Aid is Not Oil: Donor Utility, Heterogeneous Aid, and the Aid-Democratization Relationship[*]

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Abstract

Recent articles conclude that foreign aid, like other non-tax resources, inhibits political change in authoritarian regimes. This paper challenges both the negative political effects of aid and the similarity of aid to other resources. It develops a model incorporating changing donor preferences and the heterogeneity of foreign aid. Consistent with the model's predictions, an empirical test for the period 1973-2010 shows that, on average, the negative relationship between aid and the likelihood of democratic change is confined to the cold war period. However, in the post-cold war non-democratic recipients of particular strategic importance can still use aid to thwart change. The relationship between oil revenue and democratic change does not follow the same pattern over time or across recipients. This supports the conclusion that aid has different properties than other, fungible, resources.

Does foreign aid inhibit democratic change? Recent studies have lamented the political effects of foreign aid in authoritarian recipients. Aid, it is argued, provides governments in these countries with resources that can be used to thwart challenges to their authority. Morrison (2009) argues that aid has a similar effect to revenue from state-owned oil enterprises in decreasing the likelihood of regime transition. Djankov, Montalvo and Reynal-Querol (2008) conclude that foreign aid may have a larger effect than oil revenue in preventing democratization. Ahmed (2012) concludes that aid and migrant remittances have similar effects, decreasing the likelihood of government turnover in authoritarian regimes. Bueno de Mesquita and Smith (2010) argue that aid, like revenue from oil, decreases the likelihood of leader turnover and dampens the democratizing effects of mass political movements in non-democratic countries.

These findings, if true, point to a dilemma for policymakers in any aid donors that value democratization. Democratic donors regularly interact with non-democratic, developing countries. They offer aid in exchange for political favors and as a way to increase development. It would be difficult for policymakers to be in the position where giving economic assistance necessarily results in a decreased probability that these countries will experience political improvements. Yet multiple, restrictive assumptions underlie results claiming a negative relationship between aid and democratization. It is important to ascertain whether this relationship is generally true and if it can be altered by decisions in the donor.

This study challenges the idea that aid operates like natural resource revenues in decreasing the likelihood of democratic change. The analysis starts with a model of donor utility, in which a democratic donor considers whether to offer aid to a non-democratic recipient in exchange for a costly policy favor. The model relaxes key assumptions of previous studies. It allows the donor to give a heterogeneous aid package made up of fungible aid, which adds to the recipient government's budget like revenue from oil, and/or non-fungible aid which is used for purposes the recipient government would not otherwise have pursued. Donors can offer different combinations of fungible and non-fungible aid across both periods and recipients. In non-democratic recipients, fungible aid is modeled as producing a negative externality for a

democratic donor because it decreases the likelihood of democratic change. The magnitude of this externality can vary over time.

Given the negative externality, donors prefer to give non-fungible aid in non-democratic recipients. However, the recipient government is assumed to prefer fungible aid, so that a given favor can be purchased with less fungible aid than non-fungible aid. The donor decides on the composition of aid after considering the size of the externality relative to the increased expenditure needed to buy a favor with non-fungible aid. Recipients are modeled as heterogeneous, including those of average and high strategic importance to the donor, and strategic importance can vary across periods and across recipients within a period. According to the model, aid to an authoritarian recipient will contain lower amounts of fungible aid as strategic importance declines and/or desirability of democratic change increases. This implies that the likelihood of a recipient leader successfully using aid to prevent democratic change varies over time and across recipients.

Hypotheses are derived from the model and tested on a dataset consisting of 129 developing countries over the period 1973-2010. The first hypothesis posits that aid is less likely to hinder democratic change in the post-cold war period. Many developing countries saw their geopolitical strategic importance decrease with the end of the bipolar power struggle of the cold war. Donors also increased their emphasis on democratization. Both of these changes suggest that democratic donors are less willing to give aid that will prop up dictators in the post-cold war period.

The second hypothesis acknowledges the likelihood that, even with declining average strategic importance for developing countries in the post-cold war period, some individual states will continue to be of high strategic value. Recipients in positions of importance receive more fungible aid. Hypothesis two states that even in the post-cold war period, aid can decrease the likelihood of democratic change in the most strategically important recipients.

The statistical analysis provides strong evidence that the relationship between aid and democratic change varies over time and recipients. During the cold war, higher levels of aid were associated with a lower probability of democratic change. There is no evidence that this negative

relationship between aid and democratic change exists, on average, in the post-cold war period. However, for recipients of high strategic importance, the negative association between aid revenue and the likelihood of democratic change is evident after the end of the cold war.

An innovation of the model is the ability of donors to vary the fungibility of aid over time and across recipients. As the extent to which aid is fungible is contested, it is important to test the plausibility of this assumption. For each hypothesis the relationship between aid and democratic change is compared to the relationship between oil revenue and democratic change. If aid is a predominantly fungible resource, then the relationship between aid and democratic change should be similar to the relationship between oil revenue (a fungible form of government income) and democratic change.

The empirical comparison shows the dissimilarity between aid and oil revenue. Oil revenue is associated with decreased democratic change in the post-cold war period while, on average, foreign aid is associated with less democratic change only during the cold war. The relationship between aid and democratic change varies based on the strategic importance of the recipient to donors; for oil revenue no such difference across strategic importance is observed. The differences between aid and oil revenue across time and recipients are consistent with a model in which donors are able to change the composition of aid to respond to strategic realities and their own preferences; they are inconsistent with the treatment of aid as an always fungible resource.

The results of this study are at odds with several recent articles and it is important to understand the source of differences in the empirical findings. To address this issue, as a final step in the analysis I revisit three studies using replication data provided by the authors: Morrison (2009), Bueno de Mesquita and Smith (2010), and Ahmed (2012). Taken together, these studies present what appears to be a robust finding of the negative political consequences of foreign aid in authoritarian regimes. In each case a re-examination of the original analyses calls into question the negative relationship between foreign aid and the likelihood of political change, and the similarity between aid and other forms of resources.

Contrary to the recent trend toward aggregation of non-tax resources in scholarly literature,

aid is not oil. Foreign aid comes from donors who have strategic priorities and preferences regarding democratic change. They also have tools to provide a heterogeneous basket of aid which can vary across time and recipients. Because of this, there is nothing inevitable about the relationship between aid and the likelihood of democratic change. Donors play a key role in determining this relationship, varying the composition of aid to suit their own purposes. In studying the relationship between aid, oil, and other variables of interest, scholars and policy makers should account for these differences and resist the temptation to aggregate non-tax resources into a single category.

Aid, Oil, and Political Change

A government with access to large sums of revenue from oil and gas is not dependent on its citizens for funding. It is therefore less accountable and faces less pressure to democratize.[1] A considerable body of literature has emerged examining the political curse associated with natural resources, not all of it in agreement as to the effects.[2]

In recent years, scholars have argued that foreign aid can create the same political curse associated with oil. Morrison (2009) claims that similarities between aid, oil revenue, and other forms of non-tax resources justify their aggregation to form a single measure of non-tax revenue available to the government. Others are more cautious about equating aid and oil revenue, arguing that aid has unique properties because it is given by an outside source, the donor.[3]

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[1]See Ross (2001, 2012) for a discussion of the links between oil and authoritarianism, including an excellent overview of the literature and potential causal pathways. On the link between taxation and democracy, see Tilly (1990).

[2]E.g. Acemoglu et al. (2008), Boix (2003), Dunning (2008), Epstein et al. (2006), Goldberg, Mvukiyehe and Wibbels (2008), Haber and Menaldo (2011), Jensen and Wantchekon (2004), Ramsay (2011).

[3]Collier (2006).

Formal treatments of the relationship between aid and either leader or regime survival tend to model the relationship between a recipient government and its citizenry; aid is simply added to the budget constraint of recipient governments. Smith (2008) models the likelihood of revolutionary onsets and the response of the government as a function of "unearned income," which includes revenue from oil as well as foreign aid. There is no heterogeneity in unearned income in the model - it is simply added to the government's budget. Ahmed (2012) adds a variable for aid to a model relating institutional quality to the quantity of migrant remittances.[4] Once again, the main focus is on the interaction between the recipient government and its citizenry; aid is simply added to the government budget. Morrison (2007) allows for donors to give either conditional or unconditional aid, but once given, aid enters directly into the government budget. Bueno de Mesquita and Smith (2009) model the interaction between donors and recipients, in which donors seek to extract a policy concession from the recipient government in exchange for aid. They assume aid is a fungible payment from the donor to the recipient government.

The relationship between aid and the likelihood of political change in recipients has also been a fertile area for empirical research. Dunning (2004) shows that aid to Africa is associated with increased democracy in the post-cold war period, but not earlier. Djankov, Montalvo and Reynal-Querol (2008) and Morrison (2009) find that aid decreases the likelihood of regime change, while Bueno de Mesquita and Smith (2010), and Ahmed (2012) find that aid increases the likelihood of survival for individual leaders or governments. Knack (2004) finds no significant relationship between foreign aid and democratic change. Wright (2009) finds that autocratic leaders who expect to remain in office post-democratization respond positively to promises of increased aid in exchange for democratic change. Kono and Montinola (2009) find different effects of long-term and short-term aid on the likelihood of leader survival, with continued aid helping to entrench autocrats in the long run. Bermeo (2011) shows that in the post-cold war period, aid from oil-rich, autocratic donors entrenches dictatorships, while aid from democratic donors does not. Kersting and Kilby (2014) highlight the importance of aid conditionality, and

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[4]Abdih, Chami and Dagher (2012).

find differences in the aid-democratization relationship based on the geopolitical importance of the recipient.

Strategic Importance, Donor Preferences, and Heterogeneous Aid

This section models the utility of a democratic donor when allocating aid to a non-democratic recipient government in exchange for a policy favor. This abstracts from many significant aspects of aid, such as when donor priorities for aid intersect with recipient government priorities, rather than being a payment for favors rendered. This simplification is undertaken for two reasons. First, situations of aid-for-policy deals represent a difficult case. When a donor needs a favor from the recipient, it is in less of a position to dictate how aid is spent. It is in these situations that we are most likely to observe aid dampening the likelihood of democratization. Second, influential models showing negative political effects of aid, such as Bueno de Mesquita and Smith (2009), focus on instances in which aid is given as part of a policy deal. The question addressed here is whether relaxing assumptions underlying existing studies yields the possibility of a different relationship between aid and democratic change, even in cases most likely to see negative political effects of aid. The most notable innovations here include modeling aid as heterogeneous, made up of both fungible and non-fungible resources, and allowing donors to experience a negative externality when aid makes democratization in a recipient less likely.

Variation in Strategic Importance and Donor Preferences   During the cold war, dictators on the "right" side of the bipolar power struggle were rewarded with large amounts of aid. An example of this is the regime of Mobutu Sese Seko in Zaire (now Democratic Republic of Congo), which received hundreds of millions of aid dollars a year in exchange for supporting the fight against communists in the Angolan civil war. There was probably little doubt at the time that Mobutu was not using the money to "vaccinate children or train teachers."[5] Donors would likely have preferred that Mobutu help fight communism *and* that aid be used for beneficial, or at least not harmful, programs. However, strategic considerations overshadowed donor preferences and

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[5]Radelet (2003), 107-108.

the money continued to flow. The collapse of the Soviet Union brought about change; as Brautigam and Knack (2004, 275) note "the end of the cold war allows the United States and other donors to target aid more selectively, rather than using aid to strengthen corrupt but geopolitically useful autocracies."

The end of the cold war saw not only the demise of strategic importance for many developing countries, but an increased emphasis on democratization for many democratic aid donors. As Kelley (2008, 229) argues, "the [cold] war's end freed Western countries to push for democratic changes. Indeed, democracy increasingly came to be seen as strengthening rather than undermining security interests." The possibility of impeding democratization can create a negative externality for democratic donors in any period. However, the magnitude of this externality and the extent to which it is trumped by strategic considerations likely changes with the transition from the cold war to post-cold war period.

While the strategic importance to donors of the average developing country fell with the end of the cold war, in any period there will be recipients of particular geopolitical significance. Even in the post-cold war period, there will be situations where strategic importance takes precedence over preferences for democratization.[6] Similar arguments have linked the importance of the recipient to the effectiveness of conditionality at the IMF[7] and World Bank.[8] The model developed here incorporates variation in strategic importance both across periods and across recipients within periods.

Heterogeneous Aid   An innovation of the model is that it allows for two types of aid: fungible and non-fungible. Fungible aid is defined as either accruing directly to the recipient government or financing programs the recipient government would have undertaken in the absence of aid, thus freeing up resources for other purposes. Non-fungible aid is defined as providing

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[6]Kersting and Kilby (2014) make a similar point.

[7]Stone (2002, 2008).

[8]Kilby (2009).

programs/services the recipient government would not have undertaken in the absence of aid, that do not free up government finances for other purposes.

Recipient governments prefer fungible aid, while democratic donors prefer to provide non-fungible aid to non-democratic recipients to minimize the likelihood that aid inhibits democratic reform. For a given recipient, the donor weighs the magnitude of the negative externality associated with decreased likelihood of democratic change against the greater effectiveness of fungible aid for buying favors from the government. When the allocation decision results in high levels of fungible aid, it increases the likelihood that aid will be associated with less democratic change.

