

Curses and Conditionality

Do Oil and Aid Affect Democracy Differently?

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Abstract

Do natural resource rents such as oil revenue and foreign aid affect the prospects of democratization in the same way? This paper examines one avenue through which these resources influence democratization: government spending during times of economic crisis. This paper argues that oil revenue props up government spending during times of crisis, helping autocratic regimes stay in power. Aid dependency, however, often entails conditions aimed at reducing the size of the state. During times of crisis, aid dependency increases spending cuts and thus exacerbates the potential democratizing effect of crisis. Empirical evidence from transitions to democracy indicates that aid dependency increases the likelihood that economic crisis precipitates democratization; natural resource rents, on the other hand, attenuate the crisis effect. Analysis of government spending and calorie consumption in non-democracies is also consistent with the argument.

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Are foreign aid and oil revenue the same? While much research in the past decade has addressed the effect of oil on democracy and regime stability more generally, recently scholars have begun to examine how foreign aid influences democracy. Aid may be similar to oil because it is also a potentially fungible resource that authoritarian governments can use to help them stay in power. Indeed both types of revenue come from sources that are largely independent of taxes from citizens, a commonality that underpins much of the discussion comparing aid and oil.

Beginning with Ross's seminal study identifying a negative correlation between natural resources wealth and democracy, a substantial body of research suggests that oil rents and perhaps other types of natural resource wealth can explain the persistence of authoritarianism across a broad range of countries.¹ Recently, scholars have also identified a negative correlation between foreign aid and democratic political institutions (Djankov, Montalvo and Reynal-Querol, 2008). This empirical finding suggests that foreign aid may be even more detrimental to democracy than oil wealth. The aid curse mantra has since popped up in the popular press, policy circles, and online news and analysis – adding further support to the burgeoning aid critic industry.²

If foreign aid is not only of little help in generating economic growth, but also hurts the prospects of democratization, donors that care about democracy in recipient countries may need to rethink their aid policies. Given the priority democratic development now carries in strategic relationships between wealthy democracies and much of the rest of the world, and the renewed emphasis on large aid transfers,³ understanding how aid and natural resource rents affect the prospects of democratization is even more pressing.

While foreign aid and oil revenue are both potentially fungible resources that accrue to authoritarian governments, they differ in important ways: both in the source of the revenue and the modalities of delivery. With regard to the latter, Collier (2006, 1485) points out that aid is delivered through “technical assistance, projects, packages with conditions, and debt relief” – each of which may have different effects on outcomes such as governance, institutional development, and even democracy. The argument in this paper, however, focuses on the *source* of non-tax revenue: donor governments and financial institutions or nationalized oil firms.

To understand why oil and aid may differ, I begin by asking whether the source of non-tax revenue shapes the spending behavior of the government. Foreign aid donors sometimes attach requests for policy reform to aid disbursements, which can influence the behavior of recipient governments. To the extent that this is true, foreign aid may be an instrument donors use to buy policy reforms in recipient countries (Bueno de Mesquita and Smith, 2007). Non-tax revenue from national oil companies, alternatively, rarely arrives in the national treasury with requests for policy

¹See Haber and Menaldo (2011) for a dissenting view and Dunning (2008a) on inequality and the relationship between resource wealth and democracy.

²Harford and Klein (2005), Easterly (2006), Moyo (2009), and Hubbard and Duggan (2009) offer recent critiques of the foreign aid and the international organizations involved in delivering aid.

³In her first address as U.S. Secretary of State in January 2009, Hillary Clinton argued for foreign aid and development to reside “along with defense and diplomacy,” as the three pillars of U.S. foreign policy. See John Whitesides, “Clinton promises to bolster foreign aid programs.” *Reuters*, January 23 2009. available at: <http://tinyurl.com/3jqzlhb> [accessed September 4, 2011].

reform. Thus foreign aid and oil dependency may differ in the policy constraints they impose.

Recent research posits that under some conditions foreign aid donors can influence politics in recipient countries and finds that the effect of foreign aid on outcomes such as tax revenue, economic growth, and democracy can vary substantially by time period (Dunning, 2004; Bermeo, 2009; Wright, 2009; Bearce and Tirone, 2010; Bermeo, 2011; Dietrich, 2011; Clist and Morrissey, 2011). Bearce and Tirone (2010), for example, show that aid is correlated with economic growth but only in the post-1990 period; while Wright (2009) finds that aid is associated with a higher likelihood of democratic transition in large-coalition dictatorships, but again only after the Cold War. Dunning (2004) and Bermeo (2009) revisit prior work in Goldsmith (2001) and Morrison (2009) to show that the effect of aid on democratization and authoritarian stability varies substantially by time period: aid is correlated with democratization in the 1990s but not before, and only stabilizes dictatorships during the Cold War but not afterwards. If the influence of foreign aid varies over time, one explanation may lie in the increased ability of donors to buy reform in recipient countries in the post-1989 period.

Theoretical work linking non-tax revenue to democracy and regime stability suggests that these ‘free-resources’ affect spending choices in recipient governments (Ross, 2001; Smith, 2004; Morrison, 2007; Smith, 2008; Morrison, 2009; Ross, 2009; Tsui, 2010). Smith (2004), for example, argues that because there is little evidence that repression is the mechanism through which oil wealth affects regime stability, government spending on a durable support coalition may offer a more compelling explanation. More recently, Ross (2009, 15) reviews the evidence for different causal pathways through which oil revenue might influence the prospects of democratization – such as modernization, repression, civil society formation, and spending – and finds that the rentier effect receives the most empirical support. He concludes that oil wealth helps authoritarian governments spend more, increasing their “ability to buy support.” These theories point towards government spending as the mechanism through which natural resource wealth and potentially other non-tax revenue affect democracy.

This paper examines how foreign aid and oil rents influence the prospects of democratization by focusing on how these revenues shape government spending. While aid and oil may both increase an authoritarian government’s capacity to pay off potential democratizing agents and deter democracy, they differ in one important regard. In most cases, oil revenue accrues to authoritarian governments directly from the revenue-generating operations of state oil companies. In some cases, oil revenue also arrives in the coffers of the government via taxes on international firms or the sale of concessions to exploit oil reserves.

Foreign aid, on the other hand, accrues to authoritarian governments via donor aid agencies and multilateral institutions such as the IMF and the World Bank. These donors frequently attach conditions to the receipt of aid. In many cases, these conditions are not enforced by the donor (Svensson, 1999; Stone, 2004, 2008) or even if they are, the recipient still lacks the incentive to meet the conditions and would prefer to forgo aid. But under some circumstances, donors enforce conditionality and recipient governments have an incentive to comply with the conditions to

continue receiving aid. In these cases, even if foreign aid is not invested wisely to help generate economic growth, it still may influence the policy choices of the recipient government (Collier, 1997). A common economic policy reform contained in aid agreements is the reduction in the size of the state sector, which in most cases means reducing government spending. Thus aid dependency may decrease short-term spending by recipient governments even if aid investments do not spur growth. Natural resource rents and foreign aid may therefore provide different constraints on government behavior.

I focus this argument on periods when authoritarian governments are most likely to need non-tax revenue, which should be greatest during times of economic crisis. When the economy slumps, authoritarian regimes not only have fewer resources generated from domestic economic activity – for example, less tax revenue from trade – but citizens are hurt as well. If economic crisis combines tax revenue loss with increased potential for backlash from citizens hurt by economic hard times, a crisis can threaten the stability of an authoritarian regime. This is also precisely when ample non-tax resources may be of most help to the regime in deterring democracy by boosting payments to key members of the support coalition. Combining (1) the fact that aid dependency can constrain the recipient government’s fiscal options whereas oil wealth may entail no such constraints, with (2) the intuition that authoritarian regimes are most vulnerable during times of economic crisis provides the building blocks for a theory suggesting that aid and oil affect the prospects of democracy differently.

Non-tax revenue, economic crisis, and democratization

How does non-tax revenue keep dictators in power? Numerous theories posit that these resources lower the likelihood of democratization by helping dictators spend government resources on citizens who might otherwise demand democracy or possibly revolt. For example, Ross (2004) suggests that the link between taxation and the demand for representative democracy results from imposing taxes on citizens with little government spending in return. This logic implies that if non-tax revenue can finance government spending, there will be less demand for democracy. Others argue that non-tax revenue can enable dictators to pay off poor citizens with social spending or public goods, thereby reducing their incentive to revolt (Smith, 2008; Morrison, 2009). Remmer (2004, 80) raises the possibility that foreign aid increases government spending because it permits the political leaders to “reward their followers or otherwise enhance their chances of political survival” without incurring the political costs associated with taxing citizens.

For these studies, the mechanism that links non-tax revenue to political stability runs through government spending: dictators use non-tax resources to prevent opponents from mobilizing against the regime and demanding democracy. If the first part of these causal stories is government spending, the second is mobilizing collective action: government spending funded by non-tax revenue is important for authoritarian stability insofar as it can alter the incentives for collective mobilization against the dictator.

Economic crisis may increase the likelihood of collective mobilization and democratization

(Callaghy, 1990; Gasiorowski, 1995; Haggard and Kaufman, 1995; Bratton and van de Walle, 1997; Acemoglu and Robinson, 2006; Yap, forthcoming). The mechanisms linking crisis and democratization in this literature vary but many focus on the relationship between crisis and the mobilization of opponents against the dictator. For example, an economic crisis can provide a focal point for opposition protest or deplete the resources available to the regime to pay off nominal allies and repress potential opponents. If economic crises create opportunities for regime opponents to overcome collective action constraints and threaten the dictator’s rule, then we will want to know how non-tax resources shape the dictator’s response during these periods.

This point plays an important role in the literature of the distributional foundation of political regimes. Recent theoretical work in this area models democratization as the outcome of a conflict between two classes in society, citizens and wealthy elite (Acemoglu and Robinson, 2001, 2006; Boix, 2003; Houle, 2009; Ramsay, 2011). Structural conditions, such as the level of inequality or the asset mobility of the wealthy, shape the behaviors of both the authoritarian elite actor and the citizens. Morrison (2007) further incorporates non-tax revenue into these theories to show how resources stabilize authoritarian rule by increasing social spending on citizens under dictatorship, thereby reducing the incentive to mobilize against the regime.

