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Why there Should be No Political Foreign Aid Curse

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Summary. — This paper considers the causality underlying the so-called political aid curse, which proposes that foreign aid, like oil, should hinder democracy. Using a theoretical model which identifies repression and appeasement as the primary alternatives to democratization, it argues that aid revenue should not produce a political curse because it is less fungible, more conditional, and less constant than state oil revenue, making it difficult for recipient governments to use their aid to fund either repression or appeasement. Using several different measures associated with repression and appeasement, the statistical results show that aid cannot be associated with any of these dependent variables.

Key words — aid curse, foreign aid, democratization, resource curse, rentier state theory

1. INTRODUCTION

The proposition that foreign aid, much like oil, tends to hinder democracy (the so-called "political resource curse") appears to be a conventional wisdom in certain policy circles. Easterly (2006, chap. 4), Moyo (2009, pp. 52–60) and Deaton (2013, pp. 294–307) all advance a political aid curse argument in their widely cited books. Reflecting this conventional wisdom, Vidar Helgesen, the Secretary-General of the International Institute for Democracy and Electoral Assistance (IDEA), wrote that "it should now be very clear that decades of development aid to corrupt regimes, which bypassed democratic processes in these countries, did nothing more than serve to prop up autocrats and stifle national political debate." [1]

Yet when scholars have tested for the effect of foreign aid on democracy, no clear and robust result has emerged. Djankov, Montalvo, and Reynal-Querol (2008) do find evidence consistent with a political aid curse, as does Ahmed (2012) when he combines aid with remittances. But this latter paper finds no such result for foreign aid alone (Table 3, model 3), consistent with the non-results provided by Knack (2004). Contrary to the aid curse proposition, Wright (2009) finds that aid increases the probability of democratization in larger coalition regimes, following the pro-democracy effect for foreign aid in Africa reported by Goldsmith (2001). But Dunning (2004) shows how the latter result in Africa is limited to the post-1986 period with aid having no impact on democracy from 1975 to 1986. Finally, Morrison (2009) along with Dutta, Leeson, and Williamson (2013) report results in both directions: aid enhances political institutions in already democratic countries, while it has a negative effect on the same in non-democracies.

As this literature review suggests, there are at least two potential versions of a political aid curse. The first argues that aid is bad for political development in all political regimes, both non-democratic and democratic (e.g., Djankov *et al.*, 2008). The first version of the argument largely assumes that all governments use their aid in somewhat similar ways. The second version is that aid is only bad for political development in non-democracies. This second argument effectively assumes that democratic and non-democratic governments use their aid differently; indeed, it includes the possibility that democracies may even use their aid in ways that facilitate political development (e.g., Dutta *et al.*, 2013; Morrison, 2009). It thus becomes important to distinguish more clearly between these two versions, especially given contrary evidence suggesting no political aid curse (e.g., Knack, 2004) and potentially even a political aid blessing (e.g., Goldsmith, 2001).

The wide variation in these results also suggests the need to think more seriously about the causal processes at play. If one is going to argue that foreign aid has any democratization effect (either positive or negative), then it is important to begin with a model of the democratization process and not simply test for a democracy/aid relationship. More specifically, if one wants to argue that aid is like oil in being bad for democracy, then one must show *how* oil has its non-democratic effects and then demonstrate that foreign aid has similar properties. In this regard, it is not enough to claim that aid comes as "unearned income" to recipient governments. This may well be true, but oil revenue is thought to be bad for democracy not simply because it is unearned, or comes without citizen taxation, but because it also has other key properties, namely fungibility, non-conditionality, and constancy (or reliability) over time, potentially allowing petro-governments to use it for citizen appeasement and/or repression (Ross, 2001).

The theoretical gap about how foreign aid is or is not like state oil revenue represents the point of departure for our paper. With a model of democratization and theory about how foreign aid is unlike state oil revenue, we argue that there should not be a political aid curse. On the empirical side, this paper will not present yet another set of democracy/aid regressions in an effort to determine whether or not foreign aid can be associated with a political curse. Instead, our empirical analysis will be focused on intermediate variables with particular focus on repression and appeasement, which models identify as primary strategies allowing autocratic governments to avoid democratization (e.g., Acemoglu & Robinson, 2006; Bueno de Mesquita & Smith, 2010).

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With regard to repression, there has been little research directly examining how aid can be associated with actual repression or if aid has been used to fund the infrastructure that autocratic governments use for repression. With regard to appeasement, there has been some research showing how aid can be associated with increased government spending. For example, Boone (1996) reports a positive relationship between aid and government consumption. But since the latter variable includes spending in a multitude of sectors, this result does not demonstrate that aid has actually funded the public goods that would be necessary to appease citizens who might otherwise mobilize for political change. Indeed, Boone's result has often been cited as evidence showing how recipient governments waste their foreign aid, spending it on pet projects and other private goods, which would be an ineffective way to pacify a large disaffected population looking for greater public goods and other forms of economic redistribution.

The most serious effort toward testing the appeasement strategy to avoid democratization comes from Morrison (2009), whose results show a positive relationship between grants per capita and *social* spending in a sample of dictatorships. While such a relationship is consistent with a story of autocratic governments successfully appeasing their populations, it is also consistent with other propositions. For example, it could be the case that aid is associated with more social spending due to enforced aid conditionality: donors demand that their aid be spent on particular public goods and recipients comply with this demand. It is also consistent with the proposition that foreign aid is relatively *in*fungible. If aid targeted toward social spending simply displaced domestic funds already earmarked for social spending, then one should not be able to observe a positive relationship between aid and social spending. To the extent that aid is both more conditional and less fungible than other forms of "unearned revenue" such as state oil, it becomes more difficult to link aid with a political curse since it lacks some of the key properties necessary to produce this effect.

Starting with an argument about why aid is poorly suited to paying for either appeasement or repression due to aid revenue being less fungible, more conditional, and less constant over time than state oil revenues, this paper further explores intermediate variables related to the political aid curse. Using a variety of dependent variables related to appeasement and repression, we present evidence showing that foreign aid (measured different ways) has no robust effect on any of these indicators within different country/year statistical samples. Our results are inconsistent with either version of the political aid curse: we find no systematic aid curse in a full sample that combines both democracies and non-democracies (the first version), but we also find no robust aid curse in a non-democratic sub-sample (the second version). We thus cautiously conclude that aid does not systematically hurt democracy. Our large-N results do not deny that aid may have hindered democracy in certain special cases (e.g., the Mobutu regime in Zaire), but they strongly suggest that such examples would not be typical of the broader pattern. Based on this understanding, we argue against a *general* political aid curse, but we cannot assert that aid has never produced this effect anywhere.

2. THE ARGUMENT

(a) *A model of democratization*

As mentioned above, if one is going to advance an argument about aid's effect on democratization (including aid having no such effect), then it should begin with a model of political liberalization. Since the literature is already rich with such models, it would not be helpful to create yet another democratization framework specifically for this purpose. Instead, we make use of Acemoglu and Robinson's (2006) model given its prominence within the political-economy literature and because it includes what political scientists have called "rentier state theory," although Acemoglu and Robinson (henceforth A&R) never actually use this term. This latter consideration is especially important because the rentier effect has become the standard explanation for why state oil and other natural resources might hinder democratization (Ross, 2001).

The A&R model begins in an autocratic state where a small group of rich elites hold de jure political power. Since the elites benefit from such a system, they prefer to maintain their autocratic regime. The elite are opposed in this preference by a larger group of poor citizens, who stand to gain a greater slice of the economic pie in a more democratic regime following the logic of the median voter theorem. In this regard, democratization is a means to achieve greater economic redistribution (away from the rich and toward the poor). While the poor population lacks de jure political power, it nonetheless has some de facto political power (as a collective actor) based on its size advantage over the smaller group of rich elites.

Given favorable circumstances helping to overcome the collective action problem (e.g., a political and/or economic crisis), the population could revolt to obtain de jure political power. A revolution would be somewhat costly for the population because it destroys a portion of the economic assets within the national economy, thus reducing the size of the pie that could be redistributed in a post-revolutionary regime. But a revolution would be extremely costly for the rich elite because they stand to lose all of their economic assets via expropriation. Thus, to avoid a revolution (their worst possible outcome), the elite (as a collective actor) have three basic strategies:

(1) acquiesce to the population's demand for a more democratic regime (i.e., democratization),
(2) increase their repression of the population to prevent them from organizing to revolt, and
(3) provide more redistributive concessions to appease the population so that they become more satisfied with the autocratic status quo.

Given these options, democratization (the first strategy) becomes less likely as the elites are better able either to pay the costs associated with repressing the population (the second strategy) or to provide the redistributive concessions necessary to appease the population (the third strategy). In this regard, it is important to note that these two alternatives to democratization—repression and appeasement—are not unique to the A&R framework. For example, selectorate theory (Bueno de Mesquita, Smith, Siverson, & Morrow, 2003) has recently been expanded to take account of how political elites might respond to revolutionary threats from below. As summarized by Bueno de Mesquita and Smith (2010, p. 936), the autocratic state "can increase the provision of public goods [appeasement], thereby improving the welfare of the citizens and diminishing their desire for revolutionary change. Alternatively, leaders can suppress the provision of public goods [repression], particularly such goods as a free press, transparency, and easy communication that help people coordinate and organize."

