev	Min	Max	Description
$\Delta FH$ score	14.32	26.92	-52.5	81.4	Midpoint of $\Delta FH$ score interval over period
$\Delta FH$ rating	0.96	1.81	-3.5	5.5	Overall change in $FH$ rating over period
$DAC$ aid	5.205	5.47	0.056	31.67	Period average of aid over GDP
Initial $FH$ score	42.26	26.87	6.042	94.58	Midpoint of $FH$ score interval, initial year
Initial $GDP$ per capita	7.932	0.9976	5.987	11.2	PPP $GDP$ per capita in year 2000 USD (log), initial year
$GDP$ growth	1.828	1.893	-2.715	10.08	Period average of growth in real $GDP$ per capita
Observations	122				

Sample only includes countries with at least 20 years of coverage in the 1975-2000 period.

Table 3: Long Run Cross-Sectional Analysis

	1975-2000	1975-2000	1972-2011	1972-2011
	Knack countries	All countries	Knack countries	All countries
<i>DAC aid</i>	0.731 (1.53)	0.934** (2.86)	1.313** (2.10)	1.286** (3.26)
<i>Initial FH score</i>	-0.862** (-6.78)	-0.667** (-5.62)	-0.882** (-8.82)	-0.674** (-6.85)
<i>Initial GDP per capita</i>	11.56** (2.33)	3.122 (1.08)	14.18** (3.28)	3.154 (1.11)
<i>GDP growth</i>	1.181 (0.66)	1.685 (1.40)	2.455 (1.03)	2.876 (1.44)
<i>Latin America &amp; Caribbean</i>	9.740 (1.18)	19.00** (3.05)	8.102 (0.97)	17.90** (2.76)
<i>Middle East &amp; North Africa</i>	-18.24* (-1.85)	-13.69* (-1.72)	-16.76* (-1.67)	-10.08 (-1.35)
<i>Sub-Saharan Africa</i>	-4.022 (-0.46)	-1.672 (-0.25)	-2.605 (-0.26)	-1.064 (-0.14)
<i>Europe &amp; Central Asia</i>	11.84 (1.04)	27.94** (2.58)	20.30** (2.30)	36.49** (4.42)
<i>South Asia</i>	0.815 (0.08)	-6.199 (-0.66)	4.170 (0.49)	-6.335 (-0.82)
Observations	88	122	88	122

z statistics in parentheses based on robust standard errors; \* p<.1, \*\* p<.05.

Interval Regression using  $\Delta$ Freedom House score.

Table 4: Long Run Cross-Sectional Analysis, Endogeneity Tests

	2 step IntReg	2SLS	Early Aid	Previous Aid
<i>DAC aid</i>	1.086* (1.66)	1.053* (1.76)	1.166** (3.72)	0.710** (2.88)
<i>Initial FH score</i>	-0.734** (-6.69)	-0.738** (-7.06)	-0.726** (-6.86)	-0.425** (-4.96)
<i>Initial GDP per capita</i>	6.107** (2.07)	6.045** (2.30)	2.429 (0.91)	1.464 (0.66)
<i>GDP growth</i>	4.929** (3.41)	4.876** (3.82)	2.204 (1.23)	1.286 (1.04)
<i>Latin America &amp; Caribbean</i>	17.52** (2.22)	17.40** (2.36)	18.73** (2.80)	10.83** (2.09)
<i>Middle East &amp; North Africa</i>	-15.18 (-1.62)	-15.51* (-1.72)	-10.16 (-1.30)	-6.350 (-1.12)
<i>Sub-Saharan Africa</i>	4.299 (0.54)	4.271 (0.56)	-1.576 (-0.21)	1.908 (0.34)
<i>Europe &amp; Central Asia</i>	35.93** (3.16)	35.53** (3.28)	37.06** (4.41)	21.31** (3.70)
<i>South Asia</i>	-5.860 (-0.72)	-5.968 (-0.73)	-4.556 (-0.60)	0.395 (0.07)
Observations	111	111	122	122

z statistics in parentheses based on robust standard errors; \* p<.1, \*\* p<.05.

Dependent variable is  $\Delta$ Freedom House score.