How realistic is the idea that donors can alter the composition of their aid between fungible and non-fungible? Empirical evidence of aid fungibility is mixed.[9] Collier (2006) suggests that in some recipients the likelihood of donor development projects replacing programs funded by the recipient government is low, since the government is unable/unwilling to undertake any development projects in the absence of aid. Related work on the effects of aid suggests that these differ with regard to the type of assistance, time period, and/or identity of the donor, suggesting limits on fungibility.[10] Recent studies and policy statements are consistent with donor attempts to limit fungibility in certain types of recipients. Bermeo (2010) shows that the composition of aid in poorly governed countries suggests it may be less fungible than in well governed recipients, and Dietrich (2013) demonstrates that donors bypass governments when giving aid in recipients with bad governance. Hilary Benn, former Secretary of State for International Development in the United Kingdom, puts a policy voice on these findings when he claims that the UK "finds practical ways to ensure that aid cannot be siphoned off. We can and do directly fund ... 'concrete things' ...Or we can earmark aid for a particular programme of work in a sector and account for that money independently through a separate bank account. We do this in the education sector in

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[9]Feyzioglu, Swaroop and Zhu (1998); Pack and Pack (1990).

[10]Clemens et al. (2012); Bearce and Tirone (2010); Bermeo (2011).

Kenya, where the financial risk of handing over money to the government is too great."[11] Taken together, this suggests that while it is likely some aid is fungible, it is also likely that not all aid is fungible.

The ability to compare aid to more clearly fungible streams of government resources allows the question of fungibility to become a testable premise of the model, rather than an assumption accepted (or not) in the absence of evidence. The relationship between oil revenue - a resource which often accrues directly to the government - and democratic change is examined to see if the oil revenue-democratic change and aid-democratic change relationship show similar trends over time and recipients. If aid is a predominantly fungible resource, it should have properties similar to oil revenue.

Modeling Foreign Aid

The model developed here is restricted to cases of a democratic donor considering aid to a non-democratic recipient, as these are the instances where donor concern regarding possible democratization seems most relevant. It adopts a similar structure to that used in Bueno de Mesquita and Smith (2009): the donor offers a recipient aid in exchange for a policy favor that is costly for the recipient government to enact, what the authors refer to as an "aid-for-policy" deal. Examples of this include the United States buying votes with aid at the UN[12] and Switzerland using aid to buy votes supporting its membership on the executive board of international financial institutions.[13] Here, as in Bueno de Mesquita and Smith (2009), the offer is only made and accepted if it is incentive compatible for both donor and recipient.

A donor government *(d)*, when deciding on a potential aid package to a recipient *(x)*, will compare its utility when giving the aid to a baseline utility without giving the aid. This baseline

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[11]Open exchange between Bill Easterly and Hilary Benn, "Is Foreign Aid Working?", printed in Prospect Magazine, October 19, 2006.

[12]Carter and Stone (2014).

[13]Vreeland (2011).

utility can be written as

$$U_{dt,noaid} = S_{dt}(R_{dt}) \tag{1}$$

where S_{dt} is the donor's utility function at time *t* for all purposes other than aid to the recipient and R_{dt} is the total amount of resources available to the donor government.

Two different types of recipients are modeled: those of average and high strategic importance. Recipients of average strategic importance are in a position to provide the donor with ω_t in utility, where ω_t is the benefit the donor derives from the policy concession and can vary over time as the international political climate changes. Recipients of high strategic importance in any period can supply the donor with θ in additional utility.

If the donor offers an aid package, the resources available to the donor government for other purposes are reduced by the amount of the aid, *A*. Aid can be a mixture of fungible aid *(F)* and non-fungible aid *(N)*, such that the resources expended (and therefore not available for other purposes) are $A = F + N$ and $\frac{\partial S_{dt}}{\partial F} = \frac{\partial S_{dt}}{\partial N}$. Fungible aid produces a negative externality for the donor by decreasing the likelihood of democratic change in the recipient. The magnitude of the externality is captured by $\beta_t F$; β_t can vary over time as donor preferences regarding democratization change.[14]

The donor's utility function at time *t* if it enters into an aid-for-policy deal with the recipient can be written as:

$$U_{dt,aid} = \omega_t + \gamma_{xt}\theta + S_{dt}(R_{dt} - A) - \beta_t F \tag{2}$$

where γ_{xt} is an indicator variable that takes the value of one when the recipient is of high strategic importance and zero otherwise. The aid-for-policy deal is incentive compatible for the donor if $U_{dt,aid} \geq U_{dt,noaid}$:

$$\omega_t + \gamma_{xt}\theta + S_{dt}(R_{dt} - A) - \beta_t F \geq S_{dt}(R_{dt}) \tag{3}$$

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[14]Given the focus of the analysis, the externality is assumed to be in the form of decreased likelihood of democratization only. It could be broadened to include any additional negative aspects of giving *F* relative to *N*, such as a reduction in development outcomes when aid is fungible.

Given the externality, the donor would prefer to give N rather than F in non-democratic recipients. The cost associated with giving a unit of N is captured by $\frac{\partial U_{dt}}{\partial A}$, while the cost associated with a unit of F is $\frac{\partial U_{dt}}{\partial A} - \beta_t$. However, the point of the deal is to buy a policy concession from the recipient government, which is assumed to prefer fungible over non-fungible aid. Given this, it takes less F to purchase the favor than N. To decide on the right mix of F and N to offer (if any), the donor must also consider the situation from the recipient point of view.

The utility of the recipient government without aid is a function of the amount of resources it has to spend:

$$U_{xt,noaid} = S_{xt}(R_{xt}) \tag{4}$$

where R_{xt} represents total resources to the recipient government without aid. If the recipient government accepts an aid for policy deal then it incurs a cost associated with acceding to the donor's demands for ω_t and θ:

$$C = C_\omega + \gamma_{xt}C_\theta \tag{5}$$

where the recipient only incurs the cost C_θ if it is strategically important to the donor ($\gamma_{xt} = 1$).

The donor supplies the recipient with aid in the form of F and N in order to "produce" enough utility for the recipient government to undertake the desired action. Fungible aid, F, directly increases the amount of resources the recipient government has at its disposal. Non-fungible aid does not free up government resources, but may still provide a benefit to the recipient government - it may support programs the government values but would not have been able to/chosen to provide itself. The recipient government's utility under the aid-for-policy deal is given by:

$$U_{xt} = S_{xt}(R_{xt} + F) + V_{xt}(N) - C \tag{6}$$

where $V_{xt}(N)$ is a function transforming non-fungible aid into utility. The recipient government is assumed to (weakly) prefer fungible to non-fungible aid, so that $\frac{\partial U_{xt}}{\partial F} \geq \frac{\partial U_{xt}}{\partial N}$, or (equivalently)

that $\frac{\partial S_{xt}}{\partial F} \geq \frac{\partial V_{xt}}{\partial N}$.[15] An aid for policy deal will be incentive compatible for a recipient when the following holds:

$$S_{xt}(R_{xt} + F) + V_{xt}(N) - C \geq S_{xt}(R_{xt}) \tag{7}$$

In a situation of perfect information, the donor can offer the recipient a basket of *F* and *N* that produces just enough increase in utility to compensate for the cost of granting concessions, so that Equation 7 holds with equality. Obviously, the more highly the recipient values each unit of *F* and/or *N*, the less aid it will take to cover the cost of granting the concession; in these cases it is most likely that an aid-for-policy deal can be arranged that is incentive compatible to both donor and recipient. This suggests that small countries may be "bought" more cheaply since any given amount of funding will provide more benefit per person, and thus be more valuable, when there are fewer people. Additionally, assuming concave preferences for funding, poorer countries will have a higher value for each additional unit of funding, implying that buying favors will be cheaper the lower is R_{xt}. These are similar to conclusions reached in Bueno de Mesquita and Smith (2009) and Stone (2006), and therefore not the focus here.

The novelty in this model lies in the ability of the donor to give both fungible and non-fungible aid, and the negative externality imposed on the donor when it gives fungible aid. The higher the value of β_t, the more costly it is for the donor to give *F* relative to giving *N*. And, the greater the distance between $\frac{\partial S_{xt}}{\partial F}$ and $\frac{\partial V_{xt}}{\partial N}$, the less valuable *N* is to the recipient relative to *F*. In order to determine whether to offer the aid-for-policy deal to a recipient, the donor will compare its utility without the deal to the utility it would receive maximizing Equation 2 subject to the recipient's incentive compatibility constraint (Equation 7). From the first order conditions

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[15]This is a simplification that may not always hold; in some cases a recipient may place a higher value on (non-fungible) technical cooperation if it supplies expertise that cannot be purchased domestically. In those cases the donor should supply this first, before switching to fungible aid or other types of non-fungible aid. As long as technical cooperation alone is not enough to buy the policy deal, the inequality will be true for the relevant portion of the analysis.

of this constrained optimization, the donor will minimize costs when the combination of F and N offered satisfy:

$$\frac{\frac{\partial S_{xt}}{\partial F}}{\frac{\partial V_{xt}}{\partial N}} = \frac{\frac{\partial S_{dt}}{\partial A} - \beta_t}{\frac{\partial S_{dt}}{\partial A}} \tag{8}$$

The left-hand side of Equation 8 is the marginal rate of substitution between F and N for producing utility in the recipient; the right-hand side is the ratio of the costs to the donor from foregone utility of providing F relative to N. Equation 8 will hold when cost minimization is achieved through a mix of N and F (an interior solution). Corner solutions, in which the cost-minimizing aid package consist of only N or F are also possible. The former is more likely when the recipient has a relatively high value for N and relatively low cost of granting the policy concession; the latter is most likely when the recipient places low value on N relative to F. The donor will offer the recipient a package of aid if the minimum cost combination of F and N which satisfy Equation 7 also satisfy Equation 3.

Equation 8 highlights the relatively complex relationship between donor utility, recipient utility, and the negative externality β_t. First, there may be some recipients for which the left hand side of Equation 8 is always greater than the right hand side: in this case if a deal is incentive compatible for the donor it will consist of only fungible aid (corner solution). In other cases, where the left hand side is lower than the right hand side for low levels of aid, the donor will include only N in the aid package up until the point where the next unit of N adds less value in terms of recipient utility than is saved for the donor by giving N rather than the higher cost F. If this point is reached before incentive compatibility is met for the recipient, the donor will minimize costs by providing a mixed package of N and F that satisfies Equation 8, if it provides any aid at all.

Because $S_{dt}(R_{dt} - A)$, $S_{xt}(R_{xt} + F)$, and $V_{xt}(N)$ are each assumed to have the usual concave properties of utility functions, the tradeoff of N for F that satisfies Equation 8 changes as N and F change. An example can help illustrate the point. Suppose the recipient is at a point on its utility function where the next unit of utility can be purchased with 3 units of N or 2 units of F. Each unit of N will cost the donor $\frac{\partial S_{dt}}{\partial A}$ and each unit of F will cost $\frac{\partial S_{dt}}{\partial A} - \beta_t$. If $\frac{\partial S_{dt}}{\partial A} < \beta_t$ for the

extra unit of *A* required to purchase the same utility with *N* rather than *F*, then the donor will supply *N*; otherwise it will supply *F*. As the recipient receives more *N* the marginal utility increase associated with additional *N* will decline, meaning more *N* will be needed to offset a unit of *F*. On the donor side, as more *A* is supplied the decrease in $S_{dt}(R - A)$ for each additional unit of *A* increases, meaning the donor is less willing to trade off increased *A* in the form of *N* in order to avoid the externality associated with *F*.

Several useful insights come from this setup. Beginning with recipients of average importance, envision a heterogeneous group deriving varying levels of utility from *F* and *N* at an initial time, period 1. A shock occurs, such as the end of the cold war, decreasing strategic importance for the average recipient, ω_t; this shock moves the countries into period 2. Within these average recipients, some were right at the incentive compatibility constraint for the donor in period 1, while others were well inside it. Those who were at or near the donor's incentive compatibility constraint will now fall outside of it when the value of ω_t falls, and will no longer be offered an aid-for-policy deal. Because recipients requiring the most *F* at any point are the most expensive for the donor, they are the most likely to be dropped from aid deals in period 2. Thus, the remaining pool of recipients receiving a deal will receive less fungible aid on average than those in period 1, and a higher proportion of their aid package will be non-fungible aid. The changing strategic importance leads to the expectation of a changing composition in aid, away from fungible aid.

It is also useful to contemplate the effect of an increase in the negative externality for giving fungible aid to a dictatorship, β_t, in the move from period 1 (e.g. cold war) to period 2 (e.g. post-cold war). An increase in β_t increases the cost of *F* relative to *N*. For those recipients that remain incentive compatible in period 2, aid will shift away from the more costly *F* toward the relatively cheaper *N*. Importantly, given recipient preference for *F* over *N*, this will mean an increase in *A* is necessary to maintain the minimum level of benefit to make the deal incentive compatible for the recipient. Holding strategic importance constant, for recipients still receiving an aid package total aid *increases* while fungible aid *decreases*. This decline in fungible aid

suggests a decreased ability for recipient governments to use aid to thwart democratic change, as less aid accrues to them for their discretionary use.

Within any period, the situation is different for recipients of high strategic importance; in general they will receive a higher proportion of their aid package as fungible aid than recipients of average strategic importance. This follows from the fact that these recipients require more aid to grant the higher-cost policy concessions, and returns diminish more quickly for *N* than for *F*. Thus as aid increases, the difference between the amount of utility purchased with *N* and *F* also increases; minimizing costs as required by Equation 8 will involve giving a larger proportion of the aid package in the form of *F*. It is still true that an increase in β_t across periods will cause the donor to shift toward *N*. However, within any period the composition of aid for recipients of high strategic importance is weighted more toward fungible aid. The different composition of aid across recipients of average and high strategic importance suggests that the relationship between aid and democratic change may also vary across these groups of countries within a time period.

Hypotheses

Arguments that aid prevents democratic change rest on the assumption that aid is a fungible resource available to the recipient government; this assumption is relaxed here. The hypotheses are set up to test for differences in the aid-democratic change relationship over time and across recipients, as suggested by the model. They also examine the relationship between oil revenue and democratic change, to probe whether aid has properties similar to fungible resources.