The starting point for these formal models is the premise that there exist periods of time during which citizens have the capacity to overcome collective action costs and credibly threaten the dictator with revolution. Collective action is costly and, as Houle (2009, 595) points out, “if the masses are unable to mobilize, the elites have no incentive to respond to changes in inequality by adopting democracy.”

While Acemoglu and Robinson (2006, 145) do not dwell at length on circumscribing the intermittent time periods when the revolutionary threat is binding,⁴ they do offer a suggestion in this regard: during periods of economic crisis. Pressure for democracy occurs once citizens surmount the collective action threshold, and economic crises constitute those fleeting moments when the ‘revolution constraint’ is met. This suggests that empirical tests of these types of models should focus on observed time periods when we would expect increased collective action. If non-tax revenue hurts the prospects of democratization by deterring the threat of revolution, we should expect this effect during times of economic crisis – precisely when the revolution threat is most likely to be credible.

This latter argument fits nicely with the rentier thesis in the resource curse literature. In summing up recent findings for an oil curse, Ross (2009) suggests that the mechanism linking oil and authoritarianism which finds the most empirical support is the rentier effect. A central piece of the rentier effect concerns government spending. Simply put, with more resources to buy support, oil rich authoritarian regimes can survive longer by using resource spending to forestall collective action against the regime. Ross (2009, 20) notes, for example, that “authoritarian governments use gasoline subsidies as both a public good, which helps boost their popularity, and to avoid protests,

⁴To be precise, Acemoglu and Robinson group a number of different concepts into the revolution constraint: the technology of revolution, the amount of destruction to the total economy during successful revolution, and the collective action costs of the citizens.

which in authoritarian states can escalate into pro-democracy movements.” In Morrison’s (2007) model, it is precisely these additional non-tax revenues which authoritarian governments pay to citizens to deter a revolution.

Thus the implications of the rentier thesis can be applied to the literature on economic crisis, especially arguments which suggest that crises reduce authoritarian regimes’ ability to buy political support. During economic crises, non-tax revenue should bolster regimes’ efforts to purchase support by propping up government spending. If oil revenue and foreign aid are similar sources of non-tax revenue, they should both deter democratization during hard economic times by increasing spending on citizens who might otherwise mobilize against the regime.

Oil

Theories linking oil wealth to the dearth of democracy have assessed various causal pathways through which oil operates to impede the development of democratic political institutions: government spending, repression, economic development, civil society, and even price volatility. While the cross-national macro-evidence – with the exception of Haber and Menaldo’s (2011) recent analysis – consistently finds in favor of the oil curse, researchers have had more difficulty establishing robust evidence in favor of one or more of the hypothesized causal mechanism. Thus some have suggested that the anti-democratic effect of oil may be isolated to particular political contexts – such as in countries with low inequality or state capacity (Dunning, 2008*a*; Morrison, 2011).

These theories posit a direct effect for oil wealth, wherein oil influences the long-term development of political institutions. The conditional literature argues that long-term structural characteristics of the political context – factors unlikely to change quickly over time – alter the effect of oil wealth. Both sets of theories, however, do not consider how oil might condition other factors which vary in the short-term, such as economic growth.

However, the influence of oil on democracy should occur when citizens have a higher likelihood of mobilizing against the regime – that is, during periods of economic crisis. Thus the average (negative) effect of oil may simply be picking up the strong effect of oil during periods of heightened potential collective action. Two underlying mechanisms could explain why oil rich autocracies are better able to survive economic crises than their resource poor counterparts.

First, oil rich countries can stockpile rents which can later be spent to dissipate potential pro-democratic collective action when crises make the regime most vulnerable.⁵ Oil rich countries can stockpile resources in sovereign wealth funds (SFWs) or more liquid assets. Indeed, recent analysis of SFWs suggests that countries with high fuel exports and less democratic institutions are the most likely to store resources in this way (Aizenman and Glick, 2009). Further, the lack of accountability in dictatorships may mean that individual leaders can store resource rents in private assets. For example, an estimated \$4.2 billion of government revenue in oil-rich Angola could not be accounted for between 1997 and 2002 (Ross, 2008). Estimates suggest that prior at the start of the rebel

⁵Kono and Montinola (2009) suggest this is one way in which foreign aid can keep authoritarian rulers in power. They do not test how stockpiled aid conditions the relationships between economic crisis and democracy, however.

insurgency against him in February 2011, Muammar Gaddafi had access to over \$6 billion in gold reserves within Libya.⁶ In Gabon, *Le Monde* reported that elites within Bongo’s regime stole FF28 billion in the two decades prior to 1990 – a sum nearly twice as large as the country’s national debt in 1990 and an order of magnitude larger than all government salaries paid out in the 1980s (Gardinier, 1997, 150).⁷

In times of crisis, these resources can be deployed to buy crucial political support. Consider an example from Gabon. While fiscal austerity associated with IMF reforms in the late 1980s prompted riots in early 1990 and nominal multiparty elections later that year, President Bongo initially responded with increased public sector pay and more benefits for striking workers (Bratton and van de Walle, 1992, 423). In the end, the regime survived political reform and multiparty elections, because it “enjoyed a steady flow of revenues from oil and mineral exports and may have calculated that they could still dominate the political game, even during and after reform” (Bratton and van de Walle, 1992, 435).

Evidence from the mobilization of pro-democracy forces in 2011 throughout the Middle East and North Africa suggests that a first response of many oil rich regimes is to increase payments to a broad swathe of the population.⁸ This implication of the stockpile argument differs from the classic rentier thesis, which posits that fiscal crisis jeopardizes the capacity of the rentier state to maintain support with lavish spending on the state sector (Luciani, 1994). Rather, with stockpiled wealth oil rich regimes may have sufficient resources to address the (economic) demands of potential democratizing agents during crisis and weaken the opposition.⁹ While economic crises may hurt government finances as well as citizens, the stockpile argument suggests that the fiscal retrenchment in oil rich states in response to economic crises should be not be as deep as in resource-poor countries.

A second argument posits that oil rich autocracies may be particularly resilient in the face of economic hard times because resource wealth re-enforces the dominance of an *a priori* strong regime with a durable coalition of supporters (Smith, 2004, 2006). Even in the face of oil busts – or a rapid decline in the price of oil and hence of oil rents – dictatorships with strong coalitions can survive economic crises.¹⁰ This argument takes strong coalitions as the prior causal factor and relegates oil rents to a contributing but not sufficient explanation for the persistence of authoritarian rule.

The durable coalitions mechanism is consistent with the logic that political leaders have a

⁶Jack Farhy and Roula Khala. “Gold key to financing Gaddafi struggle.” *Financial Times*, March 21 2011.

⁷See Jensen and Wantchekon (2004, 820) for more examples.

⁸For example, one source reports that Libya offered \$450 in cash for each family, a 150% wage increase for civil servants, and cut taxes on food. The Algerian government promised over \$150 billion in new infrastructure spending and decreased taxes on sugar. “Subsidies for peace.” *The Economist*. March 12, 2011 (p. 32).

⁹Resource rents can also fund campaigns to weaken the opposition. For example, with the introduction of multiparty politics in resource-rich Gabon and Zaire in the early 1990s, Bongo and Mobutu funded numerous opposition parties, making it more difficult for a coherent opposition party to form and contest power (Bratton and van de Walle, 1992; Schatzberg, 1997; Ghazvinian, 2007). Further, oil resources may induce pro-regime foreign intervention in the face of protest when the regime pays substantial rents to foreign oil firms. Strong clientelistic connections between French oil firms and Bongo’s elite in Gabon provided sufficient rationale for French military intervention to support the Bongo government against protesters in 1990 (Ghazvinian 2007, 110-111; Yates 1996).

¹⁰This evidence is consistent with Colgan’s (2011) finding that oil price fluctuations do not have an appreciable effect on the level of democracy in oil-producing states.

stronger incentive to invest in the oil sector if they expect to survive in power (Dunning, 2010); that is, resilient regimes may have more oil rents because the expectation of maintaining power over long-term increases investment and hence oil rents. This argument also fits nicely with the expectation that political institutions in dictatorships—such as legislature and parties— are instrumental for authoritarian survival because they enable dictators to make credible inter-temporal promises to their support coalition (Gandhi and Przeworski, 2006; Gandhi, 2008). By regularizing bargaining and increasing the costliness of renegeing on promises of policy concessions or future rents, strong institutions can deter collective action by potential democratizing agents when the regime has fewer current resources with which to pay for the coalition’s support.¹¹ Oil rents, Smith argues, should re-enforce the capacity of institutionalized regimes to co-opt potential opponents.

Both the durable coalition mechanism and the stockpile argument suggest that oil wealth should deter democratization in the face of economic crisis. Both should therefore be tested when the regime is responding to shocks that mobilize citizens. However, the implications of these two arguments diverge with respect to government spending. The stockpile logic suggests that oil rich regimes have sufficient resources – even when current rents are declining – so that they do not have to curtail spending during times of crisis. With stockpiled resources, they can still pay off their coalition and survive the crisis. The durable coalition argument, on the other hand, suggests that governments can cut spending in response to crises because these institutionalized regimes can credibly promise future payments or substitute policy concessions for current spending. Thus if the latter were true, oil wealth should not necessarily affect the fiscal response to crises.

The stockpile argument is difficult to directly test because it is unclear how we would measure stockpiles as distinct from current rents. For example, the (logged) value of cumulative oil rents accrued over the lifetime of an authoritarian regime (albeit from 1960) is correlated with (logged) lagged oil rents at 0.95, suggesting little difference between the two measures.¹²

Perhaps more difficult, though, is the lack of transparency in the finances of oil rich countries. For example, one recent report estimates that the Libyan Investment Authority (LIA), the country’s sovereign wealth fund, had between \$50 and \$70 billion in assets prior to the downfall of the Qaddafi regime – much of which was held in cash and other liquid assets.¹³ If these estimates are correct, they suggest that the resources stockpiled in Libya’s sovereign wealth fund may be nearly as large as the entire current GDP of the country. While no doubt large, experts are unsure of the total amount of stockpiled resources available to the Qaddafi regime.¹⁴ Ross (2012) cites examples from countries as diverse as Angola, Azerbaijan, Indonesia, and Iraq to suggest that oil wealth breeds government secrecy, where the finances of nationalized oil companies are quietly merged with the

¹¹Indeed this may be one reason that economic crises are the least stabilizing in dominant party regimes (Haggard and Kaufman, 1995).