It is also important to understand that the appeasement strategy, contained within both the A&R model and the expanded version of selectorate theory, represents a more formal version of basic rentier state theory. From its original

application to natural resources (Mahdavy, 1970), rentier state theory posits that certain governments earn so much revenue (i.e., rents) from their resource endowments that they can provide a set of basic goods and services without taxing their citizens. Given that their citizens obtain these public goods without taxation, they have no need to mobilize for political change to obtain economic redistribution. What is important about the appeasement strategy coming from rentier state theory is that it is not enough for the autocratic government simply to forego taxation. The autocratic government must also provide some public goods because without those public goods, poor citizens (even untaxed) would still have an incentive to mobilize for democracy as a means to obtain economic redistribution.

While we focus here on appeasement and repression as the primary strategies to avoid democratization, it is important to acknowledge that there may be other strategies that are not directly considered within the A&R model. One possibility is simply robust economic growth as a way to reduce citizen demands for greater redistribution via democratization. In this regard, robust growth represents a different form of economic appeasement, one that does not necessarily rely on reduced taxation with greater public goods spending. While this may be a viable anti-democratization strategy in certain contexts, it is not one that many proponents of an aid curse (e.g., Easterly, 2006; Moyo, 2009) can advance in their criticism of Western aid. They cannot advance an argument about appeasement through economic growth as a way to avoid democratization because their broader argument about the pernicious effects of foreign aid includes the claim that aid does not promote economic development because recipient misgovernments misspend their aid on private goods. And to the extent that aid does not increase economic growth and development, it cannot also pacify citizen demands for democracy in this way. Stated differently, there is a contradiction in arguing for an economic aid curse (i.e., aid is bad for growth) and also for a political aid curse, at least through a growth appeasement strategy (i.e., aid is good for growth).

Another possibility is what might be termed a "divide-and-conquer" strategy, which is focused more on ethnic division than on the class conflict central to the A&R framework. In a divide-and-conquer strategy to avoid democratization, the autocratic government directs a certain set of public goods toward a favored ethnic group (divide) and then suppresses the less favored groups (conquer). While this strategy is not directly discussed by Acemoglu and Robinson (2006), it should be noted that it effectively represents a hybrid appeasement/repression strategy: the favored group is being appeased, while the less favored groups are being repressed. Based on this understanding, divide-and-conquer is not a uniquely different strategy from appeasement and repression, instead, it is a strategy that combines both appeasement and repression on a targeted basis. So we will now consider the extent to which foreign aid is well suited to paying for these two alternatives to democracy.

(b) *Comparing oil and aid*

At least in theory, oil revenues—*provided that they accrue to the state instead of to private oil companies* (more on this subject later)—could be used to provide the redistributive concessions necessary for appeasement and/or to pay the costs associated with repression. *State* oil revenues could be used in either of these ways because they have certain key properties. First, they are fungible, or could be spent in whatever sector (e.g., education, health, and/or military) that the state chooses. Second, state oil revenues come without any external conditions about how the money can be spent or without any requested policy changes (including democratization). Third, oil revenues tend to be relatively constant over time due to factors like inelastic consumer demand and a producer cartel (the Organization of Petroleum Exporting Countries [OPEC]) able to adjust oil prices through changes in oil supply.[2]

Table 1. *Oil versus aid*

Property	State oil	Foreign aid
Fungibility	More fungible	Less fungible
Conditionality	No conditions	Some conditions
Constancy over time	More constant	Less constant

But does foreign aid have these same key properties? The argument to be advanced here is not that aid is non-fungible, highly conditional, and completely volatile (unlike state oil). Instead, we simply argue that aid revenue is less fungible, more conditional, and less constant (Table 1), making it less well-suited to paying for repression and providing redistributive concessions. And to the extent that oil—even with more of these key attributes—has no robust negative effect on democracy (Haber & Mendaldo, 2011), one should not expect aid to produce a political curse through either repression or appeasement.

(i) *Fungibility*

In arguing how free resources could hinder democracy, Smith (2008, p. 780) notes that aid might be a free resource like oil "to the extent that it [aid] is fungible". Likewise, Morrison (2009, p. 110) theorizes that aid should have political effects like oil because it "is a highly fungible resource". As evidence on this point, both authors cite a single paper: Feyzioglu, Swaroop, and Zhu (1998). Indeed, this has become one of the most cited papers in the foreign aid effectiveness literature, emerging as the key reference for authors wishing to make a fungibility argument. Given the literature's reliance on this paper, it becomes worthwhile to re-read what Feyzioglu, Swaroop, and Zhu actually said about aid fungibility. They did not write that foreign aid was *highly* fungible.

Feyzioglu, Swaroop, and Zhu studied the effect of total aid in a sample of 14 developing countries from 1971 to 1990 and then broke down total, or aggregate, aid into five sectors: education, health, energy, agriculture, and transport/communication. Based on their empirical results, these authors reached two basic conclusions about aid fungibility. First, they did "not find evidence of foreign aid fungibility at the aggregate level" (Feyzioglu *et al.*, 1998, p. 41). In this regard, it is important to note that when scholars have examined the relationship between aid and democracy, they have used an aggregate aid measure, precisely where these authors "did not find aid to be fungible" (Feyzioglu *et al.*, 1998, p. 54).

Second, when disaggregating total aid into different sectors, Feyzioglu, Swaroop, and Zhu only found evidence for full fungibility in one of the five sectors: agriculture. In terms of aid for transport/communication, they found positive evidence for *nonfungibility* (Feyzioglu *et al.*, 1998, Table 5, p. 47). In the other three sectors, they either found evidence for partial fungibility (energy) or inconclusive results (education and health). In short, these authors did not report what political scientists often say they reported: highly fungible aid.

To the extent that foreign aid is less fungible than many scholars seem to realize, it is important to consider why this might be the case. Much has been written about the so-called

"modalities" of aid, or the various ways that donors can deliver it. And many of these aid modalities either directly bypass the recipient government (e.g., aid through NGOs) or limit how recipient governments can use it (e.g., tied aid and technical assistance).[3] As Collier (2006, p. 1486) wrote about technical assistance, for example, it "is not money in the hands of recipient governments. Thus, it does not increase sovereign rents. Indeed, to the extent that it is effective, it is likely to increase scrutinized revenues and so increase the incentives for effective government, precisely contrary to the effect of resource rents." The second reason for reduced aid fungibility comes on the recipient side. In response to the proposition that aid simply displaces money that recipient governments would have already spent on economic development (thus freeing revenue for other purposes, such as repression), Collier (2006, p. 1486) notes that "where aid is very large relative to government budgets as is often the case in Africa, fungibility is reduced at the margin. For example, once government funding of the development budget has fallen to zero, as is the case in some countries, there is simply no further scope for fungibility."

(ii) *Conditionality*

Closely related to fungiblity is the issue of aid conditionality. This property refers to donor demands that aid be spent in particular ways or be associated with particular policy changes, including democratization. In arguing that aid might have negative democratic effects similar to those often associated with oil, one is effectively arguing that while aid may sometimes come with attached conditions, those conditions cannot be enforced by donors; hence, aid is effectively *un*conditional much like state oil (Morrison, 2010).

The primary theoretical argument for why donors cannot enforce their aid conditions comes from the understanding that donors often provide foreign aid to achieve certain military/strategic objectives (Alesina & Dollar, 2000). To the extent that aid recipients provide the expected benefits to donors, the latter have little incentive to reduce or cut off their aid even when the former fail to comply with the attached economic and political conditions. Empirical evidence in support of this proposition comes from Heckleman and Knack (2008), who reported—when using the country/20-year period (1980–99) as their statistical unit of analysis—that aid cannot be positively associated with market-liberalizing economic reform, a condition often associated with Western aid.

But the argument that aid is effectively unconditional faces both theoretical and empirical problems. In terms of theory, while it may well apply during the Cold War, its applicability to the post-Cold War period is limited (Dunning, 2004) given evidence that the military/strategic content of Western aid has been dramatically reduced (Bermeo, 2007). In support of this contrary proposition that aid conditions have become more enforceable in the post-Cold War period, Bearce and Tirone (2010, pp. 843–844), using the same economic reform data as Heckleman and Knack (2008), found that while Western aid was uncorrelated with economic reform in the pre-1991 period, it was positively and significantly correlated with reform in post-1990 period. In this regard, it is important to state that this evidence does not indicate that all aid conditions have become enforceable in the post-Cold War period. But it does indicate how aid has become *more* conditional than state oil, making it less well-suited for repression or appeasement as alternatives to democratization.