Variation over Time　With the move from the cold war to the post-cold war period, average strategic importance (ω_t) declined and distaste for authoritarianism (β_t) increased. In response to this, donors give less fungible aid on average, leading to the hypothesis:

> *H1a: Foreign aid is less likely to inhibit democratic change in the post-cold war period.*

If observed changes in the aid-democratic change relationship are attributable to a shift away

from fungible aid, then the same changes should not be observed for fungible government resources, such as oil revenue. Thus the model implies:

H1b: Changes over time in the relationship between foreign aid and the likelihood of democratic change are not observed for the relationship between oil revenue and the likelihood of democratic change.

Variation across Recipients   Regardless of average changes across time, there are recipients in any period that are not fully democratic yet are in a position to provide a democratic donor with favors of high strategic importance. The model incorporates this heterogeneity, allowing for the greater benefit to the donor (θ) and cost to the recipient (C_θ) in these situations. As shown above, this results in an increase in fungible aid relative to recipients of average importance, both in absolute levels and as a proportion of the total aid package. This makes it more likely that the government in recipients of high strategic importance can use aid resources to impede democratic change, suggesting:

H2a: Aid is more likely to inhibit democratic change in recipients of high strategic importance.

As with variation over time, insights can be obtained by comparing the relationship between aid and democratic change with that between oil revenue and democratic change. There is no reason that being of only average strategic importance to outside donors should make a country less able to use fungible resources to prevent democratic change. If a difference in the aid-democratic change relationship between recipients of average and high strategic importance is the result of a difference in aid fungibility across these two types of recipients, then the same difference should not be observed in the oil revenue-democratic change relationship, implying:

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H2b: Differences across recipient type in the relationship between foreign aid and the likelihood of democratic change are not observed for the relationship between oil revenue and the likelihood of democratic change.

Data and Results

The hypotheses are tested using a dataset that includes 129 developing countries and covers the period 1973-2010.[16] Results of multiple alternative specifications are discussed to probe the robustness of the main findings. The following section revisits results of Morrison (2009), Bueno de Mesquita and Smith (2010), and Ahmed (2012) to better understand the factors leading to differences between the empirical findings below and previous results.

Dependent Variable   The dependent variable is an indicator of democratic change coded using changes in a country's score on the Polity2 measure from the Polity IV project.[17] The Polity2 measure runs from -10 to 10, with higher numbers representing greater democratization. The dependent variable equals 1 if a country experienced an *increase* in its Polity2 score of 3 points or more from *t-1* to *t* and zero otherwise. The sample is restricted to countries with a Polity2 score of 7 or lower in *t-1*, since scores higher than that cannot increase by three points. Unlike Morrison (2009), which considers a three-point change in the Polity score regardless of direction, the main analysis here examines only movements toward democratization. As Ulfelder (2007) notes, failure to control for the direction of the change conflates democratic transitions with democratic failures. For regressions covering the cold war period, the dependent variable takes the value of 1 in 78 of 1645 observations (4.7%); for the post-cold war the value equals 1 for 71 of 1666 observations (4.3%).

An indicator for a three-point change is chosen as the main dependent variable because it is

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[16]The supplemental appendix includes further details on the construction of the dataset.

[17]Marshall and Jaggers (2011).

less blunt then an indicator for full democratization. The question here is whether receiving more aid decreases the chance that a country will move toward democratization. This measure avoids situations in which movement from just below to just above a cutoff for democratization counts as a change, while much larger movements below the cutoff do not. Results are tested for robustness using different measures of democratic change.

Key Independent Variables   Data on aid are from the OECD, which records aid commitments by year for all recipients from OECD donors and other donors that choose to report to the OECD. The model developed above relates only to democratic donors; there is no assumption that authoritarian donors experience a negative externality from entrenching dictatorships. Therefore, total aid commitments minus any commitments from authoritarian donors are used. Aid is divided by population as reported in the Penn World Table, v.7.1[18] and the log of one plus aid per capita is used as the measure of foreign aid. A measure of the log of oil wealth per capita is also included; data on oil are from Ross (2012). Aid, oil, and other independent variables are lagged by two years to ensure that they capture the value prior to the change from *t-1* to *t* used to construct the dependent variable.

Control Variables   The log of income per capita and growth rate of income per capita (lagged 1 year, as it is a growth rate) from the Penn World Table v.7.1 are included. Because a country's value in year *t-1* on the Polity2 scale may affect its ability to further democratize, a lagged value for the Polity2 variable is included. Also included is a variable for previous transitions that counts the number of times a country has had a positive change of three-points or more on the Polity2 index in the previous 40 years, as previous experience with democracy may be important. A time trend variable for year is included.

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[18]Heston, Summers and Aten (2012).

Aid, Oil, and Democratic Change

Table 1 presents the main tests for *H1a* and *H1b*, using a Logit model with *Age, Age-Squared,* and *Age-Cubed* included to account for temporal dependence.[19] Model 1 reports results for the entire time period, 1973-2010. Model 2 reports results for the cold war period, 1990 and prior. Due to the two-year lag, 1990 is chosen as a break point: aid for observations in 1990 was committed in 1988, prior to the end of the cold war.[20] Model 3 includes observations for 1993-2010.[21] Standard errors are clustered on country; p-values are in parentheses.

[Table 1 about here.]

The coefficient on aid is negative but not significant for the full period (Model 1). As Models 2 and 3 show, this masks heterogeneity across periods. During the cold war, more aid is associated with a decreased likelihood that a country experiences democratic change. The same is not true in the post-cold war, where the coefficient on aid is positive but not significant. A Wald test for equality of the coefficients for aid between Models 2 and 3 suggest that they are not equal ($p < 0.02$). This provides strong support for *H1a:* the relationship between aid and the likelihood of democratic change varies across periods as predicted. This is consistent with changes over time in strategic importance and donor preferences, and the ability of donors to alter the composition of aid in response. If the period is further restricted to the post-2001 "war on terror" time period (not shown), there is still no negative effect of foreign aid: restricting the sample to post-2001

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[19]Carter and Signorino (2010) show that this approximates a hazard model.

[20]The analysis excludes the transition years of 1991 and 1992.

[21]A single model for 1973-2010 with an indicator for the cold war (equal to one prior to 1991) entered independently and interacted separately with aid and oil revenue shows the same pattern observed in Table 1; see supplemental appendix. The sub-samples are shown for ease of exposition and due to difficulty interpreting interaction terms in non-linear models (Ai and Norton, 2003; Greene, 2010).

yields a coefficient on aid of 0.399 (p=0.04).[22]

To further probe the likelihood that a change in the composition of aid drives these differences over time, *H1b* examines the relationship between oil revenue and democratic change. The coefficients on oil revenue in Table 1 show no evidence that oil has become less likely to impede change over time in these countries. The coefficient is negative across regressions and increases in magnitude from the cold war to post-cold war, although the null of equality cannot be rejected (p=0.35). Further evidence of the difference between aid and oil is the significant difference between the coefficient values for these variables in the post-cold war period (p=0.04).[23]

The empirical structure is designed to decrease the chance of reverse causation, which could only hold with the lags specified if democratization causes aid *before* the democratization occurs. Even if this occurs, given the comparative nature of the hypotheses, any explanation based on reverse causality would need to account for variation across periods in this relationship. While not impossible, this is likely implausible. Furthermore, explanations consistent with the model above can incorporate reverse causality; this is discussed in the supplemental appendix.

Neither the model nor the empirical results should be taken as evidence that aid can *cause* democratization. The argument is that donors, when it fits their needs and preferences, allocate

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[22]See supplemental appendix.

[23]It would be a mistake to draw lessons from this analysis for the debate regarding the relationship between oil revenue and democratization (e.g., Ross, 2012; Haber and Menaldo, 2011). This analysis is restricted to aid recipient countries, as aid is the primary variable of interest. Within this group of countries, it is reasonable to compare the coefficients of aid with those on oil. However, eligibility as an aid recipient is restricted by income (high income countries are ineligible based on OECD reporting criteria). This makes it inappropriate to draw lessons for the overall relationship between oil revenue and democratic change from this group of countries, as oil has made some countries rich and therefore ineligible for official development assistance.

aid in a manner that does not prevent democratic change. This is not the same as claiming that they know how to bring about change. The analysis here captures a different question than that asked by studies examining whether aid targeted for democracy promotion actually promotes democratization.[24] Instead, the present analysis engages with studies examining the relationship between the total amount of aid and the likelihood of political change in recipients.

In developing countries foreign aid - which comes from a donor - exhibits a change over time that is not evident for revenue from oil, providing strong support for *H1a* and *H1b*. There is no evidence that foreign aid in the post-cold war period inhibits democratic change. Oil revenue, on the other hand, is associated with a decreased likelihood of change even after the end of the cold war.

Country Fixed Effects　The results in Table 1 cannot distinguish between cross-country and within-country variation. To mitigate concerns that the results are driven by unmeasured country-specific attributes, country fixed effects can be included. Unfortunately, there is a significant loss of observations and countries due to separation when fixed effects are included in a model with a binary dependent variable, like those in Table 1.[25] Table 2 reports alternate models, which use non-binary measures of democratic change for the dependent variable and include country fixed effects.

Results reported in Table 2 show that the pattern observed in Table 1 on the coefficient for foreign aid over time is robust to the inclusion of recipient country fixed effects and to the use of alternate measures of political change for the dependent variable. Models 4 and 5 use the change from *t-1* to *t* in a country's Polity 2 score as the dependent variable. Note that this change can be either positive or negative. For Models 6 and 7, the dependent variable is measured as the change

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[24]E.g., Finkel, Perez-Linan and Seligson (2007).

[25]See Beck and Katz (2001). Including country fixed effects in Models 1-3 of Table 1 would decrease the number of observations and countries significantly (see supplemental appendix). Separation is further discussed in the context of the analysis in Ahmed (2012) in the next section.

in a country's average on the Freedom House measures of political rights and civil liberties for a given year, inverted so that higher values are more democratic. Using the change in Polity or Freedom House from *t-1* to *t* as a measure of political change and including country fixed effects, there is a negative, significant coefficient on aid in the cold war and a positive, insignificant coefficient in the post-cold war - the same pattern observed in Table 1.[26]

[Table 2 about here.]

The coefficient on oil revenue, while negative in all models, is significant in only one of the four. It is not surprising that the inclusion of country fixed effects diminishes the significance of oil revenue. Many leaders in oil-rich countries have long time horizons and may be able to smooth spending over short fluctuations in oil prices, so that yearly fluctuations in oil revenue have little impact unless they are particularly severe. The variation is more likely between countries that have significant oil revenue and those that do not, rather than within oil-rich countries.

Robustness   In addition to the inclusion of fixed effects, the use of an alternate dependent variable from Freedom House, and the revisiting of previous research shown below, other robustness checks were performed.[27] The results are almost identical to those reported in Table 1 if a rare events Logit is estimated. Additionally, using as the dependent variable an indicator for a five point or more shift in Polity2 (rather than the three point shift above), or for a two-or-more, four-or-more or six-or-more point shift, the pattern of a negative, significant coefficient on aid in the cold war and an insignificant positive coefficient in the post-cold war remains. Models 2 and 3 are re-estimated including a control for the recipient country's population size; once again a change consistent with *H1a* is noted across time for foreign aid, while revenue from oil remains negative in both periods, providing support for *H1b*.

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[26] An alternative would be to estimate the models with the binary dependent variable in Table 1 using OLS with country fixed effects (Beck, 2011). This approach yields the same pattern; see supplemental appendix.

[27] All results from alternative models are in the supplemental appendix.

Strategically Important Recipients

Tables 1 and 2 show a change, on average, in the relationship between aid and the likelihood of democratic change over time. However *H2a* suggests that in strategically important recipients, the government may be able to obtain a high degree of control over aid resources even in the post-cold war period. To test for differences in the relationship between aid and democratic change across recipients, Table 3 introduces an indicator variable that equals 1 if a country is one of the top 5 recipients of US military aid in a given year (which is not a component of the foreign aid examined here) and its interaction with *Aid*, both lagged two years. This is meant to capture only the most strategically important countries, so is restrictive by design.[28] US military assistance may not be the best proxy of strategic importance for all democratic donors. However, the United States is the largest bilateral aid donor and has significant influence at the major multilateral donors. It is important that any proxy for strategic importance for overall levels of aid pick up strategic importance to the United States. Additionally, for the most strategic cases, many NATO donors increase aid as US military aid increases, suggesting that they are responding to the strategic importance of these countries.[29]

Table 3 explores whether the variation over time shown by the results above applies regardless of a recipient government's strategic position, or if strategically important recipient governments are better able to manipulate aid for their advantage. The results suggest that the latter is more likely true. Models 8 and 9 report results for the cold war and post-cold war, respectively. The coefficients for "ln Aid per capita" capture the relationship between aid and the likelihood of democratic change in recipients not classified as strategically important, and show the same pattern over time as that observed in Table 1. The coefficients on the interaction term

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[28]Data on US military assistance are available at www.usaid.gov.

[29]Examples of this are large amounts of aid from multiple bilateral donors to Egypt, Pakistan, Jordan and the Philippines in the 1980s as well as to Iraq and Afghanistan in recent years. See also Kersting and Kilby (2014).

between aid per capita and strategic importance, *Top 5*Aid* are negative in both models and significant in the post-cold war period. The results are consistent with strategically important recipient governments using aid to prevent democratic change even in the post-cold war period. The larger magnitude coefficient and increased significance for the variable *Top 5*Aid* in the post-cold war show donors compensating for the decline in average effect in these particularly important recipients and highlight an increased divide between the two recipient types. Models 10 and 11 are analogous to Models 8 and 9, but with the addition of an interaction term *Top 5*Oil*. That interaction is not significant and including it does not change the pattern for aid. These results present strong support for *H2a* and *H2b*, showing that aid, but not oil, has a different relationship with the likelihood of democratic change in strategically important states.[30]

[Table 3 about here.]