Instruments for Columns 1 & 2 follow Knack(2000):

Initial population, initial rate of infant mortality, colonial heritage dummies

Table 5: Long Run Cross-Sectional Analysis, Robustness Checks

	Civil Liberties	Political Rights	Aid per capita	Ordered Probit	LVM
<i>DAC aid</i>	0.690** (3.06)	0.586** (3.40)	0.110** (3.34)	0.0712** (3.06)	1.216** (2.61)
<i>Initial score/rating</i>	-0.670** (-6.96)	-0.726** (-8.31)	-0.700** (-6.99)	-0.538** (-5.63)	-0.993** (-8.93)
<i>Initial GDP per capita</i>	1.952 (1.23)	1.446 (1.13)	-0.823 (-0.31)	0.156 (0.99)	0.655 (0.19)
<i>GDP growth</i>	1.671 (1.42)	1.233 (1.46)	1.473 (0.88)	0.159 (1.34)	2.976 (1.12)
<i>Latin America &amp; Caribbean</i>	7.637** (2.23)	9.525** (3.15)	15.84** (2.62)	1.013** (2.65)	21.10** (3.47)
<i>Middle East &amp; North Africa</i>	-7.251* (-1.80)	-3.619 (-1.02)	-12.66* (-1.74)	-0.495 (-1.24)	-16.52* (-1.94)
<i>Sub-Saharan Africa</i>	-1.316 (-0.31)	-0.186 (-0.05)	-1.167 (-0.16)	-0.0322 (-0.08)	-7.372 (-0.86)
<i>Europe &amp; Central Asia</i>	20.13** (4.60)	15.42** (4.14)	36.29** (4.36)	1.966** (4.09)	40.86** (3.34)
<i>South Asia</i>	-6.359* (-1.65)	-0.382 (-0.10)	-6.305 (-0.85)	-0.350 (-0.82)	-12.00 (-1.58)
Observations	122	122	122	122	112

z statistics in parentheses based on robust standard errors; \* p<.1, \*\* p<.05.

Column 1: Interval Regression using  $\Delta$ Freedom House Civil Liberties score.

Column 2: Interval Regression using  $\Delta$ Freedom House Political Rights score.

Column 3: Interval Regression using  $\Delta$ Freedom House score; aid per capita.

Column 4: Ordered Probit using  $\Delta$ Freedom House rating.

Column 5: Interval Regression treating Freedom House score as latent variable.

Table 6: Short Run Panel, Aid Allocation by Donor

	Canada	France	Germany	Italy	Japan	U.K.	U.S.	Autocrats	Multilateral
<i>US military aid</i>	1.512** (3.57)	0.462 (1.32)	1.199** (3.98)	1.666** (4.18)	1.603** (3.92)	1.903** (4.43)	2.656** (5.28)	1.237** (2.70)	0.785* (1.93)
<i>FH score<sub>t-1</sub></i>	0.0211** (3.28)	0.0257** (3.82)	0.0267** (3.61)	0.0198** (3.10)	0.0273** (4.22)	0.0245** (3.02)	0.0322** (3.78)	0.0116* (1.73)	0.0259** (3.35)
$\times$ <i>US military aid</i>	-0.0140** (-2.08)	-0.00162 (-0.28)	-0.00638 (-1.24)	-0.0196** (-3.06)	-0.0130** (-2.14)	-0.0176** (-2.67)	-0.0280** (-3.71)	-0.0109 (-1.62)	0.000769 (0.11)
<i>GDP per capita</i>	-1.414** (-3.21)	-0.980** (-2.96)	-1.131** (-2.66)	-1.635** (-4.43)	-0.517 (-1.39)	-2.288** (-4.18)	-1.807** (-3.49)	-1.038** (-2.21)	-1.511** (-3.62)
<i>Population</i>	-1.041 (-1.18)	-2.660** (-3.10)	-2.204** (-2.52)	-1.678** (-2.53)	-1.670* (-1.80)	-2.010** (-2.07)	-1.421 (-1.60)	-0.829 (-0.67)	-2.831** (-2.39)
<i>War</i>	-0.494* (-1.90)	-0.366 (-1.38)	-0.785** (-3.98)	-0.178 (-0.61)	-1.069** (-2.78)	-1.005** (-3.53)	-0.802* (-1.82)	-0.763** (-2.06)	-0.622** (-2.18)
Countries	156	156	156	156	156	156	156	156	156
Observations	5090	5090	5090	5090	5090	5090	5090	5090	5090

t statistics in parentheses based on country-clustered standard errors.

All specifications include unreported country fixed effects and year dummies.

Dependent variable is log of total official gross disbursements over GDP with \$1000 added to each aid variable to avoid log of zero.

Estimation based on all available data: 1973-2011.