In response to this logic, one might argue that certain forms of aid conditionality could actually work to facilitate appeasement. For example, if donors condition their aid on public goods spending and this condition is enforceable (Morrison, 2007), then autocrats would be forced to spend their aid on the very public goods that might appease a population that would otherwise mobilize for democracy. As mentioned in the introduction, Morrison (2009, Table 6, p. 127) offers results that could be read as consistent with this proposition: in a sample of dictatorships, grants per capita are positively associated with social spending per capita.

But there are two things to consider with regard to this important result. First, to the extent that aid grants can be associated with increased social spending, it serves as prima facie evidence that aid may not be so fungible in these sectors. If aid were completely fungible in terms of government spending on health, education, welfare, and housing, simply replacing the money that autocratic governments would have otherwise spent in these sectors, then one should not be able to observe any positive correlation between aid and government spending in these categories. And to the extent that aid is less fungible in these sectors, it undermines the primary reason discussed above for why scholars have suggested that aid might produce the same negative political effects as oil: its fungibility.

Second, if one is going to argue that a positive relationship between aid grants and social spending in dictatorships represents evidence of successful autocratic appeasement, then one also needs to know something about the same relationship *within a sample of democracies*. If the positive relationship between aid grants and social spending is even stronger in a democratic sample, then it would suggest that citizens within an autocratic regime would do even better with democratic redistribution, thus giving them an incentive to mobilize for political change despite any limited autocratic efforts toward appeasement.

(iii) *Constancy*

Even if aid were fungible and non-conditional, using it successfully to fund repression and/or appeasement as alternatives to democracy also requires that aid flows be relatively constant, or reliable, over time. While this third property is not often discussed in the aid curse literature, constancy is important for the appeasement strategy, for example, because the autocrat's promise to provide public goods, or engage in redistribution without democratization, must be credible to the population (Acemoglu & Robinson, 2006). If this concessionary promise is not credible, or cannot be sustained over time, then the population would be better off revolting when favorable conditions present themselves despite the economic costs associated with a revolution.

With regard to an aid curse, the issues of credibility and constancy are linked. If an autocrat intends to use its foreign aid to fund a sufficient level of public goods so that its population has no need to mobilize for political change, then the aid flows must be relatively constant over time so that the autocrat can maintain its public goods provision. If these public goods cannot be maintained over time due to non-constant or volatile aid flows, then the autocrat's promise to provide them as an alternative to redistributive democracy becomes less credible. Stated differently, if the population believes that these public goods may erode because future aid flows could be insufficient to maintain them, then the population has an incentive to revolt when favorable conditions arrive. Hence, the autocratic appeasement strategy, even if attempted, would ultimately fail when the citizens decide to obtain more economic redistribution through political change than through autocratic appeasement funded by unreliable and volatile aid flows.

Constancy is also important for the repression strategy to avoid democratization. If an autocrat intends to use its foreign

Table 2. *Comparing the constancy of aid and oil*

	1961–2006 $N = 5206$		1991–2006 N = 2172	
	LDV	R^2	LDV	R^2
Aid Per Capita	0.85 (0.01)	0.71	0.79 (0.01)	0.64
Oil Per Capita	0.92 (0.01)	0.85	0.99 (0.01)	0.92

aid to fund repression (i.e., to pay for more police, soldiers, and their equipment), then the aid flows must be relatively constant over time so that the autocrat can maintain its repression. Repression maintenance is necessary for success in preventing citizens from mobilizing for democracy because short-term repression, while it may make it harder for citizens to organize against the existing regime in the present, also increases the demand for some new regime with greater political/civil freedoms/rights, or reduces the demand for the current more repressive regime. Stated differently, a repression strategy must be sustainable over the long-term, thus requiring a constant and reliable funding source.

With regard to aid as an external funding source (for repression, for appeasement, or for any other purpose), there is already a large literature documenting how foreign aid flows feel volatile, uncertain, and unpredictable from the perspective of recipient governments. As Bulir and Hamann (2008, p. 2049) summarized on this point, research has shown aid to be "highly volatile, far exceeding that of other macroeconomic variables, such as GDP or fiscal revenue. Other key findings include the mild procyclicality of aid disbursements (i.e., aid tends to be disbursed mostly in periods when output or domestic revenue are high and held back when domestic economic activity is contracting) and the fact that aid disbursements are difficult to predict, particularly on the basis of donor commitments."

Since these findings about aid volatility include no direct comparison to the volatility of oil, it is useful to provide some additional evidence on this point (see also footnote 2). Imagine a revenue stream that is completely constant over time (i.e., the government receives the same amount of revenue per capita in year t that it received in year $t - 1$). If one were to regress this revenue stream on only its lagged value, one should be able to observe that the lagged dependent variable (LDV) as an independent variable takes on a coefficient of 1.0 and that this single regressor can explain all of the variation in the dependent variable (i.e., $R^2 = 1.0$). Using these two pieces of statistical information (the LDV coefficient and the R^2), one can directly compare the constancy of aid with the constancy of oil with these two revenue streams measured on a per capita basis.

Using aid per capita data from the World Bank. (2012) and the oil per capita measure from Haber and Mendaldo (2011), the results of these LDV regressions are compared in Table 2. To make sure that there are no differences that come from comparing these revenue streams using different times-series lengths (the aid data begin only in 1961, while the Haber and Menaldo data begin earlier), their constancy is compared using two identical samples: 1961–2006 ($N = 5206$) and 1991–2006 ($N = 2172$), the latter corresponding to the period when aid conditionality became more enforceable, as discussed above. In both time periods, aid revenues per capita are significantly *less* constant than oil revenues per capita. Furthermore, while the latter set of revenues have become even more constant and reliable in the post-1990 period, aid has only become less constant in the post-Cold War era, consistent with the understanding that donors are now better able to curtail or suspend their aid when recipients do not behave accordingly.

3. THE EVIDENCE

The argument advanced above proposes that aid revenues are poorly suited for either appeasement or repression as alternatives to democracy. To explore the empirical validity of this proposition, we first identify several dependent variables that can be associated with appeasement, repression, or both. It should be noted that, as much as possible, we take our data from publicly available sources and use measures that already exist in the literature. These decisions help ensure that our results are not an artifact of using unusual data or atypical operational measures. A replication dataset will be available on-line.

(a) *Dependent variables*

In terms of appeasement, we consider aid's effect on two indicators, one related to taxation and the other related to public goods spending. The first appeasement indicator is domestic taxes (namely, taxes on goods, services, income, profits, and capital gains) as a share of total government revenue (*TAXBURDEN*) calculated from *World Development Indicators* (World Bank, 2012). *TAXBURDEN* is the same tax indicator used by Ross (2001, pp. 347–348) to test the "rentier effect" associated with an oil curse. The second appeasement indicator is social spending as measured by government expenditures on education and health as a share of gross domestic product (*SOCSPENDING*) also using World Bank data. If there is a political aid curse through appeasement, then one should be able to observe that aid is negatively associated with *TAXBURDEN*, while *SOCSPENDING* is positively related to aid (Table 3).

In terms of repression, we also consider aid's effect on two different indicators. The first is military spending as a share of gross domestic product (*MILSPENDING*) using World Bank data. This was the primary measure employed by Ross (2001, p. 351) to test the "repression effect" related to the oil curse. The second measure is the Physical Integrity Rights

Table 3. *Political aid curse associations*

Dependent variable	Aid curse sign
TAXBURDEN	−
SOCSPENDING	+
MILSPENDING	+
PHYSINT	−
ANTIGOVT	−
REGCHANGE	−

Index (*PHYSINT*) from Cingranelli and Richards (1999). Often used as a measure of human rights protection, *PHYSINT* ranks country/years on a 0–8 scale with lower values indicating more torture, extrajudicial killing, political imprisonment, and disappearances. In terms of our repression indicators, *MILSPENDING* captures the ability to implement a repression strategy, while *PHYSINT* is a measure of actual repression or, more correctly, the inverse of repression given that higher values are associated with less repression. To the extent that there is a political aid curse through repression, one should be able to observe that aid is positively associated with *MILSPENDING*,[4] while *PHYSINT* is negatively related with aid. These aid curse relationships are also summarized in Table 3.