Comparison with Previous Work

These findings challenge the results of several recent studies, such as Djankov, Montalvo and Reynal-Querol (2008), Morrison (2009), Bueno de Mesquita and Smith (2010), and Ahmed (2012), which conclude that aid inhibits political change in recipients. There are multiple potential reasons for these differences. From a theoretical perspective, none of these studies tests for differences in the aid-democratic change relationship over time. From an empirical point of view, none of these studies is identical to the present analysis or to each other in terms of time, countries, variables, and methods. This section revisits the analysis in Morrison (2009), Bueno de Mesquita and Smith (2010), and Ahmed (2012) to better understand the underlying factors leading to the different results.[31] The conclusions of these works are questioned both on

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[30]A similar pattern is observed if the top 10 recipients of military aid, rather than the top 5, are designated as important.

[31]Email communication with the authors confirms that replication files for Djankov, Montalvo and Reynal-Querol (2008) are unavailable.

theoretical and empirical grounds.

Morrison, 2009

Morrison (2009) argues that multiple forms of "non-tax revenue," including foreign aid and revenue from state-owned oil enterprises, have a similar effect of decreasing the likelihood of regime transition. As a result of this, the study concludes that they can be aggregated into a single category of non-tax revenue for empirical analysis. Table 4 exactly replicates the results from Morrison (2009), Table 3, Model 1, with robust standard errors in parentheses.[32] The dependent variable is an indicator that takes the value of 1 if a country experienced a three-point change in its Polity score. This is similar to the measure used in Table 1 above, except that in Morrison (2009) the change in polity can be either positive or negative. The time period is 1973-2001. The variable "Grants per capita" is used as the measure of foreign aid. A central claim of the article is that "the particular source of nontax revenue - state-owned enterprises, aid, or other sources - does not make a difference: they all act similarly with regard to regime stability and the causal mechanisms" (109). Model 1 is presented as evidence of this: "As shown in Model 1 of Table 3, the coefficients on all three nontax revenue components are negative and significant...This result is important in its own right, as it demonstrates that foreign aid, state-owned enterprise revenue, and other kinds of nontax revenue - including borrowing - have similar effects" (119-120).

[Table 4 about here.]

Column 2 presents results using the same data and model, but restricting the time period to pre-1989; Column 3 does the same for the post-1991 period. As can be seen, the coefficient on "Grants per capita" is significant only in the earlier period (for the post-cold war, p=0.66). A Wald test on the coefficients for "Grants per capita" in columns 2 and 3 suggests they are unlikely to be equal (p=0.07), indicating that the decrease in magnitude from the cold war to the post-cold war is significant. This is in sharp contrast to the pattern observed for the coefficient on revenue

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[32]Relevant variables shown; see supplemental appendix for results including all variables.

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from state-owned enterprises (SOE), which increases in magnitude over time and is only significant in the post-1991 period (for the cold war, p=0.52).[33] The evidence is consistent with both *H1a* and *H1b* above: there is a difference in the coefficient on aid across time periods and no evidence that the relationship of oil with regime change follows the same pattern.

Differences are not only due to the failure to differentiate across time. A closer examination of the data used in Morrison (2009) suggests that they may not be well suited for testing hypotheses regarding foreign aid. The variable "Grants per capita," which is used as a measure of aid, contains large values of grants for OECD countries (which are not generally considered aid eligible under international definitions) and excludes aid in-kind that is given to developing countries (which is usually included in measures of aid). This creates a sizeable difference between this variable and standard measures of foreign aid. Additionally, about two-thirds of the possible observations are dropped from the analysis in a non-random way due to issues of missing data.[34] Even given these issues, the analysis reported in Table 4 shows no evidence of a significant negative relationship between the variable used to measure aid and the likelihood of regime change in the post-cold war period. Instead, disaggregating the data into the cold war and post-cold war periods shows results consistent with those hypothesized above.

Implications beyond Democratic Change

The studies by Bueno de Mesquita and Smith (2010) and Ahmed (2012) focus on survival of individual leaders or governments, respectively, rather than the likelihood of democratic change. The model developed above allows donor preferences regarding democratization to vary across time. It is not clear that the donor preference argument extends exactly to leader or government survival. Democratic change can occur without a change in leadership, and changes in government or leadership do not necessarily correspond with democratic change: many involve

[33]As above, interaction terms with cold war can be used instead of the sub-period analysis shown here and the conclusions hold; see supplemental appendix.

[34]These issues are substantive and discussed more fully in the supplemental appendix.

the transfer of power from one authoritarian ruler to another,[35] or the election of a new leader/party within a democracy.

Despite the difficulties in specifying an exact parallel with the model above, one implication from the model should apply: if aid was ever likely to prop up authoritarian leaders, it should not do so on average in the post-cold war period. Ahmed (2012) and Bueno de Mesquita and Smith (2010) claim that aid reduces the likelihood of government or leader turnover in authoritarian regimes. If this claim withstands scrutiny for the post-cold war period, it would pose a challenge to the main argument of this article.

Bueno de Mesquita and Smith (2010)

Bueno de Mesquita and Smith (2010) argue that governments are better able to dissipate revolutionary threats when they have "access to abundant, essentially labor-free resources (hereafter free resources) such as natural resource rents or foreign aid" (937). A re-examination of the analysis calls into question whether this claim holds in any period for foreign aid. Their central results on leader survival are reported in Models 2-4 of Table 1 in Bueno de Mesquita and Smith (2010); the relevant coefficients are reproduced here without alteration in Table 5, with the addition of the relevant p-values. The coding of leader survival is based on Goemans, Gleditsch and Chiozza (2009). Model 2 uses the Morrison (2009) aggregate measure of non-tax revenue; the coefficients for Nontax revenue (% GDP) and the interaction of nontax revenue with winning coalition size *(W)* from their Table 1, Model 2 are shown in Table 5. Models 3 and 4 drop the nontax revenue variable and instead include variables for oil exports as a percent of GDP (Models 3 and 4) and Aid as a percent of GDP (Model 4).[36] Data on foreign aid and oil for Models 3 and 4 are from the World Bank. The time period is 1972-2000 in Model 2 and 1962-2004 in Models 3 and 4.

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[35]Geddes, Wright and Frantz (2014).

[36]For a discussion of problems associated with including GDP in the denominator for such analyses, see Ross (2008).

[Table 5 about here.]

Based on these results, Bueno de Mesquita and Smith (2010) claim that "There is a negative coefficient on the free resources variable, be it measured as nontax revenue, Oil, or Aid. This indicates that if small-coalition leaders gain access to additional free resources then their risk of deposition is reduced" (943). Although they do note afterward that the "estimates on the free resource variables" in Model 4 are "insignificant" (944), there is *no* evidence that "each of these models reveals a similar pattern" as is claimed (943). While the coefficients on nontax revenue in Model 2 and oil in Model 3 are negative and significant, the negative coefficient on aid in Model 4 is much smaller and does not approach statistical significance (p=0.87). Additionally, the article claims that the impact of "free resources" varies with coalition size, W: evidence for this is the positive and significant coefficient on the interaction of W with non-tax revenue in Model 2. There is no such pattern for the coefficient on the corresponding term for $W*Aid$ in Model 4: the coefficient flips signs and is not significant (p=0.73). The study claims that "incumbents are most likely to survive when they are beholden to only a small coalition of supporters and when they have access to resources - such as oil and aid - that do not require significant economic participation by the citizens" (936). There is no evidence that this statement applies to foreign aid.

While it would be useful to examine the cold war and post-cold war periods separately, empirical difficulties arise when the underlying data are examined. The observations for Table 1 from Bueno de Mesquita and Smith (2010) are structured at the leader-year level, while the values for aid, oil, W, and other independent variables are at the country-year level. This means that the same value for each independent variable can apply to multiple leaders in the same year, making interpretation difficult. For instance, Haiti had three leaders in 1990 and each has a separate entry in the dataset. However, each entry has the same value for aid, income, W, and other variables. This is particularly problematic for W, which is coded at the end of the calendar year but applies that value to leaders from any portion of the same calender year.[37] When the analysis from

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[37]See supplemental appendix for further discussion.

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Bueno de Mesquita and Smith (2010), Table 1 is performed separately for the cold war and post-cold war periods (not shown), the key coefficients relating to foreign aid are not significant in either period. However, given the structure of the underlying data it is not clear that much weight should be placed on this result.

Bueno de Mesquita and Smith (2010) also analyze the relationship between "free resources" and institutional change - a change in the size of the winning coalition - in Table 2, Models 6-9 of their article. Once again there is no evidence that aid has the claimed effect, or that aid and oil revenue operate in a similar fashion. The bottom half of Table 5 shows results on the key terms from Models 7 and 8 reproduced exactly from the article: in Model 7 oil is the only "non-tax" resource included; Model 8 includes measures of both oil and aid. Each model also includes controls and several interaction terms including oil or aid. The authors wish to analyze "how revolutionary threats and free resources interact to affect institutional change over three years" (944). Revolutionary threats are measured as the change in the level of mass political movements over the previous three years *(Δmass)*. The expectation is that mass political movements cause an increase in the size of the winning coalition (measured three years in the future), but that non-tax revenue will negate this democratizing effect of mass movements. Hence, the study hypothesizes a negative coefficient on interactions between forms of non-tax revenue (e.g. *oil* and/or *aid*) and *Δmass*. However, this is expected to only operate in countries that start out with a small winning coalition *(W)*; if the country is already democratic (*W* is large) then non-tax revenue will not have this negative effect. Therefore, a further hypothesis is for a positive coefficient on the triple interaction between *W, oil* or *aid,* and *Δmass.*

When interpreting the coefficient on the variable *Oil*Δmass* in Model 7, the authors write that "the negative coefficient on the interaction variable, Δmass*Oil, indicates that when a leader has access to free resources, increases in mass political movements are likely to result in contractions rather than expansions of the winning coalition" (944). They note that this result only holds in small coalition systems: the positive coefficient on the term *W*Oil*Δmass* shows that as coalition size increases the negative effect disappears. This finding actually applies only to

revenue from oil resources, not to foreign aid. Table 5 shows the coefficients on the relevant interaction terms for aid in Model 8: *Aid*Δmass* and *W*Aid*Δmass*. In each case the coefficients are insignificant and *of opposite sign* to the corresponding interactions for oil.[38] This point is not noted, however. Instead, the article refers to the "pernicious effects of free resources in retarding democratization" (946).

In their abstract Bueno de Mesquita and Smith (2010) claim that "Tests of leader survival indicate that revolutionary threats increase the likelihood of deposition for nondemocratic leaders. Leaders with access to resources such as foreign aid or natural resource rents are best equipped to survive these threats and avoid the occurrence of these threats in the first place." Revisiting the results suggests that this claim is unwarranted with respect to foreign aid.

Ahmed (2012)

The argument in Ahmed (2012) differs from Bueno de Mesquita and Smith (2010) by making the case that aid and remittances, rather than aid and oil revenue, have a similar relationship with the likelihood of government survival. The paper examines the period 1975-2004. The preferred dependent variable is a dichotomous measure that equals 1 if there is government turnover according to the measure of years in office included in the Database of Political Institutions. Based on the statistical results, the article claims that aid and remittances can be combined into a single measure when evaluating their impact on the likelihood of government turnover.

Model 3 in Table 3 of Ahmed (2012) is the only model that includes aid and remittances separately. The article hypothesizes that aid and remittances will have a negative effect on the likelihood of government turnover in authoritarian regimes. Thus, the interest is in the combination of the coefficient on aid or remittances and the coefficient on its interaction with authoritarianism (measured as the inverse of the Polity2 scale, with higher scores representing more authoritarian regimes). As the article notes, none of these coefficients are significant in

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[38]When the analysis is performed for the post-cold war period (not shown), the effects of oil continue to be significant and those for aid insignificant.

Model 3. Despite this, it is determined that the coefficients on the interaction terms Autocracy*aid and Autocracy*remittances are "not different" and that this justifies aggregation of aid and remittances in all other models. The replication files show that a Wald test for equality between the two coefficients has a p-value of 0.462: while the null of equality cannot be rejected, there is no evidence that the two coefficients are, in fact, equal.

There are issues with the model and underlying data that must be addressed before further analysis. All models in Table 3 of Ahmed (2012) lose large numbers of observations due to separation induced by including country, year, and duration fixed effects in the Probit models. This is similar to selecting on the dependent variable - countries, years, or duration spells that do not experience change are dropped from the analysis and have no impact on the estimated coefficients.[39] In Model 3 of Table 3 from Ahmed (2012), separation results in the loss of 664 observations, or 29% of the data; it also reduces the country coverage from 120 to 97 since countries that never experience a change are dropped.[40]

It is possible to correct for separation and examine the relationship between aid, remittances, and the likelihood of government turnover using the data from Ahmed (2012).[41] However, additional alterations to the original data and methods are required to properly perform this test. The re-scaling of the Autocracy measure from the original Polity2 values to a new (inverted) scale ranging from 0 to 1 (with 1 as the most autocratic) in Ahmed (2012) was not done in a linear

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[39]This is different from the coefficient estimates usually produced when fixed effects are included: those disregard units (e.g. countries) for which an *independent* variable does not change when calculating the coefficient for that variable; separation drops observations based on lack of variation on the *dependent* variable (Beck and Katz, 2001; Carter and Signorino, 2010).

[40]See supplemental appendix for further discussion of how separation affects results in the original paper.