Table 7: Short Run Panel Summary Statistics

Variable	Mean	StDev	Min	Max	Description
$\Delta FH$ score	0.634	7.247	-67.5	52.5	Annual change in the Freedom House score (midpoint)
$FH$ score $_{t-1}$	50.05	21.8	4.167	97.08	Freedom House score (midpoint)
$DAC$ aid $_{t-1}$	5.944	9.034	0	152.8	DAC bilateral total official gross disbursements (% of recipient GDP)
$Multilateral$ Aid $_{t-1}$	3.988	6.671	0	146.2	DAC controlled multilateral total official gross disbursements (% of recipient GDP)
$Autocratic$ Aid $_{t-1}$	0.281	0.995	0	23.45	Autocratic donor total official gross disbursements (% of recipient GDP)
$Chinese$ Aid $_{t-1}$	0.828	0.378	0	1	=1 if eligible for Chinese aid (does not recognize Taiwan)
$US$ Military Aid $_{t-1}$	0.297	0.457	0	1	=1 if U.S. military aid > \$2 million in constant 2011 USD
$Conditional$ Donors	0.093	0.176	0	0.936	US military dummy weighted by recipient aid share from conditional donors
$Unconditional$ Donors	0.204	0.329	0	0.998	US military dummy weighted by recipient aid share from unconditional donors
$GDP$ per capita	8.089	0.967	5.144	10.53	PPP GDP per capita in year 2000 USD (log)
$Population$	15.63	1.935	9.916	21.01	Population (log)
$War$	0.052	0.221	0	1	=1 if > 1000 conflict deaths
Observations	2,886				Number of observations
Countries	156				Number of countries
Years	1999		1985	2011	Coverage in Table 8

Table 8: Short Run Panel, Change in Democracy

	(1)	(2)	(3)
<i>FH score</i> <sub><i>t</i>-1</sub>	-0.0812** (-7.91)	-0.0815** (-7.92)	-0.0819** (-7.93)
<i>DAC aid</i> <sub><i>t</i>-1</sub>	0.0562** (2.90)	0.0619** (3.21)	0.0634** (3.36)
<i>Multilateral aid</i> <sub><i>t</i>-1</sub>	0.0182 (1.29)	0.0161 (1.14)	0.0157 (1.11)
<i>Autocratic aid</i> <sub><i>t</i>-1</sub>	-0.425** (-2.11)	-0.431** (-2.08)	-0.448** (-2.15)
<i>Chinese aid</i> <sub><i>t</i>-1</sub>	-1.462** (-2.54)	-1.472** (-2.50)	-1.472** (-2.49)
<i>US military aid</i> <sub><i>t</i>-1</sub>	-0.788* (-1.71)		
<i>weighted by conditional donor</i>		-3.281** (-2.74)	-3.887** (-2.99)
<i>weighted by unconditional donor</i>			0.590 (0.89)
<i>GDP per capita</i>	0.462 (1.61)	0.495* (1.76)	0.479* (1.68)
<i>Population</i>	-0.109 (-0.84)	-0.0721 (-0.58)	-0.0824 (-0.64)
<i>War</i>	-2.225** (-2.42)	-2.066** (-2.22)	-2.030** (-2.17)
Countries	156	156	156
Observations	2886	2886	2886

z statistics in parentheses based on country-clustered standard errors.

All specifications include unreported year dummies.

Covers all years after demise of Soviet aid: 1985-2011.

Donor classification (conditional/unconditional) based on Table 6.

## 8 Appendix

Country	Cross-Section		Panel
	Knack	All	
Afghanistan		✓	✓
Albania	✓	✓	✓
Algeria	✓	✓	✓
Angola		✓	✓
Antigua and Barbuda		✓	✓
Argentina	✓	✓	✓
Armenia			✓
Azerbaijan			✓
Bahamas	✓	✓	✓
Bahrain		✓	✓
Bangladesh	✓	✓	✓
Barbados		✓	
Belarus			✓
Belize	✓	✓	✓
Benin	✓	✓	✓
Bhutan		✓	✓
Bolivia	✓	✓	✓
Bosnia and Herzegovina			✓
Botswana	✓	✓	✓
Brazil	✓	✓	✓
Brunei		✓	✓
Bulgaria	✓	✓	✓
Burkina Faso	✓	✓	✓
Burundi	✓	✓	✓
Cambodia	✓	✓	✓
Cameroon	✓	✓	✓
Cape Verde	✓	✓	✓
Central African Rep.	✓	✓	✓
Chad	✓	✓	✓
Chile	✓	✓	✓
China	✓	✓	✓
Colombia	✓	✓	✓
Comoros	✓	✓	✓
Congo, Dem. Rep.		✓	✓
Congo, Rep.	✓	✓	✓
Costa Rica	✓	✓	✓
Cote d'Ivoire	✓	✓	✓
Croatia			✓
Cuba		✓	✓
Cyprus	✓	✓	✓
Czech Republic (former Czechoslovakia)			✓
Djibouti			✓
Dominica		✓	✓
Dominican Republic	✓	✓	✓
Ecuador	✓	✓	✓
Egypt	✓	✓	✓
El Salvador	✓	✓	✓
Equatorial Guinea	✓	✓	✓