¹²This estimate relies on the Geddes (2003) definition of a *regime*.

¹³“From Tripoli to Mayfair”. *The Economist*, March 12 2011 (p. 89): “the Libyan Investment Authority reportedly holds \$60 billion-\$80 billion in assets. No official numbers exist.” To put this number in perspective, the CIA estimates that Libya’s entire GDP in 2010 at exchange rate values is \$78 billion.

¹⁴See for example “Libya’s Hidden Wealth May Be Next Battle.” Landon Thomas, *New York Times* (March 3, 2011). available at: <http://www.nytimes.com/2011/03/04/business/global/04sovereign.html> [accessed March 23, 2011].

government budget and hidden from public view.

Aid

Foreign aid may differ from oil revenue when donors can credibly impose conditions on the recipient government. Aid conditionality often entails economic reform measures aimed at reducing the size of the state by curtailing government spending (Biersteker, 1990; Remmer, 1986; Taylor, 1997). Thus aid dependency, with enforceable conditionality, can buy economic reform that requires cutting government spending in general and possibly redistributive transfers to the poor in particular (Nooruddin and Simmons, 2006). If aid conditionality constrains the fiscal response of recipient governments by limiting their ability to forestall democratic collective action with payments from the state sector, aid dependency should exacerbate the democratizing effect of crises. Indeed, this may be one explanation for why IFI pressure is correlated with austerity protests (Walton and Ragin, 1990).

One objection to this argument is the possibility that the economic reform conditions attached to aid agreements are simply not enacted because conditionality remains un-enforced. For example, studies of specific aid programs have found only scattered evidence that recipients comply with a broad range of economic reform conditions (Remmer, 1986; Harrigan and Mosley, 1991; Devarajan and Holmgren, 2001). The cross-national empirical evidence on foreign aid and economic reform is also mixed (Dreher, 2005; Atoyán and Conway, 2006; Dreher and Rupperecht, 2007; Heckelman and Knack, 2008). However, spending may be different. van de Walle (2001, 68), for example, notes that while overall economic reform implementation has been patchy in Africa, “[s]ignificant progress has been made on cutting fiscal deficits.”

Further, much of the cross-national evidence for patchy reform implementation pools strategic and non-strategic aid. Economic reform conditions are unlikely to be met when donors use aid for strategic purposes because they cannot credibly withdraw aid when the strategic purpose is present and known to the recipient (Bearce and Tirone, 2010). Non-strategic conditional aid, on the other hand, may induce reform because donors can credibly threaten to withdraw aid. Studies which employ instrumental variables to address the issue of endogeneity in the aid-reform/growth literatures frequently use instruments which are correlated with foreign aid precisely *because of the strategic nature of aid*.¹⁵ This empirical approach may simply be picking up the variation in type of aid that should be the least likely to induce reforms because they are dealing with strategic aid.¹⁶ If this argument is correct, it should be no surprise that two-stage models which use strategic

¹⁵See, for example, Easterly (2005) and Heckelman and Knack (2008). Dummies variables for aid outliers, such as Egypt, and measures of military imports, which may be correlated with armed conflict, are two examples of *strategic* variables that explain total aid transfers but which are unlikely to be correlated with *non-strategic* aid. Djankov, Montalvo and Reynal-Querol (2008) explicitly follow this strategy of using strategic variables as ‘exogenous’ instruments for aid: “The instruments are basically the ones used by Burnside and Dollar (2000): the log of initial income, the logarithm of initial population and the group of variables that capture donors ‘strategic interests’ (z), which are basically regional dummies.” (notes to Table 4). Region dummies include: Egypt, Central America, the Franc zone, and sub-Saharan Africa. Morrison (2009) and Bueno de Mesquita and Smith (2010) lag aid and do not instrument for this variable.

¹⁶See Dunning (2008b) and Deaton (2009) for a discussion of issues of causal heterogeneity in 2SLS models.

variables as instruments find that aid hurts reform. To address the issue of compliance with aid conditions, some researchers delineate strategic from non-strategic aid by looking at different time periods, under the assumption that aid in the post-1989 period is more likely to be non-strategic and therefore could successfully induce economic reform (Dunning, 2004; Bearce and Tirone, 2010). As I discuss later, this is the primary strategy used in this paper.

This latter period may also have been a time when economic crisis precipitated democratization. Echoing the logic of the rentier thesis, van de Walle (2001, 240) notes that by the 1990s, “economic crisis was putting pressure on the state elite accommodation process” and “[w]ith fewer resources at their disposal... leaders found it harder to sustain critical clientelistic networks.” Economic crisis contributed to political discontent and protest, which in turn increased the likelihood of democratization. Indeed Bratton and van de Walle (1997) find evidence from the early 1990s that participation in structural adjustment programs in Africa is correlated with political protest, and that political protest is correlated with transitions to democracy.

Finally, the fiscal constraints imposed by aid dependency may be most operative in times of economic crisis. Most authoritarian countries are developing economies and these countries are less able to counter negative economic shocks with social spending than OECD countries (Rudra, 2002; Wibbels and Acre, 2003; Wibbels, 2006). This suggests that precisely during times of economic crisis, aid recipients may be most constrained in their fiscal response. Indeed, conditionality should have its strongest bite during these periods because aid-dependent governments have no other source of financing. Recent evidence, for example, finds that the implementation of reforms is less likely in countries with high growth because growth reduces the credibility of donor aid sanctions (Dijkstra, 2002).

In short, when foreign aid successfully buys reductions in the size of the state sector in recipient countries, aid dependency should constrain dictators’ fiscal response to economic crises. These constraints, in turn, decrease an authoritarian regime’s ability to compensate potential democratizing agents who are mobilized by economic crisis, with implications for how foreign aid dependency affects democratization.¹⁷

Hypotheses

When faced with democratizing pressure from citizens mobilized by an economic crisis, authoritarian regimes have an incentive to appease citizens by increasing government spending. Oil rents allow the government to cushion the effect of economic crisis on citizens by increasing spending. Thus oil wealth deters democracy by severing the link between economic crisis and democratization. Foreign aid and oil rents differ, however, because aid conditions prevent authoritarian regimes from increasing spending during times of crisis. Aid dependency can thus accentuate the link between

¹⁷While some have argued that aid may be a useful tool to compensate the ‘losers’ from economic reform and thus make reform more politically feasible (Bearce, 2009; Bearce and Tirone, 2010), others have found that some aid programs, such as IMF loans, are not very effective at compensating reform losers but frequently entail additional economic pain. One implication of Vreeland (2003) findings is that IMF participation hurts the poor by providing a mechanism for elites in aid recipient countries to tie their hands to painful reforms.

economic crisis and democratizing pressure by constraining an authoritarian regime’s ability to curb spending cuts when faced with an economic crisis. By hurting citizens more during an economic crisis, aid dependency should increase the likelihood that crisis precipitates democratization. This argument suggests two sets of expectations, one with respect to democratization, the other to spending.

Democracy

H1a: Aid dependency increases the likelihood that economic crisis precipitates democratization.

H1b: Oil rents decrease the likelihood that economic crisis precipitates democratization.

Spending

H2a: Aid dependency increases the size of spending cuts associated with economic crisis.

H2b: Oil rents decrease the size of spending cuts associated with economic crisis.

Two additional points are worth noting regarding these hypotheses. First, the argument about foreign aid dependency and spending hinges on conditionality having sufficient teeth to actually constrain recipient regime spending during times of economic crisis. As others have argued, aid conditionality is more likely to be binding in the post-1990 period (Dunning, 2004; Wright, 2009; Bermeo, 2009; Bearce and Tirone, 2010). Thus we should find stronger support for H1a and H2a during this period than during the Cold War period.

Second, the argument about spending assumes that government expenditure during economic crises can reduce the burden of the crisis on citizens who might respond to the crisis by mobilizing against the regime. However, total government expenditures do not necessarily entail spending on programs that target the poor – or any other group of potential democratizing agents. Ideally, this concern would be addressed by looking at more detailed types of spending data (perhaps health and education spending); however missing data on these measures in a sample of non-democracies prevents meaningful analysis.¹⁸ Instead, I examine how aid, oil, and crisis affect calorie consumption – an outcome that more closely captures the utility of citizens, especially those with lower incomes.

Illustrative examples

Before proceeding with the cross-national empirical analysis, this section provides illustrative evidence of the hypothesized causal mechanisms from time-series data within selected countries. Using a paired comparisons, Figure 1 shows non-tax revenue (oil and aid),¹⁹ spending and calorie consumption in two sub-Saharan African countries ruled by personalist dictators, Gabon and Malawi. Figure 2 shows the same data for two Latin American countries under dominant party rule, Mexico and Nicaragua. These paired comparisons do not control for every factor that varies between the

¹⁸For example, in the main sample for aid (102 countries from 1962-2008, N×T=3219) 80% of the observations for public education spending are missing and 91% of observations for public health spending are missing.

¹⁹Aid is constant dollars per capita (WDI 2010) and oil is constant dollar oil and gas rents per capita (Ross, 2008).