Finally, we consider aid's effect on two indicators related to the effectiveness of both appeasement and repression. To the extent that aid-facilitated appeasement and/or repression (working either separately or together per "divide and conquer") helps to reduce that amount of societal pressure directed against the government, aid should be associated with less anti-government activity (ANTIGOVT) measured using the logged value of a weighted domestic conflict index, which includes (in order of importance) revolutions, guerrilla warfare, assassinations, riots, strikes, government crises, purges, and anti-government demonstrations (Banks & Wilson, 2013). We also consider aid's effect on a dichotomous measure of political regime change (REGCHANGE), calculated using the Durable measure from Polity (Marshall & Jaggers, 2012).[5]

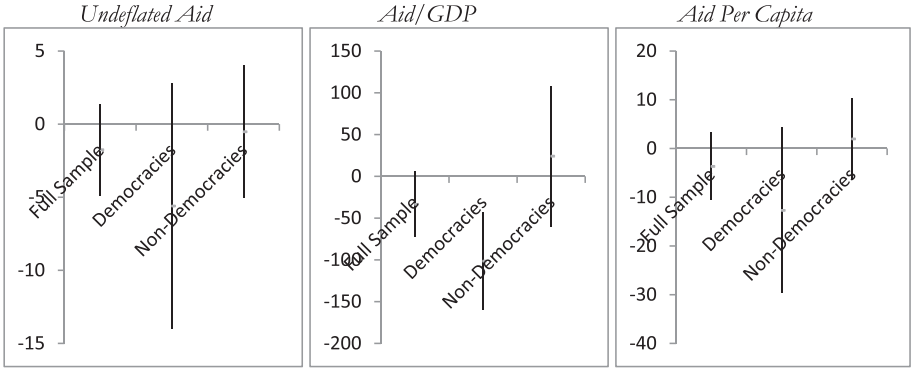

Figure 1. *Aid LRM with 95% confident intervals for TAXBURDEN.*

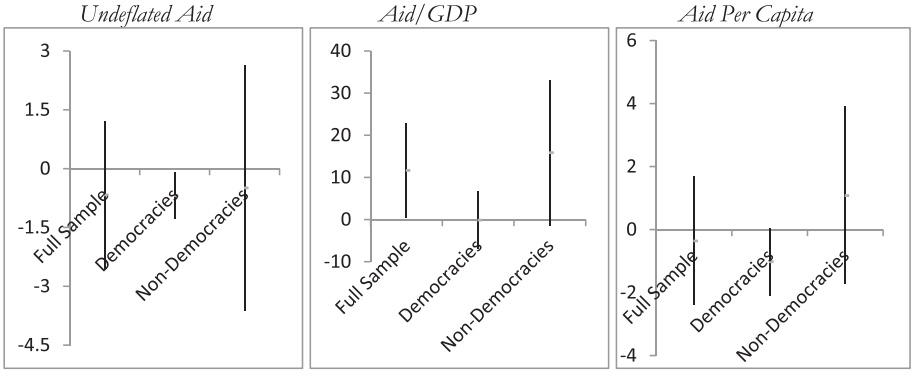

Figure 2. *Aid LRM with 95% confident intervals for SOCSPENDING.*

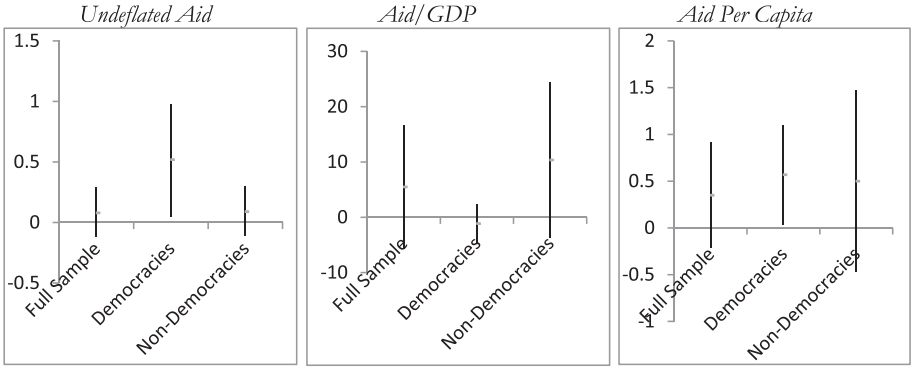

Figure 3. *Aid LRM with 95% confident intervals for MILSPENDING.*

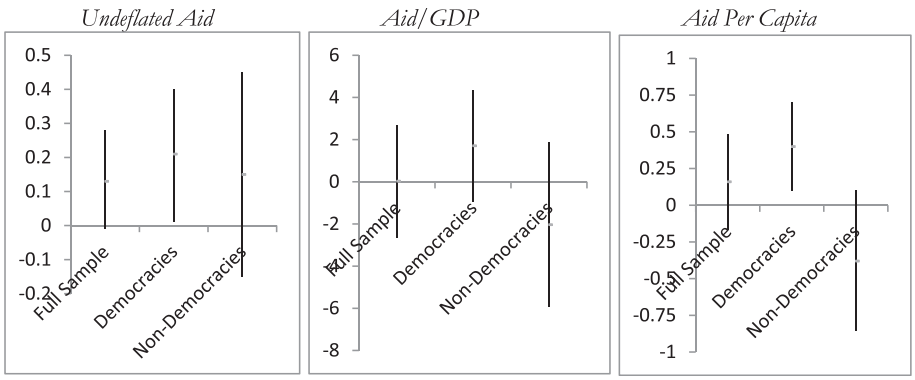

Figure 4. *Aid LRM with 95% confident intervals for PHYSINT.*

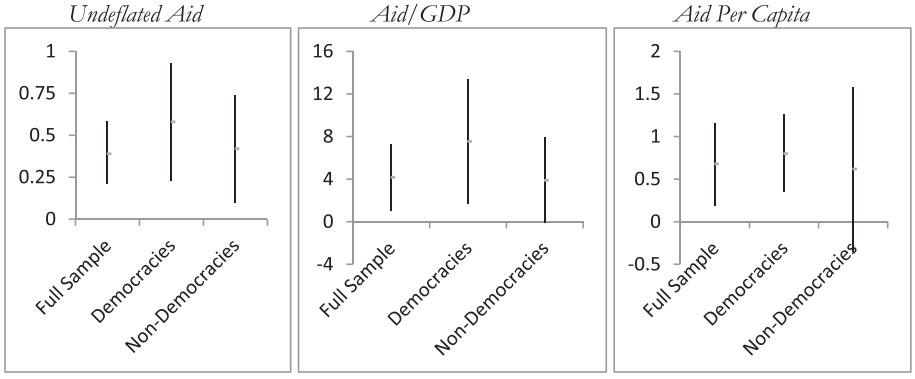

Figure 5. *Aid LRM with 95% confident intervals for ANTIGOVT.*

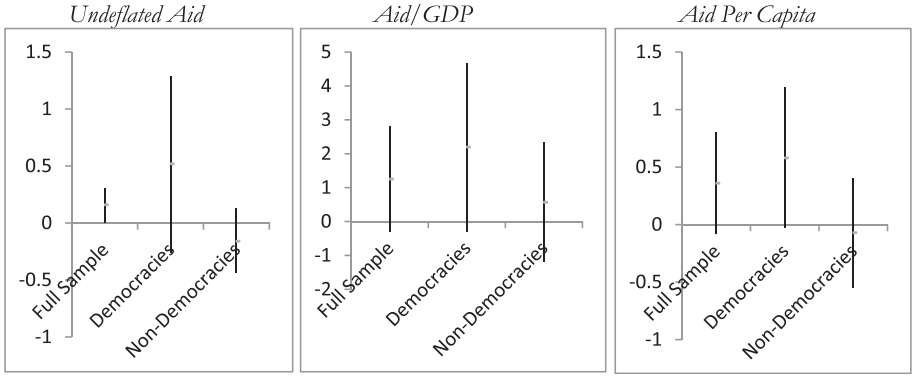

Figure 6. *Aid level coefficient with 95% confident intervals for REGCHANGE.*

This is the same variable as used by Morrison (2009, p. 119) to test his argument about aid and oil making political regimes more stable. To the extent that there is a political aid curse, foreign aid should be negatively related to both ANTIGOVT and REGCHANGE.

(b) *Model specification*

With the exception of REGCHANGE (a dichotomous dependent variable whose estimation will be discussed below), the continuous dependent variables above (TAXBURDEN, SOCSPENDING, MILSPENDING, PHYSINT, and ANTIGOVT) will be regressed on foreign aid (and oil) using an error correction model (ECM) specification. Appropriate for both integrated time-series *and* for stationary data (De Boef & Keele, 2008), the ECM specification regresses the change in the dependent variable (ΔY_{it}) on its lagged level (Y_{it-1}) with all independent variables entering both through their lagged level (X_{it-1}) and through a change measure (ΔX_{it}), as shown in Eqn. (1). Given this specification, foreign aid's long-run multiplier (LRM) is given by the coefficient on the lagged aid level (β_1) divided by the absolute value of the lagged dependent variable (α_1). [6] The short-term effect of foreign aid simply comes from the coefficient on the aid change measure (β_2).

$$\Delta Y_{it} = \alpha_0 + \alpha_1 * Y_{it-1} + \beta_1 * X_{it-1} + \beta_2 * \Delta X_{it} + \varepsilon_{it} \quad (1)$$

Given that all of the right-hand side variables in Eqn. (1) enter either as lagged levels or as changes from the previous period, the ECM specification should help to reduce endogeneity in the form of reverse causality. Our specification also includes

country fixed effects to deal with endogeneity in the form of selection bias. To the extent that there remains uncorrected endogeneity, it is important to note how this bias tends to work *in favor of finding a political aid curse*. Following the Washington consensus, donors have provided more aid to governments with better economic policies, including lower taxes and spending targeted on public goods (education and health). Likewise, foreign aid is often used to recruit and maintain military allies; thus, more aid may be provided to countries with greater military spending. Thus, reverse causality may tend to produce an aid curse coefficient (see Table 3), working against our proposition that foreign aid is poorly suited for either appeasement or repression as alternatives to democracy.