[41]The supplemental appendix shows the results of correcting for separation in Model 3 without further alteration.

fashion. The movement from -10 to -9 is associated with a change from 1 to 0.5 (a difference of 0.5), while a movement from 9 to 10 is associated with a move from 0.050 to 0.048 (a difference of 0.002). This means that the difference between a -10 and -9 on Polity2 is more than 200 times the difference between 9 and 10 in the rescaled score.[42] Instead of relying on this score, a linear scale is created in which each difference of 1 on the original -10 to 10 scale corresponds to a difference of .05 on the (inverted) 0 to 1 scale. It is necessary to lag the Autocracy variable given the coding structure of that variable and the dependent variable (government turnover), but it does not appear that this was done in the original analysis. An observation is coded as 1 for turnover if it experienced a turnover that year. However, Polity2 - on which the autocracy score is based - is coded as of December 31 of the year in which it is reported. Therefore, the Autocracy score should be lagged by one period to ensure that it is the score *prior to* the turnover that is used, rather than the score *resulting from* the turnover. Similar concerns arise from the failure to lag aid and remittances, some of which may also accrue after the turnover in a given year.

The analysis shown in Table 6 employs data drawn from the replication files for Ahmed (2012), but uses the re-scaled Autocracy score, lags all independent values by one period to ensure they are prior to the turnover, and does not include fixed effects due to the severity of separation induced by their inclusion. To model the hazard without duration fixed effects, duration polynomials based on the replication data are used.[43] The results in Table 6 show no evidence that aid significantly decreases the likelihood of government turnover in authoritarian regimes in any period: the coefficient on Autocracy*Aid is never significant.[44] Looking at

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[42]See supplemental appendix for additional discussion of this and other data issues.

[43]Relevant variables reported here; see supplemental appendix for full table.

[44]This difference from the original article is not due to removing the fixed effects. When the models in Table 6 are estimated using OLS with country, year and duration fixed effects the differences between what is found for both aid and remittances and the results from the original article are even more striking. See supplemental appendix.

Columns 2 and 3, which break out the cold war and post-cold war periods, a pattern is observed for aid that is consistent with the hypothesis of a change over time, although most coefficients are insignificant. Still, it is worth noting that the coefficient on Autocracy*Aid in the post-cold war is not only insignificant but also in the opposite direction from that hypothesized by Ahmed (2012). And the negative, significant coefficient on Aid in the post-cold war means that when Autocracy is zero (Polity2 score is 10, the most democratic), aid decreases the likelihood of turnover: so aid may decrease the likelihood of turnover in democracies, but not in autocracies - opposite to the predictions in Ahmed (2012). This same pattern for aid is seen if the remittance variables are excluded from the models in Table 6.

[Table 6 about here.]

Summary   The articles revisited in this section claim that foreign aid operates in a similar manner to oil revenue or remittances and that it suppresses political change in authoritarian countries. In no case does a further examination of the results support the conclusion that aid prevents political change in the post-cold war, or that aid operates in a similar way to other forms of non-tax revenue.

Conclusion

Foreign aid, unlike revenue from state-owned oil enterprises or migrant remittances, comes from a donor country. Donors have preferences, which can change over time as the geopolitical climate evolves. The effects of aid will be conditioned by donor priorities and donor-recipient relationships. Failure to account for this omits a key theoretical component from our understanding of foreign aid and its impact in recipient countries.

Policymakers in developed democracies face difficult choices when operating in nondemocratic countries. Understanding any unintended consequences of their policies on the political situation is important. For aid donors, this is particularly true. A substantial proportion of the world's poor live in nondemocratic countries. Several previous studies conclude that giving aid in these countries reduces the likelihood of democratic change. If decreasing the likelihood of

democratization is an unavoidable side effect of giving aid in authoritarian states, then donors will think seriously about these consequences when allocating aid.

The argument developed here predicts a more nuanced relationship between aid and democratic change. Furthermore, it suggests that donors are a driving force in determining this relationship. Donors can alter the composition of aid both over time and across recipients, varying the extent to which authoritarian governments can use aid to their own advantage. Empirical tests support the theory: aid to authoritarian recipients need not prevent democratic change. Evidence from the cold war and from strategically important recipients provides a cautionary tale regarding the ability of aid to have anti-democratic properties. However when a donor's utility is increased by avoiding these consequences, it is able to re-allocate aid within authoritarian recipients to prevent anti-democratic effects. This is seen in the post-cold war period where aid, on average, is not associated with decreased likelihood of democratic change.

The comparison of the effects of aid with those of oil revenue provides important insights for understanding the fungibility of aid. Directly ascertaining the level of aid fungibility in a generalizable manner has proved elusive. While fungibility is often asserted in scholarly work, the extent of fungibility remains contested. The findings here suggest limits on fungibility, at least in nondemocratic recipients: aid does not have similar effects to oil revenue, which is more widely accepted as a fungible resource. Instead, the results are more consistent with a theory incorporating donor preferences and the heterogeneous nature of aid than with previous theories which assumed aid was a homogenous, fungible resource.

Foreign aid is not oil. It involves a resource transfer from one state to another. As such, the priorities of governments in *both* states must be considered when developing a theory of the effects of foreign aid. Researchers should resist the temptation to aggregate across types of resources and instead consider the nuances associated with each when studying their impact on outcomes of interest.

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	All Years	Cold War	post-Cold War
	Model 1	Model 2	Model 3
ln Aid per capita (lag 2)	-0.058	-0.235**	0.176
	(0.43)	(0.01)	(0.22)
ln Oil Rev per capita (lag 2)	-0.081**	-0.067	-0.136**
	(0.03)	(0.20)	(0.03)
ln Income (lag 2)	0.179*	0.383***	0.143
	(0.06)	(0.00)	(0.46)
lagged Growth	-0.027***	-0.022**	-0.003
	(0.00)	(0.03)	(0.82)
lagged Polity	-0.115***	-0.080**	-0.190***
	(0.00)	(0.02)	(0.00)
Previous Transitions	0.283***	0.246***	0.337***
	(0.00)	(0.00)	(0.00)
Year	0.019**	0.078**	0.007
	(0.03)	(0.02)	(0.77)
Age	-0.074**	-0.098***	-0.065
	(0.01)	(0.01)	(0.19)
Age Squared	0.001	0.002	0.001
	(0.21)	(0.12)	(0.67)
Age Cubed	-0.000	-0.000	-0.000
	(0.47)	(0.38)	(0.89)
Constant	-42.512**	-159.775**	-18.972
	(0.02)	(0.02)	(0.70)
Observations	3488	1645	1666
Countries	129	107	117

Table 1: Aid, Oil, and Democratic Change. Dependent variable equals 1 if the Polity2 score changed by three or more points in a positive direction from t-1 to t. Logit analysis, robust standard errors clustered on recipient; p-values in parentheses. $*p < 0.10$; $**p < 0.05$; $***p < 0.01$

	Change in Polity2		Change in Freedom House	
	Cold War	post-Cold War	Cold War	post-Cold War
	Model 4	Model 5	Model 6	Model 7
ln Aid per capita (lag 2)	-0.205**	0.082	-0.042*	0.001
	(0.02)	(0.14)	(0.09)	(0.94)
ln Oil Rev per capita (lag 2)	-0.116	-0.039	-0.002	-0.034*
	(0.22)	(0.57)	(0.95)	(0.05)
ln Income (lag 2)	0.260	-0.032	-0.002	0.042
	(0.46)	(0.89)	(0.98)	(0.49)
lagged Growth	-0.005	-0.003	0.001	0.000
	(0.30)	(0.62)	(0.66)	(0.94)
lagged Polity	-0.208***	-0.272***		
	(0.00)	(0.00)		
lagged Freedom House			-0.264***	-0.265***
			(0.00)	(0.00)
Year	0.068***	0.030***	0.015***	0.009***
	(0.00)	(0.01)	(0.00)	(0.01)
Constant	-136.195***	-60.063***	-28.182***	-17.046***
	(0.00)	(0.01)	(0.00)	(0.01)
Observations	1884	2277	1788	2415
Countries	113	138	119	144
Country fixed effects	yes	yes	yes	yes

Table 2: Models including country fixed effects. Dependent variable is the change in Polity2 score or in the (inverted) Freedom House average from t-1 to t. Robust standard errors clustered on recipient; p-values in parentheses. $*p < 0.10$; $**p < 0.05$; $***p < 0.01$

	Cold War Model 8	post-Cold War Model 9	Cold War Model 10	post-Cold War Model 11
ln Aid per capita (lag 2)	-0.207**	0.222	-0.214**	0.221
	(0.03)	(0.16)	(0.03)	(0.16)
Top 5 US Mil Aid (lag 2)	3.008**	8.089***	3.574*	7.183**
	(0.03)	(0.00)	(0.06)	(0.05)
Top 5*Aid (lag 2)	-0.699	-2.089***	-1.057	-1.968**
	(0.14)	(0.00)	(0.14)	(0.02)
ln Oil Rev per capita (lag 2)	-0.039	-0.151**	-0.056	-0.152**
	(0.48)	(0.02)	(0.31)	(0.02)
Top 5*Oil (lag 2)			0.329	0.107
			(0.16)	(0.66)
ln Income (lag 2)	0.330**	0.131	0.360***	0.131
	(0.02)	(0.52)	(0.01)	(0.52)
lagged Growth	-0.027**	-0.002	-0.026**	-0.002
	(0.01)	(0.90)	(0.02)	(0.90)
lagged Polity	-0.072**	-0.201***	-0.080**	-0.201***
	(0.04)	(0.00)	(0.03)	(0.00)
Previous Transitions	0.211**	0.341***	0.222**	0.343***
	(0.02)	(0.00)	(0.01)	(0.00)
Year	0.074**	0.002	0.073**	0.002
	(0.03)	(0.93)	(0.04)	(0.94)
Age	-0.092**	-0.059	-0.093**	-0.058
	(0.01)	(0.24)	(0.01)	(0.25)
Age Squared	0.002	0.001	0.002	0.001
	(0.14)	(0.73)	(0.14)	(0.75)
Age Cubed	-0.000	-0.000	-0.000	-0.000
	(0.39)	(0.93)	(0.40)	(0.94)
Constant	-150.419**	-8.921	-148.690**	-8.503
	(0.03)	(0.85)	(0.03)	(0.86)
Observations	1645	1666	1645	1666
Countries	107	117	107	117

Table 3: Aid and Democratic Change for Heterogenous Recipients. Dependent variable equals 1 if the Polity2 score changed by three or more points in a positive direction from t-1 to t. Logit analysis, robust standard errors clustered on recipient; p-values in parentheses. $*p < 0.10$; $**p < 0.05$; $***p < 0.01$

Based on Morrison (2009), Table 3, Model 1			
	Replication	Cold War	Post-Cold War
	(1)	(2)	(3)
Grants per capita (t-1)	-0.0175**	-0.0664**	-0.0049
	(0.008)	(0.031)	(0.011)
SOE Rev per capita(t-1)	-0.0016**	-0.0009	-0.0195***
	(0.001)	(0.001)	(0.007)
Other non-tax Rev per capita(t-1)	-0.0012**	-0.0021**	0.0015
	(0.001)	(0.001)	(0.001)
Observations	1307	747	419
Countries	98	77	73

Table 4: Analysis of Morrison (2009). Dependent Variable equals one if there is a three-point change in Polity in either direction, 0 otherwise. Logit Analysis, robust standard errors clustered by country in parentheses. First column replicates Table 3, Model 1 from Morrison (2009). Columns 2 and 3 use the same data but for pre-1989 and post-1991, respectively. $*p < 0.10$; $**p < 0.05$; $***p < 0.01$

From Bueno de Mesquita and Smith (2010), Table 1: Leader Survival (parametric Weibull model)			
	Independent Variable	Coefficient	p-value
Model 2	Nontax Revenue (%GDP)	-0.0601	0.024
Model 2	W*Nontax Revenue	0.0808	0.046
Model 3	Oil(exports as %GDP)	-0.0201	0.051
Model 3	W*Oil	0.0141	0.380
Model 4	Aid(%GDP)	-0.00427	0.872
Model 4	W*Aid	-0.0135	0.733
From Bueno de Mesquita and Smith (2010), Table 2: Future Coalition Size, W (3 years)			
	Independent Variable	Coefficient	p-value
Model 7	Oil*Δmass	-0.00329	0.000
Model 7	W*Oil*Δmass	0.00481	0.002
Model 8	Aid*Δmass	0.00148	0.573
Model 8	W*Aid*Δmass	-0.00178	0.658

Table 5: Reproduced using data from Bueno de Mesquita and Smith (2010), Tables 1 and 2.

	Full Period	Pre-1989	Post-1991
	Column 1	Column 2	Column3
Autocracy	-0.995***	-0.712***	-1.385***
	(0.00)	(0.00)	(0.00)
Aid (% GDP) (lagged)	-0.010	0.003	-0.020*
	(0.22)	(0.85)	(0.08)
Autocracy*Aid (lagged)	0.003	-0.018	0.012
	(0.83)	(0.41)	(0.56)
Remittances (% GDP) (lagged)	-0.008	-0.026	0.008
	(0.78)	(0.75)	(0.81)
Autocracy*Remittances (lagged)	-0.038	0.007	-0.116
	(0.44)	(0.95)	(0.27)
Observations	2333	968	1113

Table 6: Based on the analysis of Ahmed (2012). Probit analysis, robust standard errors clustered on government (not reported); p-values in parentheses. Dependent variable equals 1 if there was a government turnover in year t. All independent variables are lagged one period. $*p < 0.10$; $**p < 0.05$; $***p < 0.01$

Supplemental Appendix[45]

Notes on Dataset Creation

Only aid-eligible, independent countries (not territories) are included. If a country becomes independent after 1973 it appears only for the years following independence. If a country moves to "high income" status after 1973 and becomes ineligible for foreign aid, it is included only for the years of eligibility as no aid data are available for other years. A few countries no longer in existence, most notably in Eastern Europe (e.g. Czechoslovakia), are not included in the OECD data under their former names but are included where appropriate under the new country names (e.g. Czech Republic and Slovak Republic before they became high-income countries); a few countries in Eastern Europe/Central Asia lack data for the first few years following independence, so there is a lag in their inclusion (e.g. Azerbaijan is included from 1995-2010). The Polity IV database only codes countries that have a population above 500,000; as a result, some small states are excluded from the analysis. From 1973-1987 there are a few oil-rich countries (such as Saudi Arabia and Kuwait) that lack data on income per capita and so are not included for these years. This is unfortunate, but their inclusion would not change the central findings here, which are that in the post-cold war period (for which data availability is not a problem) there is no evidence that aid entrenches authoritarian regimes. Their inclusion would likely increase the negative effect of oil during the cold war - in fact, their absence may explain the weaker finding on the oil coefficient in the cold war.