Country	Cross-Section		Panel
	Knack	All	
Eritrea			✓
Estonia			✓
Ethiopia	✓	✓	✓
Fiji	✓	✓	✓
Gabon		✓	✓
Gambia	✓	✓	✓
Georgia			✓
Ghana	✓	✓	✓
Grenada		✓	✓
Guatemala	✓	✓	✓
Guinea			✓
Guinea-Bissau	✓	✓	✓
Guyana	✓	✓	✓
Haiti			✓
Honduras	✓	✓	✓
Hungary	✓	✓	✓
India	✓	✓	✓
Indonesia	✓	✓	✓
Iran	✓	✓	✓
Iraq		✓	✓
Israel	✓	✓	✓
Jamaica	✓	✓	✓
Jordan	✓	✓	✓
Kazakstan			✓
Kenya	✓	✓	✓
Kiribati		✓	✓
Korea	✓	✓	✓
Kuwait		✓	✓
Kyrgyz Republic			✓
Laos			✓
Latvia			✓
Lebanon			✓
Lesotho	✓	✓	✓
Liberia		✓	✓
Libya			✓
Lithuania			✓
Macedonia (former Yugoslav Republic)			✓
Madagascar	✓	✓	✓
Malawi	✓	✓	✓
Malaysia	✓	✓	✓
Maldives	✓	✓	✓
Mali	✓	✓	✓
Malta	✓	✓	✓
Mauritania	✓	✓	✓
Mauritius	✓	✓	✓
Mexico	✓	✓	✓
Micronesia, Fed.States			✓
Moldova			✓
Mongolia	✓	✓	✓
Morocco	✓	✓	✓
Mozambique	✓	✓	✓

Country	Cross-Section		Panel
	Knack	All	
Namibia			✓
Nepal	✓	✓	✓
Nicaragua	✓	✓	✓
Niger	✓	✓	✓
Nigeria	✓	✓	✓
Oman		✓	✓
Pakistan	✓	✓	✓
Palau			✓
Panama	✓	✓	✓
Papua New Guinea	✓	✓	✓
Paraguay	✓	✓	✓
Peru	✓	✓	✓
Philippines	✓	✓	✓
Poland	✓	✓	✓
Qatar		✓	✓
Romania	✓	✓	✓
Russia			✓
Rwanda	✓	✓	✓
Samoa		✓	✓
Sao Tome & Principe			✓
Saudi Arabia		✓	✓
Senegal	✓	✓	✓
Seychelles		✓	✓
Sierra Leone		✓	✓
Singapore		✓	✓
Slovak Republic			✓
Slovenia			✓
Solomon Islands		✓	✓
South Africa	✓	✓	✓
Sri Lanka	✓	✓	✓
St. Kitts-Nevis		✓	✓
St. Lucia		✓	✓
St. Vincent and Gr.		✓	✓
Sudan		✓	✓
Suriname		✓	✓
Swaziland	✓	✓	✓
Syria	✓	✓	✓
Tajikistan			✓
Tanzania		✓	✓
Thailand	✓	✓	✓
Timor-Leste			✓
Togo	✓	✓	✓
Tonga		✓	✓
Trinidad & Tobago	✓	✓	✓
Tunisia	✓	✓	✓
Turkey	✓	✓	✓
Turkmenistan			✓
Uganda		✓	✓

Country	Cross-Section		Panel
	Knack	All	
Ukraine			✓
United Arab Emirates		✓	✓
Uruguay	✓	✓	✓
Uzbekistan			✓
Vanuatu		✓	✓
Venezuela	✓	✓	✓
Viet Nam			✓
Yemen			✓
Zambia	✓	✓	✓
Zimbabwe	✓	✓	✓