For each dependent variable, Eqn. (1) will be estimated several times, using three different measures of country/year foreign aid so that the reader will be able to observe the extent to which results are unique to one particular aid specification. The first aid measure is *Undeflated Aid* (measured in billions of constant US dollars) based on the concern raised in Bearce and Tirone (2010, p. 841) that deflating aid by either gross domestic product (GDP) or by population may bias the aid coefficient to the extent that the denominator (either GDP or population) has any direct effect on the dependent variable. Since it is nonetheless common in the quantitative foreign aid literature to deflate aid by either GDP or by population, the second aid measure is *Aid/GDP* and the third is *Aid Per Capita* (Aid/population, measured in hundreds of constant US dollars). Data for all three of these aid measures come from the World Bank, and they include foreign aid both in the form of grants and highly concessional loans. [7]

Although not the theoretical focus of our paper, each model will also include an equivalent oil measure (either *Undeflated Oil*, *Oil/GDP*, or *Oil Per Capita*) so that aid's effect can be directly compared to oil's effect (with the former expected to be weaker than the latter to the extent that the latter has any "curse" effect). [8] This is also a valuable exercise since, with a few notable exceptions (e.g., Ross, 2001 and Morrison, 2009), oil curse regressions have not systematically tested for the causal processes at play, focusing almost exclusively on the oil/democracy relationship while ignoring whether this relationship stems more from repression, appeasement, or both.

As control variables, we include *Population* (measured in millions), *GDP* (measured in billions of constant US$), *GDP per capita* (logged), the *Growth* rate of *GDP per capita*, *MID Involvement*, and a full set of country fixed effects. *Population* and *GDP* control for the size of potential aid recipient, which is especially important when using *Undeflated Aid*. But these measures are also important when using a deflated aid measure (either *Aid/GDP* or *Aid Per Capita*) in order to minimize the effect of the aid denominator. The logged measure of *GDP per capita* captures the effect of economic development. The *Growth* rate of GDP per capita controls for the current state of the national economy. [9] *MID Involvement* indicates whether or not the country/year was involved in a militarized interstate dispute (MID), which may influence the repression variables in particular. [10]

Our fixed effects estimates begin with a full sample of potential aid recipient country/years in order to reduce concerns about sample selection and to test the first version of the political aid curse that the political aid curse applies to all countries

Table 4. *TAXBURDEN models (aid/oil curse sign −)*

	Full sample	Democracies	Non-democracies
Lagged DV	−0.34*** (0.04)	−0.37*** (0.06)	−0.40*** (0.07)
Undeflated aid$_{t-1}$	−0.60 (0.54)	−2.06 (1.67)	−0.21 (0.90)
Undeflated aid LRM	−1.75 (1.58)	−5.60 (4.20)	−0.52 (2.26)
ΔUndeflated aid	−0.73 (0.99)	−1.40 (1.08)	−0.43 (1.69)
Undeflated Oil$_{t-1}$	−1.89** (0.90)	−1.87 (2.07)	−1.57* (0.86)
Undeflated oil LRM	−5.50** (2.64)	−5.09 (5.62)	−3.91* (2.08)
ΔUndeflated oil	−1.00 (0.82)	0.87 (0.71)	−2.20*** (0.60)
N=	919	518	401
Lagged DV	−0.35*** (0.04)	−0.44*** (0.06)	−0.37*** (0.06)
Aid/GDP$_{t-1}$	−11.73* (7.01)	−45.06*** (14.46)	8.94 (15.62)
Aid/GDP LRM	−33.48* (19.65)	−101.42*** (29.34)	24.16 (41.92)
ΔAid/GDP	−20.22** (8.88)	−30.94*** (2.95)	−8.92 (16.32)
Oil/GDP$_{t-1}$	1.32 (6.97)	45.08*** (5.64)	−5.42 (7.56)
Oil/GDP LRM	3.75 (19.84)	101.45*** (12.43)	−14.65 (22.03)
ΔOil/GDP	−1.94 (8.58)	29.99*** (4.44)	−8.74 (11.13)
N=	921	518	403
Lagged DV	−0.34*** (0.04)	−0.37*** (0.06)	−0.39*** (0.07)
Aid Per Capita$_{t-1}$	−1.26 (1.19)	−4.74 (3.39)	0.77 (1.67)
Aid Per Capita LRM	−3.67 (3.49)	−12.69 (8.53)	1.97 (4.15)
ΔAid Per Capita	−2.45*** (0.80)	−3.82*** (1.39)	−1.30* (0.68)
Oil Per Capita$_{t-1}$	−0.06 (0.05)	−0.02 (0.30)	−0.09** (0.04)
Oil Per Capita LRM	−0.17 (0.14)	−0.06 (0.81)	−0.23* (0.13)
ΔOil Per Capita	−0.03 (0.07)	0.30** (0.13)	−0.09 (0.08)
N=	921	518	403

Cell entries are OLS coefficients with robust standard errors clustered on the country in parentheses. Although not reported for space consideration, the control variables in all *TAXBURDEN* models include *Population*, *GDP*, *GDP per capita* (logged), *Growth*, *MID Involvement* and a full set of country fixed effects. Statistical significance is indicated as follows.
***$p < .01$ (two-tailed).
**$p < .05$ (two-tailed).
*$p < .10$ (two-tailed).

that receive aid.[11] Using the same specification, we then split the full sample of aid-eligible country/years into democratic and non-democratic sub-samples (with democratic country/years defined as 7 or greater using the [−10, 10] Polity scale) in order to see whether the full sample results are driven more by the democratic observations or non-democratic observations.[12] These split sample results are necessary to test the second version of the political aid curse: that the political aid curse only applies to non-democracies. It should be noted that both versions of the argument expect to see a political aid curse in the non-democratic sub-sample so this is where the aid curse results should be the strongest. The first version would also expect to observe a political aid curse in the democratic sub-sample, while the second version would not.

Thus, for each of the dependent variables in Table 3, we estimate nine different error correction models. Using each of the three aid measures (*Undeflated Aid*, *Aid/GDP*, and *Aid Per Capita*) and an equivalent oil measure, the ECMs include: (1) the full sample, (2) the democratic sub-sample, and (3) the non-democratic sub-sample. Given 54 sets of statistical results (9 models for each of 6 dependent variables), they are presented in separate tables for each dependent variable and they include, for space considerations, only the lagged dependent variable, the lagged aid level (both of which are used to calculate aid's long-run multiplier [LRM]), and the aid change variable (along with the equivalent oil variables for comparison purposes). No control variables or country fixed effects are reported in these statistical tables, although they are part of the model specification. For readers who prefer a different presentation of results, we also include a parallel set of aid coefficient graphs, showing the various aid LRMs along with 95% confidence intervals (Figures 1–6).

(c) *Statistical results*

(i) *TAXBURDEN*

The results when using domestic taxes as a share of total government revenue as the dependent variable are presented in Table 4 (with the aid LRMs graphed in Figure 1). While the aid coefficients generally take on the negative curse sign (i.e., lower taxes), the only statistically significant long run multiplier (LRM) for aid when using the full sample comes for *Aid/GDP* and that particular LRM result is only statistically significant with 90% confidence. Furthermore, one can also observe that this single full sample LRM result when using *Aid/GDP* is driven entirely by the democratic sub-sample. There is no statistically significant LRM effect for aid in the non-democratic sub-sample when using *Aid/GDP* (or any other aid measure). At best for the political aid curse argument, there are some short-term effects as captured by the ΔAid variables, but these short-term aid curse results are always stronger in the democratic sub-sample than in the non-democratic sub-sample. And the only statistically significant short-term aid curse result for the non-democratic sub-sample comes when using Δ*Aid Per Capita*, which is only statistically significant with 90% confidence. These weak aid curse results in the non-democratic sub-sample are difficult to reconcile with either version of the political aid curse since both versions would predict that autocrats use their aid to reduce taxes in order to appease citizens living in their non-democratic regimes.