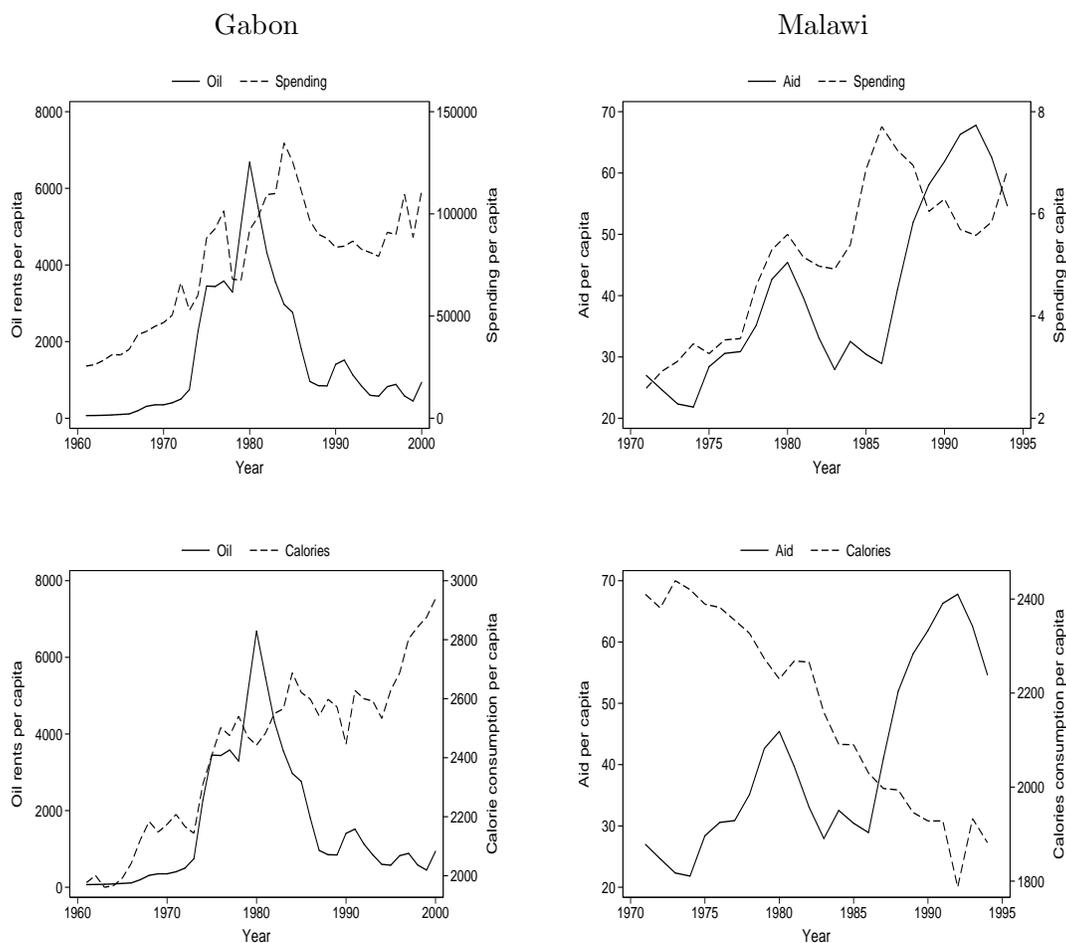


Figure 1: *Non-tax revenue sources, spending and calories in Gabon and Malawi.* Aid and oil rents are moving average over t and $t-1$ in constant per capita dollars. Spending and calories figures are the moving average over t and $t+1$.

respective countries but serve to illustrate the main causal argument.

The two aid dependent countries, Malawi and Nicaragua, averaged over sixty dollars in aid per capita in the late 1980s and early 1990s, putting them in the top twenty percent of aid recipients. Neither of these countries benefited from oil rents. The two oil dependent countries, Gabon and Mexico, averaged over \$250 dollars per capita in oil rents from 1986-1994, falling in the 84th and 91st percentiles of the oil dependency distribution. While Gabon also received substantial foreign aid, Mexico received almost none. In Gabon, however, oil rents per capita were nearly eight times as large as aid receipts.

The left panel of Figure 1 depicts the rise in oil rents in Gabon during oil boom of the 1970s, followed by a steep decline throughout the 1980s. Government spending and calories follow the rise of oil rents but these time-series diverge from oil rents once the latter begin their decline. Neither spending or calories face steep decreases similar to oil rents, even when the regime faced

severe crisis in 1990-1991. The right panel depicts the steady increase in aid in Malawi through the latter part of the 1970s – a pattern mirrored in the time-series for government spending. In the second half of the 1980s, however, these series diverge, with increasing aid receipts followed by lower spending levels. As aid increases sharply from 1986-1992, spending drops by nearly a third from its peak. Spending does not increase again until aid starts to decline in 1993-1994. This period coincides with economic decline: growth averaged -3% from 1986-1988, and in the early 1990s Malawi was hit with debilitating aid sanctions, part of the explanation for severe economic crisis from 1992-1994. The Banda regime fell in 1994. Overall, average calorie consumption declined sharply from 1970 to 1994, but even this overall trend reflects the opposite of what we expect if aid kept autocratic governments in power by increasing spending on the poor. The steepest decline in calorie consumption, in 1992, coincides with the peak of aid dependency. Thus prior to the mid-1980s, oil and aid receipts in these two countries parallel the movement in spending and in Gabon, calorie consumption. But after that, aid and spending/calories have a negative association, while in Gabon a steep decline in oil rents did not entail a large reduction in spending.

As similar set of patterns appear in Mexico and Nicaragua, two dominant party dictatorships. In Mexico, oil rents rise in the 1970s and then decline sharply in the 1980s. While spending and calorie consumption rise with oil, they level off as rents decline, suggesting that the PRI was able to main government spending even as oil revenue fell. The first half of the 1980s in Nicaragua saw a decline in foreign aid that coincided with a rise in government spending. The latter half of the decade – when the Sandanista regime faced severe economic crisis – the opposite occurs: aid increases while spending and calorie consumption decreases.

The within-country patterns from these four countries illustrate the main mechanism for how aid and oil dependency differ during times of crisis. In oil rich countries, even when short-term rents fall, these regimes maintain spending and their citizens retain high calorie consumption. However, in the two aid-dependent countries aid and spending diverge, particularly during times of crisis.

Data and methods

The empirical analysis in this paper builds on the transitions literature which measures democratization as binary variable marking a change in the group of ruling elite, often denoted as alteration in power (Alvarez and Przeworski, 2000). We begin with a sample of non-democracies. This ensures that we are not mixing information on democratic survival with cases of non-democracy that are at risk of *transitioning to democracy* (Ulfelder, 2007). The primary data set is an updated version of the Geddes authoritarian regimes data (Geddes, 1999, 2003; Wright, 2008; Geddes, Wright and Frantz, 2012). This data distinguishes between regime failures that end in a transition to democracy and failures that result in a transition to a subsequent authoritarian regime (Gleditsch and Choung, 2004; Wright and Escribà-Folch, forthcoming). Instead of grouping these two types of failure together, the following analysis looks only at transitions to democracy and treats transitions to subsequent authoritarian regimes as right-censored observations. The sample covers over 200 regimes in up to 105 countries from 1961-2008.

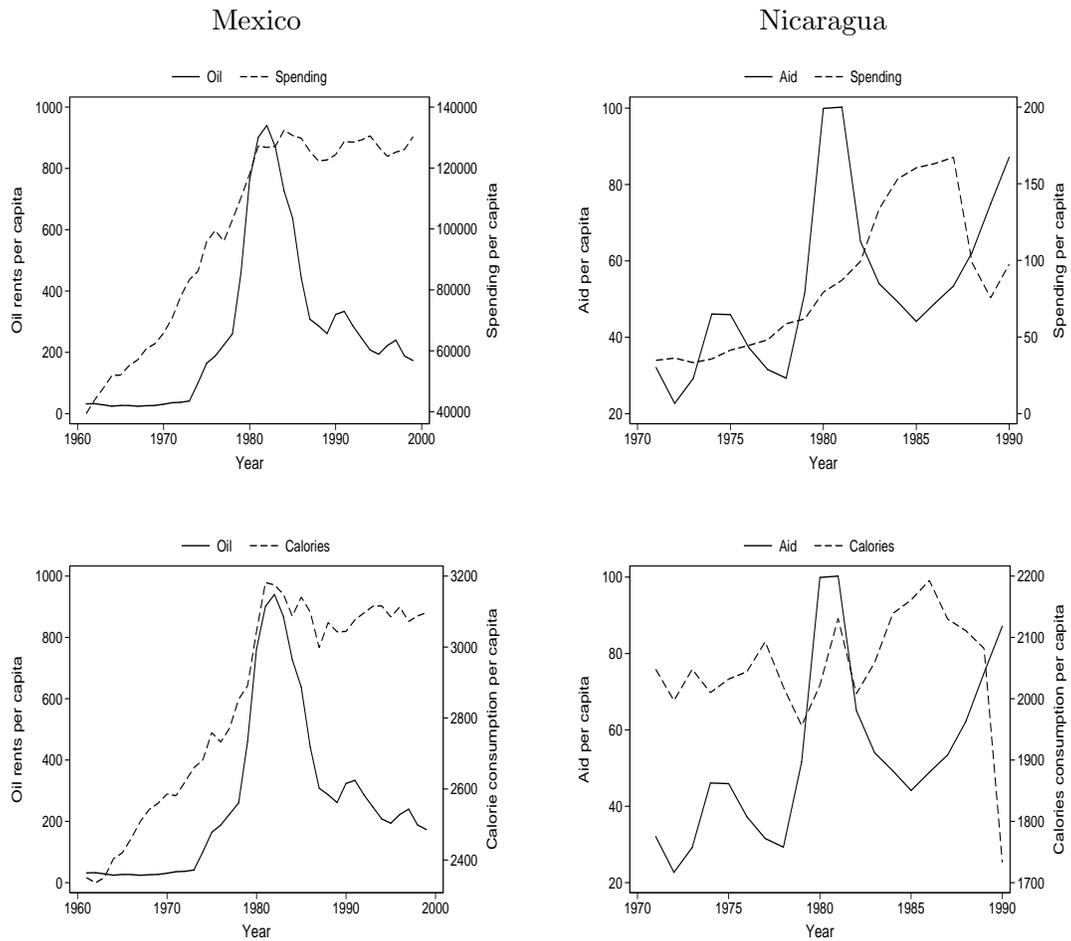


Figure 2: *Non-tax revenue sources, spending and calories in Mexico and Nicaragua.* Aid and oil rents are moving average over t and $t-1$ in constant per capita dollars. Spending and calories figures are the moving average over t and $t+1$.

To test the robustness of the empirical findings, the analysis also uses data from Chebibub, Gandhi, and Vreeland (2010, hereafter CGV). It covers more years and countries than the Geddes data because it includes all periods of non-democratic rule even if the territory is not governed by a cohesive state (e.g. Somalia since the fall of Siad Barre) or if a foreign power controls the state (Afghanistan after the U.S. invasion in 2001). The CGV sample covers up to 111 countries from 1961-2008.