To construct the variable of aid per capita, I use table DAC 3a and include total aid (all donors) minus aid from the Arab Fund (AFESD), the Arab Bank for Economic Development in Africa (BADEA), the Islamic Development Bank, Kuwait, Saudi Arabia, and the United Arab Emirates. As the theory pertains only to democratic donors, the exclusion of these largely authoritarian donors is appropriate.

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[45]This supplemental appendix, data and replication files will be made available online following publication.

While missing data almost always result in the loss of observations, every effort has been made to minimize the problem here. There are 3,699 possible country-year observations for the analysis in Table 1: observations where the county-year is coded by Polity, the lagged polity2 score is below 8 (to allow for the possibility of a 3 point increase), and the country is aid-eligible according to the OECD. Of these, only 211 observations are excluded from the analysis; 3,488 (94%) are included. Of the excluded country-years, 30 had not been independent for 2 years yet (so two-year lag could not be generated), 171 others are missing data on income per capita (lagged), 9 others are missing data to compute the dependent variable for the year, and one other is missing data on aid per capita (2 year lag). This compares favorably to other studies revisited in the text, where the percent of possible observations actually included in the analyses are lower.

Separation   The problem of separation when including fixed effects is referred to in connection with Table 1 in the main article, where fixed effects are not included. Their inclusion would result in loss of a sizeable number of observations and countries. If country fixed effects are included in the models from Table 1 (main article), Model 1 drops from 3488 observations (129 countries) to 2482 observations (85 countries), Model 2 drops from 1645 observations (107 countries) to 769 observations (49 countries) and Model 3 from 1666 observations (117 countries) to 686 observations (47 countries). For this reason alternative specifications are used in Table 3 in the main article.

Endogeneity   Endogeneity is always a potential concern. Like scholars doing closely related work, this analysis lags independent variables. For reverse causation to occur given this structure, it would be necessary for democratic change between t-1 and t to cause aid in period t-2 (before the change occurs). As the theory here predicts a change in the relationship between aid and the likelihood of democratic change over time or across recipients, any concerns regarding reverse causation would need to explain how democratization *causes* aid in the previous year differently across time/countries. There is one potential way this could occur. If donors are "picking winners" with regard to democratization then they would increase aid to countries more likely to experience a positive democratic change in the near future. For this to result in an empirical

relationship, donors must actually be good at picking winners *ex ante* - a questionable assumption. For this reverse causation to account for changes over time, donors must pick winners in the post-cold war differently than in the cold war. This would be consistent with an increasing preference for democratization in the post-cold war period. In other words, it could be thought of as the opposite side to the negative externality from the model: donors also experience a positive externality when democratization occurs, and that externality is worth more in the post-cold war period. Therefore, even if reverse causation is behind the relationship, it is consistent with changing donor preferences regarding the aid-democratization relationship over time, with donors choosing to invest in likely democratization differently across periods. Difficulties in teasing out the causal direction of this relationship should rightly temper any claims that aid can cause democratization, but no such strong causal claim is advanced here. Instead, the analysis here examines whether more aid is associated with less democratic change in some situations relative to others - which is consistent with donors giving aid differently across periods to states likely to democratize.

I do not instrument for foreign aid. Ahmed (2012) instruments for aid with changes in oil prices, as aid increases when oil states have more money to give. As it is aid from authoritarian, oil-rich donors that is likely to be most sensitive to these changes, it is not appropriate as an instrument for aid from the democratic donors studied here (Bermeo, 2011).

	Mean	Std. Dev.	Minimum	Maximum
Indicator: Three point positive change in Polity	0.051	0.220	0	1
ln Aid per capita (lag 2)	3.593	1.426	0	7.779
ln Oil Rev per capita (lag 2)	2.338	2.977	0	10.038
ln Income (lag 2)	7.688	1.079	5.081	11.071
lagged Growth	1.684	8.471	-64.408	115.42
lagged Polity	-2.935	5.592	-10	7
Previous Transitions	0.923	1.275	0	8
Year	1991.5	10.752	1973	2010
Age	15.559	17.346	0	105

Table A.1: Summary statistics using data from Table 1, Model 1.

Additional Tables referred to in Main Text

	1973-2010	Excludes 1991&1992
	Column 1	Column 2
ln Aid per capita (lag 2)	0.202*	0.138
	(0.05)	(0.29)
Aid*Cold War	-0.431***	-0.377**
	(0.00)	(0.01)
ln Oil Rev per capita (lag 2)	-0.115***	-0.159***
	(0.01)	(0.00)
Oil*Cold War	0.080	0.098
	(0.15)	(0.10)
Cold War	0.899	1.719**
	(0.19)	(0.02)
ln Income (lag 2)	0.211**	0.308***
	(0.03)	(0.00)
lagged Growth	-0.028***	-0.018*
	(0.00)	(0.08)
lagged Polity	-0.130***	-0.135***
	(0.00)	(0.00)
Previous Transitions	0.291***	0.300***
	(0.00)	(0.00)
Year	0.005	0.049**
	(0.78)	(0.02)
Age	-0.076**	-0.093***
	(0.02)	(0.00)
Age Squared	0.001	0.002
	(0.25)	(0.13)
Age Cubed	-0.000	-0.000
	(0.52)	(0.39)
Constant	-15.194	-102.873**
	(0.68)	(0.01)
Observations	3488	3311
Countries	129	129

Table A.2: Interaction Alternative: Test similar to Table 1, but estimated with the interaction terms Aid*Cold War and Oil*Cold War rather than splitting the sample into cold war and post-cold war. The Cold War variable is an indicator that takes the value of 1 prior to 1991; all other variables are identical to Table 1. Column 1 is for all observations included in Model 1 of the main paper. Column 2 excludes the years 1991 and 1992, which were excluded in the sub-period analyses of Models 2 and 3 in the main paper. Including these years (Column 1) increases the difference in the effects of aid from the cold war to the post cold war (so excluding them in Models 2 and 3 of the paper is a more conservative approach). These were years of large aid and large transitions, but likely due to non-generalizable changes in the immediate aftermath of the cold war - which is why they are excluded in Models 2 and 3. The coefficient on Aid per capita reflects the relationship between aid and democratic change when cold war equals zero: we can see that there is no evidence of a negative relationship between aid and change in the post cold war period. The results of a Wald test on the coefficients for Aid + Aid*Cold War suggest in favor of rejecting the null of zero relationship in the cold war for both Column 1 (p=0.01) and Column 2 (p=0.01); on net there is a negative relationship between aid and the likelihood of change in the cold war. Thus, the pattern of a negative, significant relationship in the cold war and a positive relationship (although not necessarily significant) in the post cold war is the same as reported in Table 1. Logit analysis, robust standard errors clustered on recipient (not reported); p-values in parentheses. Dependent variable equals 1 if the Polity2 score changed by three or more points in a positive direction from t-1 to t. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$

	2002-2010
ln Aid per capita (lag 2)	0.399**
	(0.04)
ln Oil Rev per capita (lag 2)	-0.125
	(0.12)
ln Income (lag 2)	0.207
	(0.49)
lagged Growth	-0.023
	(0.53)
lagged Polity	-0.225***
	(0.00)
Previous Transitions	0.410***
	(0.01)
Year	0.118
	(0.17)
Age	-0.168**
	(0.03)
Age Squared	0.004
	(0.15)
Age Cubed	-0.000
	(0.30)
Constant	-243.108
	(0.16)
Observations	780
Countries	101

Table A.3: Post-2001: analogous to Table 1, but restricted to the post-2001 "war on terror" period. Robust standard errors clustered on recipient (not reported); p-values in parentheses. Dependent variable equals 1 if the Polity2 score changed by three or more points in a positive direction from t-1 to t. $*p < 0.10$; $**p < 0.05$; $***p < 0.01$

	All Years	Cold War	post-Cold War
ln Aid per capita (lag 2)	0.008	-0.022*	0.009
	(0.22)	(0.07)	(0.31)
ln Oil Rev per capita (lag 2)	-0.002	-0.007	0.005
	(0.68)	(0.66)	(0.53)
ln Income (lag 2)	-0.019	0.011	-0.031
	(0.28)	(0.82)	(0.24)
lagged Growth	-0.001***	-0.001**	-0.000
	(0.01)	(0.03)	(1.00)
lagged Polity	-0.008***	0.002	-0.022***
	(0.00)	(0.57)	(0.00)
Previous Transitions	-0.046***	-0.136***	-0.070***
	(0.00)	(0.00)	(0.01)
Year	0.005***	0.008***	0.006***
	(0.00)	(0.00)	(0.00)
Age	-0.005**	-0.006	-0.004
	(0.02)	(0.13)	(0.15)
Age Squared	0.000	0.000	0.000
	(0.26)	(0.42)	(0.76)
Age Cubed	-0.000	-0.000	-0.000
	(0.65)	(0.79)	(0.79)
Constant	-9.530***	-15.590***	-11.762***
	(0.00)	(0.00)	(0.00)
Observations	3488	1645	1666
Countries	129	107	117
Country Fixed Effects?	yes	yes	yes

Table A.4: OLS with Country Fixed Effects: Analogous to Table 1 but estimated using OLS with country fixed effects. Robust standard errors clustered on recipient (not reported); p-values in parentheses. Dependent variable equals 1 if the Polity2 score changed by three or more points in a positive direction from t-1 to t. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$

	Cold War Column 2	post-Cold War Column 3
ln Aid per capita (lag 2)	-0.231**	0.170
	(0.02)	(0.23)
ln Oil Rev per capita (lag 2)	-0.065	-0.133**
	(0.21)	(0.03)
ln Income (lag 2)	0.383***	0.149
	(0.00)	(0.44)
lagged Growth	-0.022**	-0.002
	(0.03)	(0.91)
lagged Polity	-0.078**	-0.186***
	(0.03)	(0.00)
Previous Transitions	0.245***	0.332***
	(0.00)	(0.00)
Year	0.077**	0.006
	(0.02)	(0.81)
Age	-0.095***	-0.065
	(0.01)	(0.18)
Age Squared	0.002	0.001
	(0.14)	(0.64)
Age Cubed	-0.000	-0.000
	(0.47)	(0.89)
Constant	-157.253**	-16.350
	(0.02)	(0.74)
Observations	1645	1666
Countries	107	117

Table A.5: Rare Events Logit: Analogous to Table 1, Models 2 and 3, but estimated with a Rare Events Logit. Robust standard errors clustered on recipient (not reported); p-values in parentheses. Dependent variable equals 1 if the Polity2 score changed by three or more points in a positive direction from t-1 to t. $*p < 0.10$; $**p < 0.05$; $***p < 0.01$

	All Years Column 1	Cold War Column 2	post-Cold War Column 3
ln Aid per capita (lag 2)	-0.191**	-0.387***	0.140
	(0.04)	(0.00)	(0.58)
ln Oil Rev per capita (lag 2)	-0.137***	-0.134**	-0.104
	(0.01)	(0.05)	(0.31)
ln Income (lag 2)	0.217*	0.418***	-0.014
	(0.08)	(0.00)	(0.97)
lagged Growth	-0.027***	-0.031**	0.001
	(0.00)	(0.02)	(0.95)
lagged Polity	-0.101***	-0.091	-0.235***
	(0.00)	(0.15)	(0.00)
Previous Transitions	0.269***	0.395***	0.198
	(0.00)	(0.00)	(0.20)
Year	0.019*	0.125***	0.039
	(0.07)	(0.00)	(0.26)
Age	-0.031	-0.064	-0.221
	(0.40)	(0.16)	(0.10)
Age Squared	-0.000	0.001	0.016
	(0.90)	(0.60)	(0.24)
Age Cubed	0.000	0.000	-0.000
	(0.66)	(0.94)	(0.30)
Constant	-42.876**	-253.788***	-81.480
	(0.05)	(0.00)	(0.24)
Observations	2994	1539	1297
Countries	118	101	95

Table A.6: Five point change in Polity: Analogous to Table 1, but the dependent variable equals 1 if the Polity2 score changed by five or more points in a positive direction from t-1 to t (instead of the 3 point change in Table 1). Logit analysis, robust standard errors clustered on recipient (not reported); p-values in parentheses. To avoid clutter I do not include all regressions that were estimated using different changes in the polity2 score to code the dependent variable. However, when the dependent variable is set to equal 1 for a two-or-more point shift in polity2 score, the coefficient on aid for the cold war period is -0.179 (p=0.053) and for the post-cold war it is 0.116 (p=0.344); for a four-or-more point shift, the coefficient on aid for the cold war is -0.336 (p=0.004) and for the post-cold war it is 0.243 (p=0.181); for a six-or-more point shift, the coefficient on aid for the cold war is -0.454 (p=0.00) and for the post cold war 0.281 (p=.276). *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$