(ii) *SOCSPENDING*

The results when the dependent variable is government spending on education and health as a share of gross domestic

Table 5. *SOCSPENDING models (aid/oil curse sign +)*

	Full sample	Democracies	Non-democracies
Lagged DV	−0.05* (0.03)	−0.19*** (0.02)	−0.03 (0.02)
Undeflated aid$_{t-1}$	−0.03 (0.03)	−0.13** (0.06)	−0.02 (0.04)
Undeflated aid LRM	−0.67 (0.95)	−0.69** (0.30)	−0.49 (1.58)
ΔUndeflated aid	−0.01 (0.01)	−0.06* (0.03)	−0.02 (0.02)
Undeflated oil$_{t-1}$	−0.002 (0.010)	−0.05 (0.05)	0.005 (0.009)
Undeflated oil LRM	−0.03 (0.18)	−0.27 (0.24)	0.14 (0.30)
ΔUndeflated oil	−0.03*** (0.01)	0.02 (0.03)	−0.04** (0.02)
N=	3,311	1,379	1,932
Lagged DV	−0.05* (0.03)	−0.19*** (0.02)	−0.03 (0.02)
Aid/GDP$_{t-1}$	0.62* (0.32)	−0.02 (0.67)	0.55* (0.31)
Aid/GDP LRM	11.66** (5.67)	−0.12 (3.44)	15.87* (8.68)
ΔAid/GDP	0.12 (0.38)	0.25 (0.69)	−0.05 (0.30)
Oil/GDP$_{t-1}$	−0.01 (0.08)	0.14 (0.13)	−0.09 (0.06)
Oil/GDP LRM	−0.22 (1.40)	0.72 (0.64)	−2.52* (1.43)
ΔOil/GDP	0.08 (0.19)	0.46** (0.19)	−0.22** (0.09)
N=	3,308	1,375	1,933
Lagged DV	−0.05* (0.03)	−0.19*** (0.02)	−0.03 (0.02)
Aid Per Capita$_{t-1}$	−0.02 (0.05)	−0.20* (0.10)	0.04 (0.05)
Aid Per Capita LRM	−0.36 (1.03)	−1.02* (0.54)	1.08 (1.42)
ΔAid Per Capita	0.005 (0.048)	−0.08 (0.08)	0.03 (0.04)
Oil Per Capita$_{t-1}$	−0.0001 (0.0004)	−0.0005 (0.0047)	0.0001 (0.0004)
Oil Per Capita LRM	−0.003 (0.007)	−0.002 (0.024)	0.003 (0.012)
ΔOil Per Capita	−0.002 (0.001)	−0.001 (0.006)	−0.002 (0.001)
N=	3,312	1,379	1,933

Cell entries are OLS coefficients with robust standard errors clustered on the country in parentheses. Although not reported for space consideration, the control variables in all *SOCSPENDING* models include *Population*, *GDP*, *GDP per capita* (logged), *Growth*, *MID Involvement* and a full set of country fixed effects. Statistical significance is indicated as follows.
***$p < .01$ (two-tailed).
**$p < .05$ (two-tailed).
*$p < .10$ (two-tailed).

product are presented in Table 5 (with the aid LRMs graphed in Figure 2). When using the full sample, only one of the three aid measures (*Aid/GDP*) produces a long-term result (as captured by the LRM) that can be interpreted as consistent with the political aid curse. This result, unlike the TAXBURDEN result discussed above, is driven by the non-democratic sub-sample, although the positive *Aid/GDP* coefficient in the non-democratic sub-sample is only statistically significant with 90% confidence. Furthermore, this same result does not appear in any sample when using either *Undeflated Aid* or *Aid Per Capita*, suggesting that it is not a particularly robust political aid curse result (the statistically significant negative coefficients in the democratic sub-sample have the wrong sign for the political aid curse proposition, although they may be consistent with an economic aid curse).[13]

Although not reported here for space considerations, we also estimated the same set of models with education and health spending separated into different dependent variables. The same basic set of results emerge: aid does not systematically increase government spending in these public goods categories, suggesting that aid is unlikely to appease citizens who desire greater economic redistribution.

(iii) *MILSPENDING*

The results when the dependent variable is military spending as a share of gross domestic product are presented in Table 6 (with the aid LRMs graphed in Figure 3). Within the full sample, we find no long-term or short-term political aid curse result using any of the three aid measures. Notably there is evidence of both a long- and short-term aid curse effect in the democratic sub-sample, where *Undeflated Aid* and *Aid Per Capita* are positively associated with MILSPENDING. But it is important to note that neither repression result is apparent in the non-democratic sub-sample. The fact that we can find evidence that aid increases military spending, but only for democracies, is inconsistent with either version of the political aid curse.

Although not reported in this paper for space considerations, we also estimated the same set of models with military personnel as a share of the total labor stock as the dependent variable. The same basic set of results also appears for this potential repression dependent variable: aid can be associated with more military personnel, but only within the democratic sub-sample. There is no similar effect for the non-democratic sample or in the full sample.

(iv) *PHYSINT*

The results when *PHYSINT*, an inverted measure of actual repression with higher values indicating greater human rights protection, are presented in Table 7 (with the aid LRMs graphed in Figure 4). Here we observe no significant results that are consistent with a political aid curse, using any of the three aid measures or in any of the three statistical samples. There are, however, statistically significant results that run in the contrary direction, especially when using *Undeflated Aid*. These results could be read as consistent with a political aid *blessing*: if anything, aid increases human rights protection in democracies with no particular effect in non-democracies.

(v) *ANTIGOVT*

The results when using Banks and Wilson's (2013) index of anti-government activity (which considers revolutions,

Table 6. *MILSPENDING models (aid/oil curse sign +)*

	Full sample	Democracies	Non-democracies
Lagged DV	−0.37*** (0.10)	−0.48*** (0.05)	−0.39*** (0.12)
Undeflated aid$_{t-1}$	0.03 (0.04)	0.25** (0.12)	0.04 (0.04)
Undeflated aid LRM	0.08 (0.10)	0.52** (0.23)	0.09 (0.10)
ΔUndeflated aid	0.02 (0.02)	0.16** (0.07)	−0.05 (0.04)
Undeflated oil$_{t-1}$	−0.03 (0.04)	0.02 (0.04)	0.02 (0.09)
Undeflated oil LRM	−0.09 (0.10)	0.04 (0.09)	0.04 (0.23)
ΔUndeflated oil	−0.08 (0.11)	0.02 (0.05)	−0.16 (0.13)
N=	1,927	956	971
Lagged DV	−0.37*** (0.10)	−0.47*** (0.04)	−0.42*** (0.11)
Aid/GDP$_{t-1}$	2.05 (1.78)	−0.52 (0.81)	4.33* (2.46)
Aid/GDP LRM	5.50 (5.66)	−1.12 (1.75)	10.37 (7.09)
ΔAid/GDP	−0.40 (0.61)	0.03 (0.62)	−0.57 (0.92)
Oil/GDP$_{t-1}$	0.86** (0.38)	−0.17 (0.31)	1.24 (1.05)
Oil/GDP LRM	2.30** (1.10)	−0.37 (0.65)	2.97 (2.91)
ΔOil/GDP	1.02 (0.68)	−1.51*** (0.50)	1.75* (0.91)
N=	1,929	956	973
Lagged DV	−0.37*** (0.10)	−0.48*** (0.04)	−0.39*** (0.12)
Aid Per Capita$_{t-1}$	0.13 (0.09)	0.27** (0.13)	0.20 (0.17)
Aid Per Capita LRM	0.35 (0.28)	0.57** (0.27)	0.50 (0.49)
ΔAid Per Capita	0.04 (0.08)	0.19** (0.10)	−0.03 (0.12)
Oil Per Capita$_{t-1}$	0.003 (0.009)	0.06 (0.04)	0.005 (0.012)
Oil Per Capita LRM	0.01 (0.03)	0.13 (0.08)	0.01 (0.03)
ΔOil Per Capita	−0.02*** (0.01)	−0.01 (0.02)	−0.02** (0.01)
N=	1,929	956	973

Cell entries are OLS coefficients with robust standard errors clustered on the country in parentheses. Although not reported for space consideration, the control variables in all *MILSPENDING* models include *Population*, *GDP*, *GDP per capita* (logged), *Growth*, *MID Involvement* and a full set of country fixed effects. Statistical significance is indicated as follows.

***$p < .01$ (two−tailed).

**$p < .05$ (two−tailed).

*$p < .10$ (two−tailed).