While the Geddes and CGV data differ slightly, they both capture transitions in the group of elite that have access to power. Geddes' cases are coded by the group of elite that control selection of the leader, and the CGV data codes democratic transitions when there has been turnover of power. The analysis uses data on transitions rather than measures of democracy derived from the Polity index because the latter, while a useful measure of institutionalization, does not isolate fundamental shifts in the group of elite who have access to power.²⁰ For example, many measures derived from the Polity scale mark instances of institutional liberalization within the tenure of dictatorship, such as Mobutu's National Sovereign Conference in 1992 and the 1997 election of Khatami in Iran, as episodes of democratization where a transition between groups of elite did not occur. Further, upward movement along the Polity scale may conflate a transition between separate authoritarian regimes with democratization. For example, the 1979 Iranian Revolution, which was a transition between two dictatorships, is marked by a 10-point increase in the combined Polity scale.

The logic of democratization discussed above begins with the premise that promises of distribution are not credible unless group other than the ruling elite has access to power (Acemoglu and Robinson, 2006). Credibility in this case is not established by the current elite remaining in power with additional institutional constraints on the executive (a la North-Weingast), but with a shift in the group of citizens with access to power. This implies that we should use a measure of democracy or regime failure that captures fundamental changes in the ruling elite. This logic would thus rule out institutional liberalizations within the tenure of an authoritarian regime.

Key independent variables

The foreign aid variable is aid per capita compiled by the OECD DAC and obtained from the World Development Indicators (WDI).²¹ This measure captures loans and grants from multi-lateral and bilateral donors, but it does not include military aid. The analysis uses aid per capita instead of aid as a share of economic output so this measure does not capture economic shocks. Much of the literature on foreign aid and regime or leader survival denominates aid as share of economic output: $\frac{Aid}{GDP}$ (Knack, 2004; Djankov, Montalvo and Reynal-Querol, 2008; Kono and Montinola, 2009; Bueno de Mesquita and Smith, 2010). Measuring aid as a share of economic output means that the aid variable contains information on economic shocks. This is potentially problematic because economic growth in general and negative economic shocks in particular may cause authoritarian

²⁰Appendix A discusses the choice of dependent variable in detail.

²¹Accessed July 17, 2010.

breakdown and democratization (Haggard and Kaufman, 1995; Gasiorowski, 1995; Geddes, 1999).

If aid levels remain the same for a particular recipient country, but the economy experiences a negative shock and is thus more likely to democratize, $\frac{Aid}{GDP}$ increases, which might mean $\frac{Aid}{GDP}$ will be positively correlated with democratization for reasons that have nothing to do with aid. This denominator issue, raised in the context of the literature on foreign aid and economic growth,²² suggests that $\frac{Aid}{GDP}$ might be spuriously correlated with political change. The main foreign aid measure is aid per capita in constant (2000) dollars. I then calculate the moving average over the previous two years and log this variable: $\ln(1 + ((Aid_{t-1} + Aid_{t-2})/2))$ where all negative values of aid (net repayment of loans) are set to zero.²³

Data on oil rents are from Ross (2008), lagged one year. It measures a country's total rents from oil and gas per capita in constant (2000) dollars, and captures oil and natural gas production, subtracting country-specific extraction costs, including capital. Because the distribution of this variable is highly skewed, I use the natural log: $\ln(1 + Oil_{t-1})$.

To measure economic crisis, I use a variable calculated from economic growth. I use growth rather than financial crisis or currency crisis because I want a measure that is broadly applicable to the 'poor,' however circumscribed. To provide the widest possible coverage of this variable for authoritarian regimes, I use Maddison's (2006) data on GDP per capita.²⁴ To construct the crisis variable, I first take the two-year lagged moving average of economic growth. The lag helps alleviate concerns that political change is driving the measure of crisis (reverse causation); the two-year moving average helps smooth the data and ensures that the crisis variable is not simply picking up regression to the mean dynamics (Gasiorowski, 1995). I then multiply this lagged moving average by negative one and recode all observations of positive growth as zero. Truncating the distribution of *Crisis* at zero isolates the effect of negative growth – or economic crises – allowing all observations of positive growth to take the same value (zero). This ensures that the results are not driven by episodes of strong economic growth, which may increase the longevity of authoritarian rule and thus decrease the likelihood of democratization.

In all specifications, I also include a variable for economic *Boom*. This is the converse of *Crisis*, measuring the lagged two-year moving average of positive growth instead of negative growth. Finally, because *Crisis* is growth multiplied by negative one, the expected direction of the estimated coefficient is positive: crisis (and more severe crisis) should increase the likelihood of democratization. I also test the robustness of the main result with the natural log of *Crisis* to ensure that severe crises are not driving the result.

In non-democracies economic crises are not disproportionately observed in highly aid dependent countries, though aid dependent countries are slightly more likely to have negative growth. Dividing the sample at the median aid level, I find that 34% of observations for high aid countries had negative

²²See Roodman (2008) and Bearce and Tirone (2010).

²³Setting (small) negative values to zero does not change the main results. This affects less than two percent of observations.

²⁴Appendix E, Table 1 columns 7 and 8, shows that the main findings are robust to a crisis variable constructed with data from the WDI.

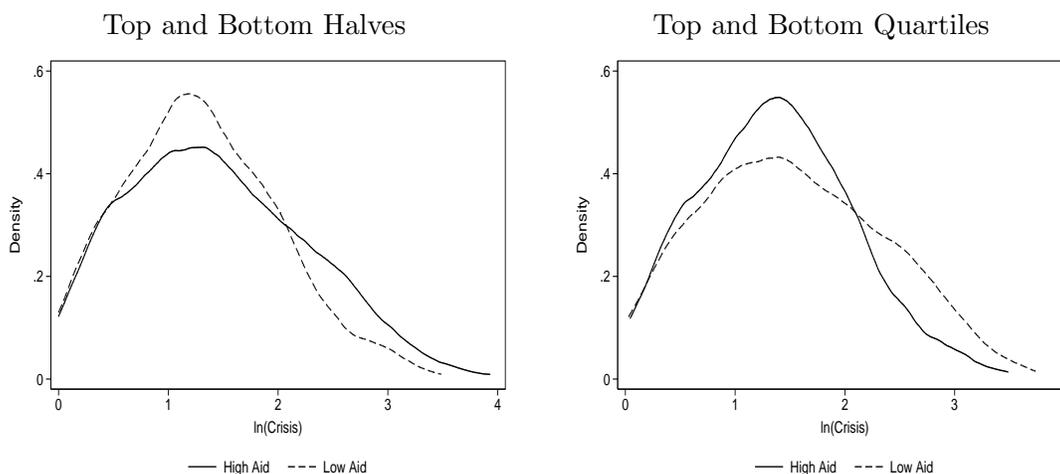


Figure 3: *Distribution of Economic Crisis by Aid Levels*. Displays only observations where growth is negative (*Crisis* not equal to zero).

growth and 19% with growth less than -2%.²⁵ In the bottom half of the aid distribution, these tallies are 31% and 19% respectively, suggesting that negative growth is just as likely to occur in aid dependent non-democracies and those with less aid. Figure 1 graphs the distribution of $\ln(\text{Crisis})$ for observations where growth is negative for these split samples to see if the severity of crises differs by aid dependency. The left panel splits the sample at the median; the distribution of crisis severity is similar in both halves of the sample, though there are slightly more severe crises in high aid countries. The right panel looks only at the lowest and highest quartiles of the aid variable; the distribution of crises is slightly different here, with the density for low aid slightly larger on right end of the $\ln(\text{Crisis})$ scale. This suggests that the most severe economic crises are slightly more likely to occur in countries that are less aid dependent. At least at this level of aggregation, however, there is little to suggest that economic crises are much more likely or severe in highly aid dependent countries.

Further measures of aid and oil dependency

While the continuous measures of lagged aid and oil have been directly purged of information on economic shocks by denominating them by population rather than size of the economy, the variation in these continuous measures may still be influenced by economic shocks. For example, a decline in oil prices may cause both a dip in oil rents per capita as well as a short-term negative economic shock, which would be captured by the measure of economic crisis. To address this issue, I code binary variables for aid and oil dependence. For aid, this variable is coded 1 when the lagged moving average of (logged) aid per capita over $t - 3, t - 4, t - 5, t - 6$ exceeds 3.27 (which is slightly lower than \$30 per capita). While necessarily arbitrary, this cut-point marks just under half the sample (47%) as aid dependent. This measure captures the concept of aid dependency without relying on

²⁵Recall that aid is standardized by population and not by the size of the economy.

variation in aid during the period when I measure economic crisis (negative economic growth in $t - 1, t - 2$). Below, I show how the results change when we vary the cut point for demarcating *High* and *Low* aid countries. In general, the precise location of the cut point matters little.

For oil dependency, the binary variable is coded 1 when rents per capita exceeds \$10 in one of the past ten years (39% of the sample). Below, I show that the results for binary oil dependency do not differ substantially when this threshold varies between \$1 and \$200 per capita. Again, this binary measure of natural resource dependence captures a feature of the domestic economy that should not fluctuate with short-term economic crises. Further, both of these binary measures use information with deep lags which helps address concerns that aid and oil dependency are caused by political change in the observation year.

Control variables

For the baseline specification, I employ the following control variables: $\ln(GDPpc)$, *Boom*, *Duration* time polynomials, and calendar *Time* polynomials. A long literature addresses the relationship between level of development and democracy, so I include $\ln(GDPpc)$ from Maddison's data. I treat this measure as exogenous, though I cannot make any causal statements about the relationship between the development and democracy. I include calendar time polynomials to control for global trends in democracy that may also be correlated with foreign aid or oil shocks. For example, the end of the Cold War saw a surge in democracy and a marked decrease in foreign aid to many countries. I include three polynomials to capture time trends that are more complex than a linear trend or a Cold War dummy variable. Due to separation issues, I cannot include year dummies without losing observations from the sample.