	Cold War Column 2	post-Cold War Column 3
ln Aid per capita (lag 2)	-0.112	0.342*
	(0.33)	(0.06)
ln Oil Rev per capita (lag 2)	-0.090*	-0.173***
	(0.08)	(0.01)
ln Population (lag 2)	0.213***	0.253
	(0.01)	(0.11)
ln Income (lag 2)	0.538***	0.321
	(0.00)	(0.18)
lagged Growth	-0.024**	-0.007
	(0.03)	(0.70)
lagged Polity	-0.090**	-0.197***
	(0.01)	(0.00)
Previous Transitions	0.180**	0.272***
	(0.03)	(0.01)
Year	0.075**	0.004
	(0.03)	(0.86)
Age	-0.099***	-0.068
	(0.01)	(0.17)
Age Squared	0.002	0.000
	(0.11)	(0.76)
Age Cubed	-0.000	0.000
	(0.35)	(0.95)
Constant	-159.126**	-19.244
	(0.02)	(0.69)
Observations	1645	1666
Countries	107	117

Table A.7: Including population: analogous to Table 1, Models 2 and 3, but includes the natural log of a recipient country's population. Robust standard errors clustered on recipient (not reported); p-values in parentheses. Dependent variable equals 1 if the Polity2 score changed by three or more points in a positive direction from t-1 to t. Note that the direction of the change on the aid coefficient over time is the same as in Table 1, but in this case it is from a negative, insignificant value in the cold war to a positive, significant value in the post cold war; the coefficient on oil is negative in both periods.*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$

	All Years Column 1	Cold War Column 2	post-Cold War Column 3
ln Aid per capita (lag 2)	0.053	0.089	-0.037
	(0.62)	(0.67)	(0.81)
ln Oil Rev per capita (lag 2)	0.063	-0.007	0.112
	(0.20)	(0.94)	(0.14)
ln Income (lag 2)	-0.554***	-0.485*	-0.794***
	(0.00)	(0.09)	(0.00)
lagged Growth	0.008	0.002	0.005
	(0.58)	(0.89)	(0.85)
lagged Polity	0.131***	0.120***	0.163***
	(0.00)	(0.00)	(0.00)
Previous Transitions	-0.077	-0.200	-0.060
	(0.48)	(0.24)	(0.66)
Year	-0.053***	-0.084**	-0.043
	(0.00)	(0.03)	(0.23)
Age	-0.169**	-0.375*	-0.112
	(0.02)	(0.07)	(0.22)
Age Squared	0.005	0.021	0.006
	(0.22)	(0.45)	(0.23)
Age Cubed	-0.000	-0.001	-0.000
	(0.29)	(0.49)	(0.21)
Constant	106.953***	167.305**	87.744
	(0.00)	(0.03)	(0.22)
Observations	3723	1409	2124
Countries	132	98	130

Table A.8: Democratic Failure: Analogous to Table 1, but the dependent variable equals 1 if the Polity2 score changed by three or more points in a *negative* direction from t-1 to t. Logit analysis, robust standard errors clustered on recipient (not reported); p-values in parentheses. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$

Notes on Replication Data Used

This section contains notes on the replication data used in the three studies referred to in the main paper, Morrison (2009), Bueno de Mesquita and Smith (2010), and Ahmed (2012). No dataset is perfect, and tradeoffs are almost always made between, for instance, country coverage and inclusion of variables. However, in this case it is troubling that what has started to amount to a "robust" finding on the negative political effects of aid in authoritarian countries is drawn from datasets that might not, on further examination, be best suited to answering the question at hand.

Morrison (2009)

Morrison (2009) uses a variable called "Grants per capita" as a measure of foreign aid. The measure appears to capture forms of non-tax revenue, and may be an appropriate measure for testing hypotheses about such revenue (which is the main point of that article). However, the revenue included in this variable differs significantly from what is usually considered "foreign aid" - which is almost always defined as "official development assistance" following the definitions used by the OECD. Of the 98 countries in Model 1 of Morrison (2009), 20 are OECD countries, accounting for 34% of the observations.[46] Common definitions of foreign aid exclude OECD countries from aid eligibility. Yet, the average value of "Grants per capita" (lagged) for observations in Column 1 that are OECD countries is $109; for non-OECD countries it is $44. Based on the average grants per capita for a country across observations included in Column 1, 14 of the top 20 recipients of grants were OECD countries, including average yearly grants per capita (lagged) of $542 for Switzerland, $184 for Ireland, $169 for Denmark, $168 for Finland, $157 for Portugal, etc. This is problematic as many OECD countries score a "10" on the Polity scale and never experience a regime transition, meaning that their high values of "aid" are associated with lack of regime change. For other countries, a portion of what is considered aid by international definitions is not included in "Grants per capita." The observations included in Model 1 for Egypt have average grants per capita of $10.53; averaging across those same years the World Bank's

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[46]The classification into OECD and non-OECD is based on coding in the article's replication dataset.

World Development Indicators shows $46.09 in "Net ODA received per capita (current US$)".

For the Republic of Congo, the average value of "Grants per capita" is $3.42; the average in the WDI for the same years is $74.45. Returning to the definition of "Grants" provided by Morrison (2009) and the IMF documentation to which it refers may provide an explanation for the inclusion of OECD countries and the discrepancies for developing countries. Morrison (2009) quotes the IMF definition of grants as "all nonrepayable unrequited payments received from other governments or programs, for general budget support, or any other purpose" (116, Table 2; quoted from IMF (1986) (130)). This definition of grants applies to both grants from abroad as well as to grants "from other levels of national government", "from supranational authorities to member countries", and "to supranational authorities from their headquarters" (IMF, 1986, (130-131)). Therefore, subsidies from the EU to member states would count as "Grants per capita" as used in Morrison (2009), but would not count as "aid" in any database of official development assistance. Additionally, "grants in kind, for example those received in the form of civil or military equipment, materials, technical assistance or donor government construction of facilities, with no payment to the recipient government, are not included here but in a memorandum item for grants in kind" (130). This suggests that a sizeable portion of foreign aid from grants - those given in-kind - is not captured by the "Grants" category but instead by the "other non-tax revenue" category. This variable also shows a significant, negative relationship with the likelihood of regime change during the cold war only (see Table 4 in the main paper).

The inclusion of only 78 non-OECD countries for the total period - 57 in the cold war and 54 in the post-cold war - is also problematic for drawing conclusions more widely about developing countries. The exclusion of observations is not random. There are 1,307 observations included in Model 1 of the original article. For the same years and criteria, there are 2,662 observations in the replication database with values on the lagged polity2 variable that are not included. The mean lagged Polity2 value for included observations is 3.377; the mean for the excluded observations is -1.620 (t-test of the difference between means yields $p < 0.000$). Restricting this to non-OECD countries, the mean is 0.278 for the 861 included observations and -2.411 for the 2,470 excluded

observations with data on the lagged Polity2 variable (once again a t-test of difference in means yields $p < 0.000$). Thus, excluded observations are significantly less democratic than included observations. Furthermore, more than two-thirds of observations with values on the polity variable were excluded from the analysis because of missing data on other variables, particularly the non-tax revenue variables (including grants). It is difficult to draw conclusions based on a non-randomly selected set of less than one-third of the available observations.

Table A.9 shows results for the full models from Table 4 in the main article. Table A.10 shows a similar analysis using the Morrison replication data, but modeling the difference across periods by interacting the different forms of non-tax revenue with an indicator variable for the cold war.

	Based on Morrison (2009), Table 3, Model 1		
	Replication	Cold War	Post-Cold War
	(1)	(2)	(3)
GDP per capita growth	-0.0534*	-0.0447	-0.0826
	(0.032)	(0.046)	(0.055)
GDP per capita, ln (t-1)	0.0497	0.2682	0.0952
	(0.158)	(0.276)	(0.298)
Δ % population urban	0.3557	-0.8259	1.8898***
	(0.364)	(0.617)	(0.714)
Ethnolinguistic fractionalization	0.3161	-0.4116	-0.0799
	(0.606)	(0.954)	(0.918)
Population density, ln (t-1)	-0.0632	-0.0084	-0.2643
	(0.121)	(0.166)	(0.218)
Past Regime Instability	0.0888	-0.1046	0.1993**
	(0.058)	(0.104)	(0.086)
Grants per capita (t-1)	-0.0175**	-0.0664**	-0.0049
	(0.008)	(0.031)	(0.011)
SOE Rev per capita(t-1)	-0.0016**	-0.0009	-0.0195***
	(0.001)	(0.001)	(0.007)
Other non-tax Rev per capita(t-1)	-0.0012**	-0.0021**	0.0015
	(0.001)	(0.001)	(0.001)
Regime Age	-0.3470***	-0.4400***	-0.3076**
	(0.075)	(0.130)	(0.147)
Spline 1	0.0013***	0.0015***	0.0013**
	(0.000)	(0.001)	(0.001)
Spline 2	-0.0001**	-0.0001	-0.0001
	(0.000)	(0.000)	(0.000)
Constant	-1.5764	-1.1472	-1.9748
	(1.203)	(2.041)	(1.960)
Observations	1307	747	419
Countries	98	77	73

Table A.9: Analysis of Morrison (2009). Dependent Variable equals one if there is a three-point change in Polity in either direction, 0 otherwise. Logit Analysis, robust standard errors clustered by country in parentheses. First column replicates Table 3, Model 1 from Morrison (2009). Columns 2 and 3 use the same data but for pre-1989 and post-1991, respectively. $*p < 0.10$; $**p < 0.05$; $***p < 0.01$

	Observations from Morrison (2009), Model 1
Grants per capita (t-1)	-0.000
	(0.95)
Grants*Cold War (t-1)	-0.030**
	(0.03)
SOE Rev per capita(t-1)	-0.017***
	(0.01)
SOE*Cold War(t-1)	0.016***
	(0.01)
Other non-tax Rev per capita(t-1)	0.001
	(0.35)
Other*Cold War(t-1)	-0.002*
	(0.05)
Cold War(t-1)	-0.221
	(0.59)
GDP per capita growth	-0.052
	(0.10)
GDP per capita, ln (t-1)	0.120
	(0.47)
Δ % population urban	0.320
	(0.44)
Ethnolinguistic fractionalization	0.252
	(0.67)
Population density, ln (t-1)	-0.076
	(0.54)
Past Regime Instability	0.091
	(0.11)
Regime Age	-0.331***
	(0.00)
Spline 1	0.001***
	(0.00)
Spline 2	-0.000**
	(0.02)
Constant	-1.705
	(0.20)
Observations	1307
Countries	98

Table A.10: Based on observations for Table 3, Model 1 in Morrison (2009). Dependent Variable is equal to one if there is a regime transition (three-point change in Polity), 0 otherwise. Logit Analysis, robust standard errors clustered by country (not shown), p-values in parentheses. All variables are the same except for the inclusion of a cold war indicator variable equal to 1 prior to 1991 and its interaction with each form of non-tax resources: grants, state-owned enterprise revenue, and "other" non-tax revenue. For both grants and "other", the significant, negative effect claimed in the original article holds only in the cold war period. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$

Bueno de Mesquita and Smith (2010)

The analysis for Table 1 in Bueno de Mesquita and Smith (2010) uses a parametric Weibull model. The authors state that this is chosen because there are theoretical reasons to expect the hazard to change over time. In particular, they expect the hazard to decrease more sharply over time for small coalition systems, and model the ancillary parameter as a function of W (see p. 941 of their article for additional explanation).

The leader survival data used in Table 1 of Bueno de Mesquita and Smith (2010) present difficulties for analyzing the relationship between aid and the likelihood of leader survival. Because of this, I do not present a cold war and post-cold war breakdown of the findings, which are insignificant for the key claims regarding foreign aid in any period. The observations are at the leader-year level, but most data are at the country-year level. For instance, Haiti had three leaders in 1990 and each has a separate entry in the dataset. However, the data for aid, income, W, and other variables are at the country-year level. Therefore, each of the three different leader entries for Haiti in 1990 has the same value for aid, income, and W - none of these variables can be tied to a particular leader during the year. It is difficult to know how one would disentangle the effect of aid on the likelihood of survival for an individual leader, since we don't know which leader to associate with the aid. This is true in each case that a leadership change is observed, as two leaders are recorded for a single year when the change occurs, meaning two entries with duplicate data on many variables.

The problem may be particularly problematic for the variable W, which is coded as of the end of the calendar year for which it is recorded. For instance, Guatemala experienced a coup in 1982. The deposed leader had been in power since July 1, 1978. The years 1978-1981 record a value of .5 for W. On March 23, 1982 a new leader emerged from the coup. The value of W drops to 0 *starting in 1982*. The entire year, including the months before the coup, is coded as 0. This means that the last entry for the deposed leader is associated with a value for W of 0, even though the actual value of W was likely .5 for that leader as it had been in all previous years of his rule; the 0 for 1982 should most likely be associated only with the new leader from March 23 onward, but

there is no way to know this for sure given the yearly reporting of the data on which W is based. So, there are two entries for Guatemala in 1982 - one for the old leader and one for the new - and each have the same value for W (and other variables), although it seems unlikely that this value actually applied to both leaders.

The data used for Table 2 in Bueno de Mesquita and Smith (2010) have a similar problem to that of Morrison (2009) in that many observations are dropped due to lack of data on the oil revenue or aid variables. Model 6 employs the Morrison (2009) measure of non-tax revenue and so will have a very similar missing data problem to that study. The other models use World Bank data for oil and aid, but many observations are still dropped.

Ahmed (2012)

Table A.11 shows the full models for the results displayed in Table 6 in the main text. Additional notes on the models and data follow.