Table 7. *PHYSINT models (aid/oil curse sign −)*

	Full sample	Democracies	Non−democracies
Lagged DV	−0.50*** (0.03)	−0.62*** (0.05)	−0.50*** (0.04)
Undeflated Aid_{t-1}	0.07* (0.04)	0.13* (0.07)	0.08 (0.08)
Undeflated aid LRM	0.13* (0.07)	0.21** (0.09)	0.15 (0.15)
ΔUndeflated aid	0.05* (0.03)	0.09* (0.04)	0.11* (0.06)
Undeflated oil_{t-1}	0.03 (0.04)	0.01 (0.15)	−0.002 (0.031)
Undeflated oil LRM	0.06 (0.07)	0.02 (0.24)	−0.003 (0.061)
ΔUndeflated Oil	−0.17* (0.09)	−0.25** (0.10)	−0.14 (0.11)
N=	2,742	1,223	1,519
Lagged DV	−0.50*** (0.03)	−0.61*** (0.05)	−0.51*** (0.04)
Aid/GDP_{t-1}	0.01 (0.67)	1.05 (0.85)	−1.03 (0.97)
Aid/GDP LRM	0.03 (1.35)	1.71 (1.33)	−2.03 (1.96)
ΔAid/GDP	0.13 (0.42)	−0.23 (0.85)	0.25 (0.58)
Oil/GDP_{t-1}	0.16 (0.11)	0.11 (0.08)	0.02 (0.33)
Oil/GDP LRM	0.33 (0.22)	0.17 (0.14)	0.05 (0.66)
ΔOil/GDP	−1.02** (0.48)	−0.52 (0.37)	−1.71** (0.66)
N=	2,744	1,223	1,521
Lagged DV	−0.50*** (0.03)	−0.62*** (0.05)	−0.50*** (0.04)
Aid Per $Capita_{t-1}$	0.08 (0.08)	0.25** (0.10)	−0.19 (0.12)
Aid Per Capita LRM	0.16 (0.16)	0.40*** (0.15)	−0.38 (0.24)
ΔAid Per Capita	0.03 (0.08)	0.11 (0.09)	−0.11 (0.10)
Oil Per $Capita_{t-1}$	−0.004*** (0.002)	−0.003 (0.009)	−0.005** (0.002)
Oil Per Capita LRM	−0.009*** (0.003)	−0.004 (0.014)	−0.009** (0.004)
ΔOil Per Capita	−0.02*** (0.00)	−0.06*** (0.02)	−0.02*** (0.00)
N=	2,744	1,223	1,521

Cell entries are OLS coefficients with robust standard errors clustered on the country in parentheses. Although not reported for space consideration, the control variables in all *PHYSINT* models include *Population*, *GDP*, *GDP per capita* (logged), *Growth*, *MID Involvement* and a full set of country fixed effects. Statistical significance is indicated as follows.
***$p < .01$ (two−tailed).
**$p < .05$ (two−tailed).
*$p < .10$ (two−tailed).

guerrilla warfare, assassinations, riots, strikes, government crises, purges, and anti-government demonstrations) are presented in Table 8 (with the aid LRMs graphed in Figure 5). Here one can observe statistically significant results using all three aid measures and in all three statistical samples. But none of these statistically significant aid coefficients take on the negative sign expected for a political aid curse (indicating that aid is used to suppress anti-government activity or otherwise appeases those who might mobilize against the government). To the contrary, these results show how aid appears to increase anti-government pressure perhaps by increasing societal expectations or by raising the stakes for government control.

(vi) *REGCHANGE*

Since the dependent variable of regime change is measured using a dichotomous variable, the results presented in Table 9 come from logit models. While the right-hand side variables all enter both as a lagged level and as a change variable, this is not a true ECM specification since we do not include a lagged dependent variable necessary to calculate the LRM. We instead control for serial auto-correlation by adding the age of the current political regime (using the Durable variable from Polity). [14] Given that these are logit models, we also omit the country fixed effects in order to reduce the inconsistency problems associated with maximum likelihood estimators (Greene, 2004).

The results in Table 9 (with the aid level coefficients graphed in Figure 6) do show some statistically significant aid coefficients (using *Undeflated Aid* in the full sample and either *Aid/GDP* or *Aid Per Capita* in the democratic sub-sample). But just like the ANTIGOVT results discussed above, the statistically significant aid results for REGCHANGE all enter with the wrong sign for a political aid curse, which expects aid to make regime change *less* likely (thus preserving the current government). Instead, one can observe how, at least for democracies, aid tends to increase the probability of regime change (with no statistically significant effect for non-democracies). [15]

(d) *An oil curse?*

Given only a few isolated results consistent with a political aid curse effect (and no robust results pointed in this direction), what about an oil curse? Certainly some scholars have found an oil curse result when political regime type is the dependent variable (e.g., Ross, 2001), but does a similar result emerge when various indicators related to appeasement and repression become the dependent variable?

The oil results in Tables 4–9 can be read as weakly consistent with those offered by Haber and Mendaldo (2011): there is simply not much robust evidence to support a political oil curse when indicators for appeasement, repression or both are the dependent variable, although the political curse effect for oil does appear to be somewhat stronger than the political curse effect for aid. In terms of appeasement, we find stronger evidence for an oil curse (than for an aid curse) in the TAXBURDEN models. In terms of repression, our results report some evidence consistent with an oil curse in certain PHYSINT models (unlike the opposite results for aid). Finally, we also find some short-term curse effect for oil in the ANTIGOVT models, which could be read as consistent with either the repression of anti-government activity or the appeasement of those who might otherwise mobilize against the current regime. But there is little evidence of an oil curse, either on

Table 8. *ANTIGOVT models (aid/oil curse sign −)*

	Full sample	Democracies	Non−democracies
Lagged DV	−0.73*** (0.02)	−0.80*** (0.03)	−0.72*** (0.03)
Undeflated aid$_{t-1}$	0.29*** (0.07)	0.47*** (0.13)	0.30*** (0.12)
Undeflated aid LRM	0.39*** (0.10)	0.58*** (0.18)	0.42*** (0.16)
ΔUndeflated aid	0.09 (0.10)	0.26*** (0.09)	−0.08 (0.24)
Undeflated oil$_{t-1}$	−0.04 (0.08)	−0.62*** (0.10)	0.02 (0.06)
Undeflated oil LRM	−0.05 (0.11)	−0.77*** (0.12)	0.03 (0.09)
ΔUndeflated oil	−0.11*** (0.04)	−0.44** (0.20)	−0.08* (0.04)
N=	4,075	1,586	2,489
Lagged DV	−0.73*** (0.02)	−0.81*** (0.03)	−0.72*** (0.03)
Aid/GDP$_{t-1}$	3.04*** (1.15)	6.09** (2.32)	2.80* (1.46)
Aid/GDP LRM	4.18*** (1.58)	7.55** (2.94)	3.89* (2.02)
ΔAid/GDP	−0.29 (1.19)	3.26** (1.33)	−1.85 (1.56)
Oil/GDP$_{t-1}$	−0.15 (0.33)	−1.68** (0.72)	0.23 (0.33)
Oil/GDP LRM	−0.20 (0.45)	−2.08** (0.90)	0.32 (0.46)
ΔOil/GDP	−0.87* (0.48)	−1.27 (1.10)	−0.65 (0.55)
N=	4,062	1,582	2,480
Lagged DV	−0.73*** (0.02)	−0.80*** (0.03)	−0.72*** (0.03)
Aid Per Capita$_{t-1}$	0.49*** (0.18)	0.65*** (0.18)	0.45 (0.35)
Aid Per Capita LRM	0.68*** (0.25)	0.80*** (0.23)	0.62 (0.49)
ΔAid Per Capita	0.17 (0.17)	0.18 (0.25)	0.20 (0.24)
Oil Per Capita$_{t-1}$	0.002 (0.001)	−0.07*** (0.02)	0.004** (0.001)
Oil Per Capita LRM	0.002 (0.002)	−0.08*** (0.03)	0.005** (0.002)
ΔOil Per Capita	0.0005 (0.0044)	−0.04* (0.02)	0.002 (0.004)
N=	4,077	1,586	2,491

Cell entries are OLS coefficients with robust standard errors clustered on the country in parentheses. Although not reported for space consideration, the control variables in all *ANTIGOVT* models include *Population*, *GDP*, *GDP per capita* (logged), *Growth*, *MID Involvement* and a full set of country fixed effects. Statistical significance is indicated as follows.
***$p < .01$ (two−tailed).
**$p < .05$ (two−tailed).
*$p < .10$ (two−tailed).

a long- or short-term basis, in the SOCSPENDING, MILSPENDING, and REGCHANGE models.

If state oil is more fungible, less conditional, and more constant over time than aid, then why do we not observe more of an oil curse across these various appeasement/repression indicators? There are two possibilities, both of which are consistent with our argument about relative fungibility, conditionality, and constancy. First, while state oil revenues *could* be used for repression and appeasement because of their fungibility, nonconditionality, and constancy over time, autocrats chose *not*

Table 9. *REGCHANGE models (aid/oil curse sign −)*

	Full sample	Democracies	Non−democracies
Undeflated aid$_{t-1}$	0.16** (0.08)	0.52 (0.40)	−0.16 (0.14)
ΔUndeflated aid	0.13** (0.06)	0.38 (0.43)	−0.03 (0.16)
Undeflated oil$_{t-1}$	−0.005 (0.078)	0.07 (0.12)	−0.16 (0.16)
ΔUndeflated oil	0.17** (0.08)	0.54** (0.25)	−0.02 (0.15)
N=	4,180	1,521	2,659
Aid/GDP$_{t-1}$	1.15 (0.80)	2.20* (1.27)	0.57 (0.90)
ΔAid/GDP	1.08 (0.84)	4.73*** (1.67)	−1.26 (1.25)
Oil/GDP$_{t-1}$	−0.34 (0.30)	−0.06 (0.32)	−0.70 (0.51)
ΔOil/GDP	−0.05 (0.43)	−0.62 (0.78)	0.62 (0.58)
N=	4,168	1,517	2,651
Aid Per Capita$_{t-1}$	0.36 (0.22)	0.58* (0.31)	−0.07 (0.24)
ΔAid Per Capita	0.19 (0.19)	0.37** (0.15)	−0.47 (0.31)
Oil Per Capita$_{t-1}$	−0.03* (0.02)	−0.02 (0.02)	−0.03* (0.02)
ΔOil Per Capita	0.01 (0.02)	0.002 (0.073)	0.005 (0.015)
N=	4,183	1,521	2,662

Cell entries are logit coefficients with robust standard errors clustered on the country in parentheses. Although not reported for space consideration, the control variables in all REGCHANGE models include *Population*, *GDP*, *GDP per capita* (logged), *Growth, MID Involvement*. Statistical significance is indicated as follows.
***$p < .01$ (two-tailed).
**$p < .05$ (two-tailed).
*$p < .10$ (two-tailed).