To investigate the robustness of the model specification, Appendix E (Tables 1 and 2) reports specifications with additional control variables intended to capture unit heterogeneity: *Neighbor democracy*, *Region* dummies, and *Regime type* dummies. Neighbor democracy measures diffusion effects and foreign pressure for democracy, both of which should increase the likelihood of a democratic transition (Gleditsch and Ward, 2006) and may also be correlated with foreign aid. To construct this variable, I calculate the share of countries that are democratic (binary, as coded by CGV) among all countries with capital cities within 4000km of the target country's capital. With the Geddes data, I include dummy variables for military regime, party regime, and monarchies where personalist regime is the omitted category. With the CGV data, I include dummy variables for military regime and monarchy with civilian regime as the omitted category.

Method

I employ a binary time series, cross-section probit model with controls for time dependence: *duration*, *duration*², and *duration*³ (Carter and Signorino, 2010). I checked for non-proportional hazards in the data by including interactions between the duration polynomials and the main

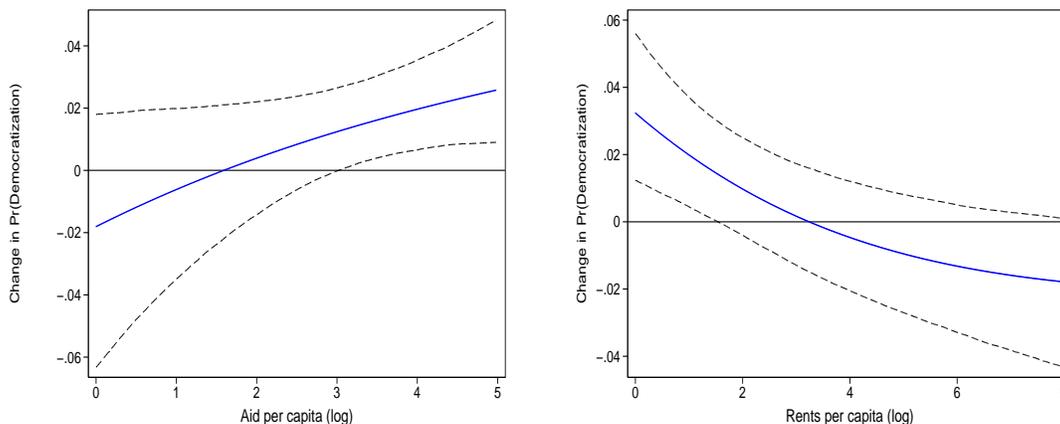


Figure 4: *Marginal effect of Crisis on likelihood of democratization, by Aid and Oil. Simulations based on models 2 and 6, Table 1.*

independent variables; likelihood-ratio tests indicate that these hazards are proportional.²⁶ The main specification is a probit with standard errors clustered on country. To address possible unit heterogeneity, I also report results that include random effects (RE).

Results

This section discusses the main results by examining the substantive effect of economic crisis on the likelihood of democratization. I concentrate on results from the base models which use the measure of democratization from Geddes, Wright and Frantz (2012). The full model results are reported in columns 2 and 6 in Table 1.

Figure 4 shows how the effect of *Crisis* on the likelihood of democratization varies by levels of aid and oil dependency. I examine the marginal effect of a change in the *Crisis* variable from no crisis ($Crisis=0$ and $Boom = 3\%$) to an economic growth crisis of -4% ($Boom=0$), which is approximately the mean of *Crisis* above zero. I set all other variables (duration polynomials and level of development) at their respective means, except calendar time which is held constant at 1994. The left graph in Figure 4 shows that as aid dependency increases the marginal effect of *Crisis* increases. At low levels of aid, *Crisis* has no discernable effect on the likelihood of democratization, but turns positive and statistically significant once aid levels surpass about \$20 per capita (3.0 on the log scale). This value of aid is at the 62nd percentile of the with-in sample distribution of aid, suggesting that economic crisis increases the likelihood democratization in roughly the top third of aid recipients. At the median aid level, crisis increases the likelihood of democratization by 1.4%, with this figure rising to 2.5% at very high levels of aid (90th percentile).

²⁶An LR test indicates that interacting *Crisis* with 3 duration polynomials is not necessary in the base model specification. An LR test does suggest that the interaction between *Crisis* and *Duration* (no polynomial transformations) significantly reduces the likelihood ratio. Including this latter interaction term strengthens the main finding for the interaction between *Aid* and *Crisis*. In the spirit of parsimony, however, I proceed without the interaction between *Duration* and *Crisis*.

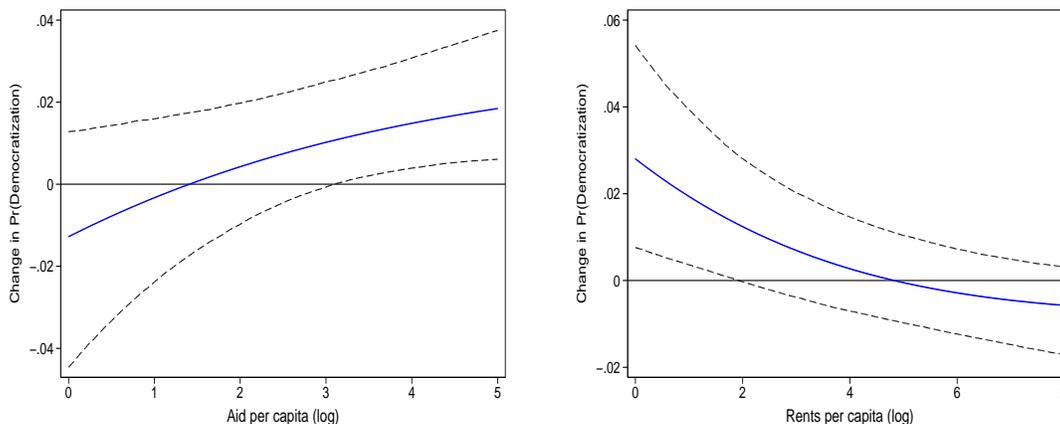


Figure 5: *Aid, Oil, Crisis, and Democratization*. Cheibub, Gandhi, and Vreeland (2010) binary measure of democratization. Simulations based on models 1 and 2, Table 2.

The right panel in Figure 4 shows that the marginal effect of *Crisis* is only positive when there is little or no revenue from oil and gas rents. The lower-bound of the confidence interval crosses zero at 1.5 on the log scale for oil or about \$4 per capita. Just over 61 percent of authoritarian regimes in the sample fall below this threshold, suggesting that economic crisis increases the likelihood of democratization in the majority of regimes that have little or no oil, but has little effect on the prospects of a democratic transition in oil rich regimes. With no rents, *Crisis* increases the likelihood by 3.2%.

[Table 1 About Here]

Overall, democratization is a low-likelihood event, occurring in roughly 2.2% of observed country-years. These simulations suggest that at high aid levels (75th percentile), economic crisis is associated with a 1.9% increase in the likelihood of democratization. Economic crisis in autocracies with little or no natural resource rents are associated with an increased likelihood of democratization of just over 3.2%. The other models in Table 1 show that these results are robust to the inclusion of random effects and using the logged value of *Crisis*. This latter transformation substantially reduces the skewed distribution of economic crises, which should give us confidence that the main findings are not driven but a handful of cases that experienced severe economic crises in the 1980s. Appendix E (Tables 1 and 2) reports models that include controls for region and regime type, with similar results.

Table 2 reports a number of robustness tests. First, I reran the models using the CGV data (Table 2, columns 1 and 2). Figure 5 shows the substantive result, depicting a similar pattern to that in Figure 4: economic crisis increases the likelihood of democratization at high levels of aid and low levels of oil dependency. Second, I constructed longer lags for the (moving average) aid and oil rents variables – covering $t - 3, t - 4, t - 5, t - 6$ instead of the years immediately preceding the observation of the dependent variable. These results, reported in Table 2 columns 3 and 5, are similar to the base models.

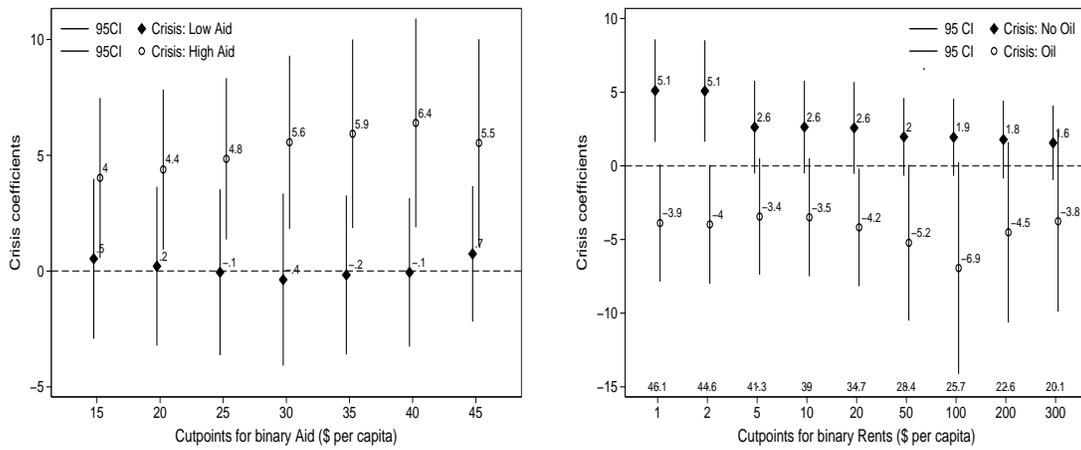


Figure 6: **Binary measures of aid and oil dependency.** Coefficients for *Crisis* at high and low levels of binary aid and oil for various cut-points marking binary aid and oil dependency. Based on models 4 and 6, Table 2, with various cut-points.

[Table 2 About Here]

Third, I tested the models using binary indicators of aid and oil dependency as described above. These results are reported in Table 2, columns 4 and 6. Because the cut-points for the binary indicators are necessarily arbitrary, I tested the model across various thresholds for aid and oil. The coefficients for *Crisis* at high and low aid and oil levels are shown in Figure 6. The threshold for dividing the sample in high and low aid categories varies between \$15 per capita (34th percentile of the aid distribution) and \$45 per capita (73rd percentile of the aid distribution). At all these cut points, the coefficient for *Crisis* at high aid levels is positive and statistically different from zero as well as statistically different from the coefficient for *Crisis* at low aid levels. Thus using a binary measure aid dependency across a broad range of thresholds indicates that economic crisis is correlated with democratization at high but not low levels of aid.