It is important to examine whether differences in the calculations from the text here with those reported in Ahmed (2012) are the result of excluding country fixed effects, rather than fixing other issues with the data and methods. Table A.12 is similar to Table 6 from the main text. However, it is estimated using OLS and including fixed effects for country, year, and duration. Beck (2011) discusses the benefits and drawbacks of using OLS with fixed effects when the dependent variable is binary. For comparison here, it is worth noting that using OLS instead of Probit allows the inclusion of fixed effects without the loss of observations from separation. Using this model, both Aid and Remittances in the post-cold war appear to have a negative effect on the likelihood of turnover in democratic states (coefficients are negative, which means that when Autocracy is zero (most democratic score) there is a negative relationship). However, the coefficients for both Autocracy*Aid and Autocracy*Remittances are positive and significant in the post-cold war, suggesting that higher aid and remittances *increase* the likelihood of turnover in authoritarian states. This is opposite to the pattern claimed in Ahmed (2012). Thus it appears that after correcting for the failure to lag the independent variables, the coding of the autocracy score, and the selection on the dependent variable there is no evidence in favor of the conclusion

that aid and remittances decrease turnover in authoritarian states - whether or not country fixed effects are included.

The main text refers to multiple issue that arise when examining the data and methods from Ahmed (2012). This section provides additional details on each issue.

Separation   This issue effects, either directly or indirectly, almost all results reported in Ahmed (2012). Table 3, Model 4 drops from 2,333 observations (120 countries) to 1,278 (81 countries) and Model 5 drops to 1,545 (74 countries) because of separation caused by year and country indicator variables. While the article claims to have included indicator variables for duration in Models 4 and 5, they are actually not included - so a constant hazard is assumed although it may not be appropriate, particularly for Model 5. The issue caused by separation is the loss of observations caused by including the fixed effects in a model such as Probit or Logit. Thus, the instrumental variable results in Table 4 of Ahmed (2012) are also suspect as they are restricted to the observations used in Model 3 - which had already dropped all units for which there is no variation on the dependent variable. The inclusion of fixed effects in Table 4 does not cause additional loss of observations in these models because the observations had already been dropped.

As an example of the problem induced by separation, Table A.13 exactly replicates Model 3 from Ahmed (2012) in Column 1 and then runs the same regression but without the indicator variables in Column 2. To model the hazard without the duration indicator variables, Column 2 instead includes three variables - coded using the measure for duration from the author's dataset - for duration, duration^2, and duration^3. This approach is advocated by Carter and Signorino (2010) since it approximates the hazard model but does not suffer from problems of separation caused by including duration fixed effects. Comparing column 2 with the original Model 3 in column 1 shows that separation resulted in the loss of 2303-1639=664 observations, or 29% of the data. The number of countries covered rises from 97, as originally reported, to 120 in the corrected version.

Autocracy Score   Two separate issues arise with this score: how it is coded and the decision not to lag it. Figure A.1 graphs the Polity2 score and the autocracy score from Ahmed (2012). As is

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mentioned in the main text, the transformation from Polity to the autocracy scale is not linear, and results in very large differences at the autocratic end of the scale and very small differences at the democratic end. An example can further illustrate why this is troubling. China, which scores a -7 on the Polity2 scale in 2000, has an autocracy score of 0.25, making it closer to countries that score a perfect 10 in 2000 (such as Mauritius, Israel, the United States) and receive an autocracy score of 0.048 than to other authoritarian regimes, like Saudi Arabia, which score a -10 on the Polity2 scale and a 1 on the Ahmed autocracy scale. The difference between China and the United States is 0.20; the difference between China and Saudi Arabia is 0.75. There is no justification given for this rescaling and none is readily apparent.

An additional problem emerges from the failure to lag the Autocracy variable given the coding structure of that variable and the dependent variable (government turnover). An observation is coded as 1 for turnover if it experienced a turnover in that year: for instance, Malawi held its first democratic elections in 1994 returning a new government, and a turnover is recorded for that year. However, Polity2 - on which the autocracy score is based - is coded as of December 31 of the year in which it is reported. So, the Polity2 score for Malawi changes from -8 in 1993 to +7 in 1994. Given the structure of the data and the absence of lags, the +7 Polity2 score in 1994 is seen as "causing" the turnover in 1994, although it is clearly a result of the turnover, not a cause.

Data for Remittances   Of the 2333 observations in Column 1 of Table 6 in the main text, 910 of them (39%) have lagged remittances recorded as zero for the year in question. Examining these data suggests that these zeros are miscoded: for instance, Angola and Burundi have 0 recorded for remittances every year from 1986 to 2002; Albania has zero values for 1989-1992, but then values in excess of 20% for the following years. This potential problem with the remittance variable makes it difficult to draw conclusions with regard to remittances and the likelihood of government turnover. Additionally, there is a fairly significant loss of observations in these models due to lack

of data on remittances (coded blank as opposed to zero), again calling into question the
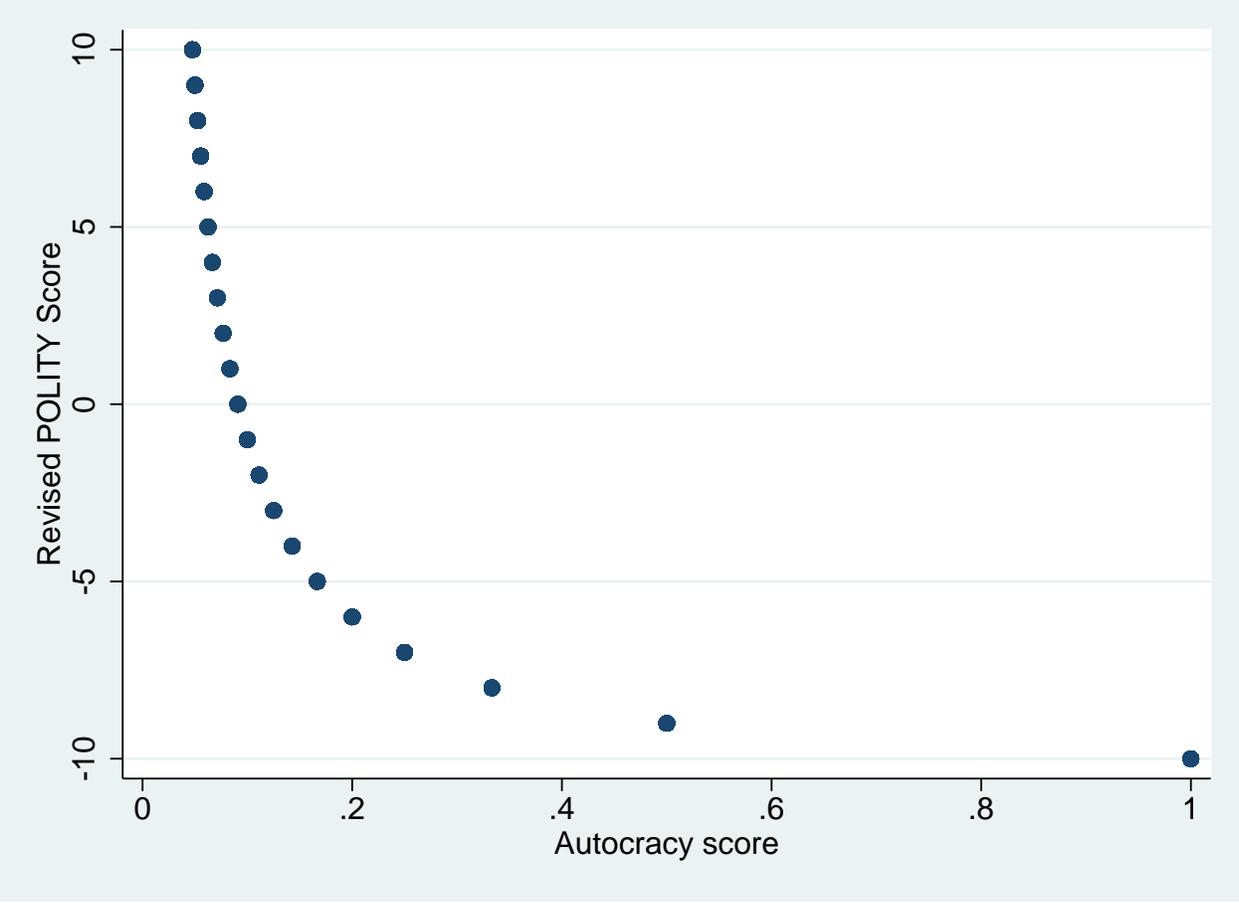applicability of the dataset for drawing broad inferences.

Figure A.1: Notes on Ahmed (2012): Graph of Polity2 scores and the Authoritarian Coding

	Full Period	Pre-1989	Post-1991
	Column 1	Column 2	Column3
Autocracy	-0.995***	-0.712***	-1.385***
	(0.00)	(0.00)	(0.00)
Aid (% GDP) (lagged)	-0.010	0.003	-0.020*
	(0.22)	(0.85)	(0.08)
Autocracy*Aid (lagged)	0.003	-0.018	0.012
	(0.83)	(0.41)	(0.56)
Remittances (% GDP) (lagged)	-0.008	-0.026	0.008
	(0.78)	(0.75)	(0.81)
Autocracy*Remittances (lagged)	-0.038	0.007	-0.116
	(0.44)	(0.95)	(0.27)
Finite term (lagged)	-0.018	0.009	0.002
	(0.85)	(0.95)	(0.99)
ln(GDP per capita) (lagged)	-0.061	-0.038	-0.112*
	(0.10)	(0.48)	(0.07)
Growth in GDP per capita (lagged)	-0.009	-0.013*	0.000
	(0.14)	(0.10)	(0.99)
ln(population) (lagged)	-0.007	-0.002	-0.014
	(0.79)	(0.97)	(0.70)
War (lagged)	0.135	0.066	0.155
	(0.16)	(0.64)	(0.31)
Low political discontent (lagged)	0.267***	0.265**	0.169
	(0.00)	(0.04)	(0.45)
High political discontent (lagged)	0.131	-0.182	0.494***
	(0.15)	(0.18)	(0.00)
Duration	0.191***	0.210***	0.199***
	(0.00)	(0.00)	(0.00)
Duration^2	-0.013***	-0.017***	-0.013***
	(0.00)	(0.00)	(0.00)
Duration^3	0.000***	0.000***	0.000***
	(0.00)	(0.00)	(0.00)
Constant	-0.637	-0.910	-0.140
	(0.28)	(0.35)	(0.87)
Observations	2333	968	1113

Table A.11: Based on the analysis of Ahmed (2012). Probit analysis, robust standard errors clustered on government (not reported); p-values in parentheses. Dependent variable equals 1 if there was a government turnover in year t. All independent variables are lagged one period. $*p < 0.10$; $**p < 0.05$; $***p < 0.01$

	Full Period	Pre-1989	Post-1991
	Column 1	Column 2	Column3
Autocracy (lagged)	-0.247***	-0.040	-0.511***
	(0.00)	(0.69)	(0.00)
Aid (% GDP, lagged)	-0.001	-0.003	-0.007**
	(0.52)	(0.58)	(0.03)
Autocracy*Aid (lagged)	0.000	0.002	0.014***
	(0.88)	(0.80)	(0.01)
Remittances (% GDP, lagged)	-0.007	-0.022	-0.019*
	(0.37)	(0.56)	(0.07)
Autocracy*Remittances (lagged)	0.007	0.006	0.026*
	(0.59)	(0.90)	(0.10)
Finite term (lagged)	0.004	0.013	-0.082
	(0.88)	(0.79)	(0.30)
ln(GDP per capita)(lagged)	0.001	-0.073	0.358***
	(0.98)	(0.51)	(0.01)
Growth in GDP per capita (lagged)	-0.003**	-0.003**	-0.002
	(0.02)	(0.04)	(0.50)
ln(population)(lagged)	-0.109	-0.774**	0.341
	(0.45)	(0.03)	(0.27)
War (lagged)	0.064**	0.037	0.069
	(0.02)	(0.52)	(0.11)
Low political discontent (lagged)	0.024	0.021	-0.039
	(0.29)	(0.58)	(0.48)
High political discontent (lagged)	0.040**	-0.020	0.156***
	(0.03)	(0.45)	(0.00)
Duration Dummies	Yes	Yes	Yes
Country Dummies	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes
Constant	2.027	12.998**	-7.608
	(0.41)	(0.03)	(0.15)
Observations	2333	968	1113

Table A.12: Based on data from Ahmed (2012). OLS analysis, robust standard errors clustered on country (not reported); p-values in parentheses. Dependent variable equals 1 if there was a government turnover in year t. All independent variables are lagged one period. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$

	Model 3	Model 3 No Separation
From Ahmed (2012), Table 3: Government Turnover (Probit model)		
Autocracy	-0.396	-0.293***
	(0.276)	(0.077)
Aid(% GDP)	0.003	0.001
	(0.003)	(0.002)
Autocracy*aid	-0.026	-0.009
	(0.017)	(0.008)
Remittances (% GDP)	0.005	0.005
	(0.009)	(0.004)
Autocracy*Remittances	-0.071	-0.069*
	(0.060)	(0.036)
Finite Term	-0.039	-0.011
	(0.042)	(0.018)
Log GDP pc	0.014	0.011
	(0.057)	(0.007)
Growth	-0.005**	-0.003**
	(0.002)	(0.001)
Log Population	-0.319*	0.002
	(0.171)	(0.005)
Incidence of Civil War	0.060*	0.006
	(0.038)	(0.018)
Low internal discontent	0.032	0.020
	(0.026)	(0.018)
High internal discontent	0.120***	0.068***
	(0.039)	(0.022)
Duration Dummies	Yes	No
Country Dummies	Yes	No
Year Dummies	Yes	No
Duration		0.030***
		(0.005)
Duration^2		-0.002***
		(0.000)
Duration^3		0.000***
		(0.000)
N	1639	2303
Countries	97	120

Table A.13: Replication of Ahmed (2012), Table 3, Model 3. Probit analysis; marginal effects with robust standard errors clustered by government in parentheses. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$