Table 10. *Statistically significant long-term results consistent with a political aid curse*

Full Sample

	TAXBURDEN	SOCSPENDING	MILSPENDING	PHYSINT	ANTIGOVT	REGCHANGE
Undeflated Aid						
Aid/GDP	√	√				
Aid Per Capita						

Democracies

	TAXBURDEN	SOCSPENDING	MILSPENDING	PHYSINT	ANTIGOVT	REGCHANGE
Undeflated Aid			√			
Aid/GDP	√					
Aid Per Capita			√			

Non-Democracies

	TAXBURDEN	SOCSPENDING	MILSPENDING	PHYSINT	ANTIGOVT	REGCHANGE
Undeflated Aid						
Aid/GDP		√				
Aid Per Capita						

to use their oil revenues for either of these alternatives to democratization. Instead, they use their oil revenues for private goods (although we do not have the space to test this possibility here). Second, much of the revenue captured in the various oil measures (calculated from Haber & Mendaldo, 2011) *does not go to the state*. This understanding leads directly to the argument advanced by Jones Luong and Weinthal (2010) to explain why oil should not systematically produce a political curse; it depends on the ownership structure: state *versus* private. Given the latter ownership structure, oil revenues would not accrue directly to the state. And in the situation of private ownership (either domestic or foreign), it simply does not matter that oil revenues are more fungible, non-conditional, and constant over time (compared to aid) because these considerations only produce a political curse when these revenues accrue to the state, instead of to private oil companies.

4. CONCLUSION

This paper has argued that foreign aid should not produce a political curse (i.e., hinder democratization) because it is poorly suited as a revenue source to paying for either appeasement or repression as alternatives to democratization given aid's relative infungibility, conditionality, and volatility over time. Using several different dependent variables related to appeasement and repression, we presented statistical evidence to show how there is no robust aid curse relationship for any of these dependent variables. Our evidence is inconsistent with either version of the political aid curse argument as we find no effect in a full sample of potential aid recipients or in the non-democratic sub-sample where the political aid curse effect should be the strongest. Our results thus match with aid/democracy *non*-results offered by Knack (2004), leading us to conclude cautiously that aid has no strong effect on political regime type.

In reaching this conclusion, it is important to acknowledge that aid may well have had anti-democratization effects in certain cases. Our large-N statistical research design cannot rule out this effect in special circumstances, but it strongly suggests that such cases are not typical of a broader aid pattern and that observers should be careful about inferring from exceptional situations. It is also important to acknowledge that our statistical analysis does present a few isolated results that could be selectively read as supportive of a broader political aid curse.

Table 10 shows where we did find statistically significant long-term evidence consistent with the proposition of a political aid curse using any of the 6 appeasement/repression dependent variables for our 3 aid measures in our 3 statistical samples. Given 54 regressions, one would expect to observe *by chance alone* 2–3 results that are consistent with a political aid curse result. Table 10 shows 6 statistically significant results, which is indeed more than one would expect by chance alone. But there is only a single long-term aid curse result in the non-democratic sub-sample and that comes for increased social spending (SOCSPENDING) when using *Aid/GDP*. While this one result is certainly consistent with a political aid curse through appeasement, it is also consistent with aid facilitating economic development with increased public goods, a more positive interpretation of this isolated result. But if one chooses to draw conclusions from any of the isolated results summarized in Table 10, then it should be noted there are just as many statistically significant aid coefficients that run contrary to the political aid curse, including some that would suggest a political aid blessing.

Finally, while our analysis deliberately focused on intermediate variables related to the political aid curse, we cannot rule out that there may be other appeasement and repression indicators that would reveal more evidence consistent with this phenomenon. We also cannot rule out that there may be strategies other than repression and appeasement that would allow autocrats to delay democratization pressures. But at this point in time, we lack a coherent theory of political development that identifies these other strategies and how they operate differently from repression and appeasement. Such a theory may help salvage the political aid curse proposition, but we leave this task for other scholars.

NOTES

1. "Editorial: The butterfly effect and the Arab Spring" posted May 31, 2011: http://www.idea.int/news/butterfly-arab-spring.html.

2. Ross (2012) argues that oil revenues tend to be unstable over time. As evidence on this point, he presents a figure showing how the price of a barrel of oil has experienced two major upward price swings in the post-1960 period (p. 51). In response to this evidence, two comments are in order. First, upward price swings do not produce much of a volatility problem for oil producers (unlike for oil consumers) since these spikes, if anything, tend to increase (not decrease) oil revenues. Second, oil *price* volatility by itself does not represent strong evidence of oil *revenue* volatility since revenues are price multiplied by supply. To the extent that upward price movements are associated with reductions in supply, oil revenues may tend to be quite stable over time even with price swings; evidence on this point will be presented later in the paper.

3. According to Easterly and Pfutze (2008, p. 45), tied aid and technical assistance represents about 45% of total aid, both bilateral and multilateral.

4. We recognize that MILSPENDING could also be a narrow appeasement indicator given that governments need to satisfy their militaries in order to remain in power. Fortunately, this appeasement logic would put the same positive sign on the aid coefficient as would the repression logic.

5. REGCHANGE takes on a value of 1 when Durable equals 0, indicating that there has been a regime change of 3 or more points using the 21-point Polity scale. Otherwise, REGCHANGE takes on a value of 0, indicating no political regime change (or regime stability).

6. We calculate the LRM and its standard error using non-linear combination command in Stata (nlcom).

7. The aid measures used here are thus different from the one used by Morrison (2009), which considered only the grant element of foreign aid. Since foreign aid in the form of official development assistance (ODA) and official aid (OA) includes both elements (grants plus highly concessional loans), our aid measures are more consistent with the traditional understanding of what constitutes foreign aid. The correlation between the aid per capita measure used in this paper and the grants per capita measure used by Morrison is 0.61.

8. All of these oil variables are calculated using the Haber and Mendaldo (2011) dataset: https://iriss.stanford.edu/sshp/datasets. It should be noted that these measures do not distinguish between the oil revenues accruing to the state and the same accruing to private oil companies.

9. The data for *Population*, *GDP*, *GDP per capita* (logged), *Growth rate of GDP per capita* come from the World Bank (2012).

10. The data for *MID Involvement* come from Ghosn, Palmer, and Bremer (2004).

11. This unit of analysis excludes all countries that are aid donors, treating all non-donors as potential aid recipients even if they receive no foreign aid.

12. One could also interact the aid variables with a measure of *Democracy* (e.g., Polity), but the ECM specification is not particularly "friendly" to interaction terms since all independent variables enter as lagged levels and through a change measure. This means that interacting aid with democracy in a true ECM specification would include four interaction terms (*Aid*Democracy*, *Aid*ΔDemocracy*, *ΔAid*Democracy*, and *ΔAid*ΔDemocracy*) next to four component terms (*Aid*, *ΔAid*, *Democracy*, *ΔDemocracy*). Given the challenges of identifying marginal effects in such a model specification, we opt for the simpler split sample alternative.

13. In this regard, our results differ from those in Morrison (2009, p. 127), who reported that grants per capita are significantly associated with greater social spending in a sample of dictatorships. But when (using his replication dataset) we replace his grants per capita independent variable with our *Aid Per Capita* (capturing both grants and highly concessional loans), we find no statistically significant positive relationship between aid and social spending in dictatorships.

14. We also created cubic splines for the age of the political regime, but our maximum likelihood estimates in the democratic sub-sample did not converge when these splines were included. For a consistent specification across all three samples, we thus omitted the splines. But the statistical significance of the aid terms does not change in either the full or non-democratic sub-sample when the cubic splines are included.

15. It should be noted that these results differ from those reported in Morrison (2009, p. 119). Using a grants per capita independent variable, he found a statistically significant negative correlation with the same dependent variable. But when (using his replication dataset) we replace his grants per capita independent variable with our *Aid Per Capita* (capturing both grants and highly concessional loans), we find results similar to those reported in Table 9: no statistically significant aid effect on regime change.

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