In the right panel of Figure 6, I pursue a similar exercise for oil rents. The threshold for oil dependency varies from 1\$ per capita to \$300 per capita. The numbers that appear above these threshold markers are the share of the sample that falls above that threshold and is thus categorized as *Oil* instead of *No Oil*. The coefficients for *Crisis* at *No Oil* are positive and marginally significant at all threshold levels up to \$300, while the *Crisis* coefficient at *Oil* is negative. Again, using this binary indicator based on a deep lag for oil dependency indicates that economic crisis increases the likelihood of democratization in authoritarian regimes with little or no oil.

Exploring the foreign aid finding

The argument concerning foreign aid conditionality suggests that economic crisis in aid dependent countries should be more likely to precipitate democratization when conditionality is enforceable. One approach to analyzing conditionality, albeit blunt, is to examine the data for separate time periods under the assumption that on average foreign conditionality by Western donors was more

enforceable in the post-Cold War period than in prior years (Dunning, 2004; Bermeo, 2009; Wright, 2009; Bermeo, 2011). Further, during the Cold War period some Western foreign aid from large donors such as the U.S. was intended to keep some dictators in power. If the mechanism that links aid to democratization in this period runs through conditionality (Crawford, 2001; Goldsmith, 2001; Dunning, 2004; Wright, 2009), then the conditioning effect of *Aid* on the relationship between *Crisis* and democracy should only appear in the latter period. Thus I checked the main results for *Aid* and *Crisis* by time period using both the Geddes and CGV data, marking the Cold War period as ending in 1989. The results are reported in the first four columns of Table 3. While the coefficients for the interaction between *Aid* and *Crisis* are positive in both periods, they are considerably larger and statistically significant only in the post-Cold War period. This finding is consistent with the mechanism of conditionality *if we assume that it was better enforced in the latter period*.

[Table 3 About Here]

A second approach is to consider the type of aid. Some have argued that multilateral agencies may be better placed to enforce economic conditionality than bilateral donors because the former are more isolated from geo-strategic concerns (Girod, 2008; Powell and Bobba, 2006). Thus we might expect multilateral but not bilateral aid to influence the spending behavior of recipients during times of crisis. Others, however, have noted that there much less consensus about the extent to which some multilateral institutions enforce conditionality (Vreeland, 2006).

The nature of grants and loans may also be a factor. For example, loans may generate fiscal discipline because they typically need to be paid back but grants entail no such expectation (Brautigam, 2000). Indeed Gupta et al. (2004) find evidence that loans increase revenues but grants decrease them. This study, however, groups together data from 1990s with data from the prior two decades. If this logic is correct, we should expect the effect of aid on fiscal retrenchment during economic crisis to be stronger for loans.

The results in the last four columns of Table 3 test the base model with four different categories of aid: grants, loans, multi-, and bi-lateral. The interaction coefficients in all four models are all positive and roughly the same size, though the interaction in the multi-lateral equation is not statistically different from zero. These tests suggest that there is little difference in the type of aid that conditions the effect of economic crisis. These models are robust to the inclusion of random effects, region dummies, and the complement aid type as a control. Breaking down aid types further (Appendix E, Table 5), I find that bi-lateral grants and both multi-lateral grants and loans have a similar conditioning effect on economic crisis. The only category of aid that has no effect is bi-lateral loans – which is also the most weakly correlated with other types and the smallest of the four types of aid. Thus, there is little evidence to suggest that just one type of aid is driving the main result.

A couple of factors may explain why. First, donor countries can influence the behavior of multilateral institutions which make it difficult for multilateral institutions to behave independently vis-à-vis conditionality. Numerous empirical studies show that the preferences of member states

influence the lending decisions of multilateral institutions (Kilby, 2006; Fleck and Kilby, 2006; Lyne and Tierney, 2009). Perhaps more importantly, the lines between an IFI and large bilateral donors may be blurred from the perspective of the recipient country. For example, Handley (2008, 43) reports that in conversations with government officials in Ghana, sources would often confuse the role of the IMF, the World Bank, and the U.S. “This tendency,” she notes, “is itself is telling: pressure would likely have been principally from the U.S. and the World Bank, but it could well have been experienced as coming from all of them... almost indistinguishably.”²⁷

Second, the variables measuring different types of aid are relatively co-linear. The majority of authoritarian aid recipients face multiple donors and simultaneously receive different types of aid. Although aid grants are more common than loans in non-democracies, and the former’s share of all aid to dictatorships has been decreasing since a peak in the early 1980s, until very recently the majority of authoritarian recipients receive both.²⁸

Government spending

This section examines whether aid and oil rents condition the effect of economic crisis on government spending. If, as suggested by previous analysis, oil ameliorates the democratizing effect of economic crisis, one pathway may be through compensating citizens by increasing government spending.

Previous research has looked at how foreign aid and oil revenue affect government spending, though it has rarely focused specifically on authoritarian regimes. Remmer (2004), for example, examines a sample that includes both democracies and dictatorships, but the majority of observations are from democracies simply because much of the data on government finances in non-democracies is missing. Jensen and Wantchekon (2004) show that in Africa, resource dependence is correlated with government consumption. As we will see below, the empirical results represented here confirm that oil is associated with a long-term increase in government spending. However, the argument in the present paper concerns how authoritarian regimes respond to economic crisis in the short-term. Can these regimes use government spending to stem the tide of collective action that may potentially destabilize the regime when faced with an economic crisis?

The analysis uses an error correction model to test how economic crisis influences short-term spending and to see if aid and oil condition this relationship. The reported results use the Geddes sample of authoritarian regimes with non-missing data on government spending. The spending data are taken from the Penn World Tables (PWT), which reports them as a share of GDP.²⁹ Because I do not want to capture variation in GDP in the measure of spending, I transform this variable into constant dollar values. I then log the constant dollar total.

Because the analysis uses PWT on government consumption, I also calculate *Crisis/Boom*

²⁷See also Rakner (2003, 38) with respect to aid donors in Zambia: “[B]y 1992, the multinational finance institutions, led by the World Bank through its co-ordinating functions, again came to occupy a leading role within an increasingly homogenous aid regime.”

²⁸Descriptive graphs available from the author upon request.

²⁹In the Appendix (D-1.5, D-1.6, D-2.8), I report results from using the WDI measure of government spending, similarly transformed to log constant dollar value.

from this series. Instead of reporting different variations of the *Crisis* variable, I only report the results for $\ln(Crisis)$. I also include country- and three calendar year time trend polynomials. The country fixed effects should capture much of the variation for variables that move slowly over time such as population, culture, and religion. These country-specific intercepts should also allay concerns that the constant dollar totals for spending are not dominated by size of the country. To begin, I test the following model using OLS, with no interaction terms:

$$\begin{aligned} \Delta GovtSpend = & \beta_0 + \beta_1 GovtSpend_{t-1} + \beta_2 \Delta Crisis + \beta_3 Crisis_{t-1} + \beta_4 \Delta Aid + \beta_5 Aid_{t-1} + \\ & + CalendarTime_p + \beta_i \vartheta_i + \beta_t \psi_t + \varepsilon \end{aligned}$$

where ϑ_i are country effects and ψ_t are calendar year polynomials. The main theoretical implication is estimated by the difference variables, which capture the short-term, one-off effects of the covariates (De Boef and Keele, 2008). Next, I test the same model, but substitute *Oil* for *Aid*. After testing models without interaction terms, I include them.

The results are reported in the top panel of Table 4. The analysis excludes years where oil shocks are likely to be present (1973-74 and 1979-80), but results reported in the Appendix (D-1) show that excluding these years from the analysis does not substantially change the results. Economic crises are associated with a steep decline in short-term government spending: the differenced *Crisis* coefficient in column 1 is large, negative, and statistically significant when no interaction is included. This evidence is consistent with Wibbels' (2006) argument that economic shocks in developing countries induce pro-cyclical fiscal policy (Talvi and Végh, 2005). Second, there is evidence to suggest that the long-run effect of oil is to increase government spending, results that are consistent with Jensen and Wantchekon (2004) and Ross (2009). The long-term effect of aid (not reported), however, is negative – the opposite what Remmer (2004) finds.³⁰

[Table 4 About Here]

The left panel of Figure 7 plots the substantive effect of an economic crisis, set at -4.5% growth (or 1.5 on the log crisis scale) which is roughly the median crisis above zero. For aid, *High* and *Low* values are one standard deviation above and below the mean aid value. For oil, *Oil* is the mean above zero and *No Oil* is zero. The typical crisis is associated with a 4.0% decline in government size in countries with low aid levels; this figure drops to -6.7% at high aid levels. This suggests that the observed reduction in government spending is over 50% larger in highly aid dependent countries. In separate regressions by time period, the results indicate that this jump in spending cuts is concentrated in the post-1989 period. The decline in government size is 3.4% in low aid countries, but 8.8% in high aid countries during this period.

The results for oil suggest the opposite effect. The decline in government spending associated with an economic crisis is nearly twice as large in countries with oil rents than in those which lack

³⁰Three possibilities may account for this. First, this estimate uses spending data from the Penn World Tables, though I find a positive (but not significant) coefficient for similar tests using World Bank data. Second, the dependent variable is not dominated by population or the size of the economy, but is the logged constant dollar value. Third, the sample used here is nearly twice as large as that in Remmer (2004) and includes only non-democracies.

them. The effect of crisis on spending in countries with oil rents is calculated using the median level of oil rents above zero in the sample, roughly the same level of rents observed in countries such as Mexico, Nigeria, or Syria in the 1990s. The effect of oil wealth on spending cuts during crisis – at least as estimated by an OLS regression – would be even larger in countries that have higher levels of oil rents, such as Kuwait, Saudi Arabia, and Oman.

On average, authoritarian regimes cut spending when faced with a negative economic shock. However, these findings suggest that oil may help regimes cushion the effect of this negative shock, while aid exacerbates the crisis effect – particularly in the post-1989 period. If oil helps deter democratization by allowing authoritarian regimes to distribute spending to those who would otherwise fight for democracy, these results suggest that oil blunts the short-term decrease in spending during periods of economic crisis. This interpretation of the spending results complements the expectations of the spending version of the rentier thesis: oil hurts democracy by negating the democratizing effect of economic crisis and does so by propping up spending. The finding for foreign aid and spending is just the opposite. It suggests that aid dependent countries cut spending more deeply during economic crises. If these deep spending cuts hurt poor citizens, the cuts may increase their incentive to press for democracy. If government spending is redistributive, this finding is also consistent with research which suggests that IMF agreements – a form of aid dependency – hurt the poor by redistributing income upwards (Vreeland, 2003). Further, if cutting government spending is the result of policy measures intended to increase economic freedom, as measured by the Fraser Institute Index, the finding that foreign aid exacerbates spending cuts during times of crisis is also consistent with Goldsmith’s (2001) evidence that aid is correlated with economic freedom in sub-Saharan Africa as well as Bearce and Tirone’s (2010) finding that foreign aid in the post-1989 period is positively correlated with economic reform.

Calorie consumption

While the findings for spending are consistent with the main hypotheses, government size may not be the best measure of how autocratic regimes responds to democratizing pressure, for two reasons. First, as Acemoglu and Robinson (2001) emphasize, one-time redistribution may not credibly keep the poor from collectively organizing for democracy. Because opportunities for successful collective action are fleeting, democratizing agents take advantage of these chances to seek *institutional* guarantees of future redistribution – which Acemoglu and Robinson (2001) interpret as democracy. Short-term changes in spending may not suffice to placate the poor. If, however, spending stems the tide of collective action in the first place, it may still serve the purpose of curtailing prodemocratic pressure resulting from economic crises. That is, if spending can reduce the economic loss that spurs collective action, it is not necessarily a reaction to collective action but a reaction to crisis which has the potential to breed collective action. Theoretically, whether spending occurs to pay off the poor before or after collective action is simply a choice made in setting up the timing of actions in a theoretical model.

A second objection to thinking about government spending as a tool for autocrats to appease

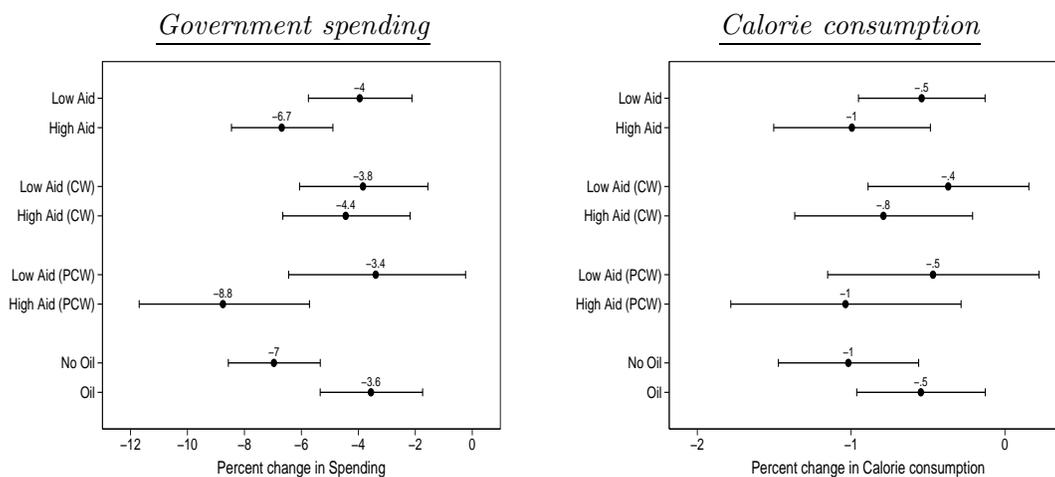


Figure 7: **Effect of Economic Crisis on Government spending and Calorie consumption.** Linear combination of $(\ln(\text{Crisis}) + \ln(\text{Crisis}) * \text{Aid})$ and $(\ln(\text{Crisis}) + \ln(\text{Crisis}) * \text{Oil})$ for high and low levels of aid and oil. Low and High are one standard deviation below and above the mean value for aid. Low oil set to zero; high oil set to the median above zero. $\ln(\text{Crisis})$ is $\log(1 + (100 * \text{Crisis}))$.

the poor in times of economic crisis is that total spending may not be a good measure of pro-poor policy. For example, Ross (2006) points out that much of social spending in the health sector may benefit the upper and middle class more than the poor. Thus spending may in fact be a better measure of transfers to the middle class. Further, publicly available spending data may obscure real spending priorities in dictatorships.

To address these concerns I examine how economic crisis and non-tax resources influence calorie consumption in authoritarian regimes. I use calorie consumption as a proxy for pro-poor government spending and/or policies in the time of economic crisis (Blaydes and Kayser, forthcoming). This variable addresses both concerns raised above because: (1) it measures a factor that is more likely to fluctuate for the poor than for other social classes; and (2) calorie consumption is a direct measure of personal utility that may deter collective action and thus may constitute a response to crisis even before collective action arises. If oil revenue props up government spending during times of crisis, while aid dependency exacerbates spending cuts, and if government spending translates into at least some pro-poor spending, then these factors should also affect calorie consumption. First, economic crises should reduce calorie consumption. Second, oil wealth should attenuate this negative correlation; and aid dependency should entail sharper decreases in calorie consumption during periods of economic crisis.

The measure of calories is the daily average calorie intake, from the Food and Agricultural Organization of the United Nations (FAO). Data is available for over 100 non-democratic countries from 1961-2005. I follow a similar empirical strategy as outlined above for spending: an error-correction model with country- and calendar year time trends. The distribution of calorie consumption is not highly skewed, but I still use the logged measure to ensure that outliers are not driving the

analysis.³¹

The lower panel of Table 4 reports the results and the right panel of Figure 7 graphs the substantive effects for calorie consumption. Economic crises are associated with a larger decline in calories in aid dependent countries (1.0%) than in countries that receive less aid (0.5%). The analysis that pools all years may obscure differences among periods, so I again analyze separate models for the pre-1990 and post-1989 periods. The steeper calorie losses among more aid dependent countries during crises is concentrated in the post-1989 period. Finally, the results for oil dependency are also consistent with the earlier findings: calorie losses in regimes with no oil are larger (1.0%) than in countries with an average level of oil rents (0.5%), though again these percent changes are relatively small.

Discussion

This paper advances the literature on non-tax revenue and democracy in two ways. First, the analysis builds on theories of democratization and state finance to focus the empirical tests on periods of economic crisis. The theory of democratization laid out in Acemoglu and Robinson (2001, 2006) begins with the assumption that citizens have overcome collective action constraints and can credibly threaten revolution against authoritarian rulers. The empirical implications that stem from this theory arise during periods when this assumption holds. This paper argues that times of economic crisis are likely to be such occasions when citizens can overcome collective action problems and threaten the regime. This contention has implications for how we test theories that link aid and oil to democratization. If non-tax revenue can forestall or deter revolutionary threats, the evidence for this story should not necessarily be found in the average effect of non-tax resources on authoritarian stability across all time periods, but should be concentrated in periods when the revolutionary threat is binding.

Second, the analysis in this paper covers a broader sample of non-democratic countries than used in previous studies. To do this, I have gathered data on variables with relatively few missing observations, such as total government spending and calorie consumption. The empirical analysis also moves away from measures of regime stability and democracy that employ off-the-shelf data, such as the Polity scale, to a measure that better captures ‘democratization’— at least when defined as a change in the group of elites who have access to power. By not using the Polity scale, though, the analysis departs from concepts such as institutional checks on the power of the executive, which may be relevant for some theories (e.g. the North-Weingast thesis or veto player theories).

The findings in this paper have implications for the further study of oil and aid. First, this analysis employs a blunt measure of aid conditionality by simply dividing the sample in two time periods. Not only are the results for foreign aid, crisis, and democratization stronger in the post-1989 period, but in unreported results I find that the main result for aid goes away once data from

³¹The reported results exclude years of potential oil shocks (1973-79 and 1979-80), though I show that this decision does not affect the main finding. See the Appendix (D-1) for these robustness tests. All reports analysis of calories also excludes Kuwait from 1990-1994 due the presence of large outliers associated with the Iraqi invasion.

the period 1990-2000 are excluded. This suggests that there may have been a small window of time in which dictators believed aid conditionality would be enforced and during which the fiscal options of aid dependent countries were constrained by donor preferences for a smaller state sector. This does not necessarily mean that aid disbursements at multilateral institutions were uninfluenced by major bilateral donors (the U.S. in particular) during this period. Rather, the contention of this paper is that this decade may have been a brief period when dictators believed that the threat to withdraw of Western aid was credible.³²

Future research will benefit from more closely measuring the degree of conditionality to better test this story. I only caution that such measures should be recipient-centric rather than donor-centric because aid recipients typically encounter multiple donors (both multi- and bi-lateral) and more importantly, the story of conditionality hinges on the beliefs of recipients and not necessarily on the systematic behavior of donors. Indeed, the latter may not be observed if a donor threat is believed to be credible. Finally, it is important to note that when I exclude this same period (1990-200) from the oil models, the main results remain. Again, this indicates something unique about foreign aid during the 1990s that makes it a distinct type of non-tax resource – one that suggests conditionality may be at work.

Second, the findings for oil suggest that the spending mechanism articulated in the rentier thesis may be a latent capability to manipulate state spending that becomes operative and consequential during periods of economic crisis – a rentier effect that occurs irrespective of oil prices. Because both the rentier state as a latent capacity (the stockpile argument) and the durable coalitions hypotheses have the same implications for how oil rents affect democracy, this paper suggests that one way to distinguish these is to investigate the effect of oil on government spending. The rentier story implies that oil rich autocracies should not cut spending significantly during economic crises, while the durable coalition hypothesis suggests no effect because institutions substitute for spending during crises. That said, the analysis does not directly test the durable coalition hypothesis by examining whether oil conditions the effect of crisis in regimes that consolidated power before the onset of oil rents. Future research should address this issue.

The results in this paper suggest that aid and oil are not the same, influencing the prospects of democratization in different ways. In particular, during economic crises aid dependent countries experience deeper spending cuts and greater loss of consumed calories. Oil revenue, on the other hand, helps prop up government spending and softens the calorie shock due to economic hard times. One interpretation of these results for democracy is that oil hurts democracy but aid does not. This interpretation, however, does not necessarily mean that aid is good and oil is bad. If the theoretical expectations regarding spending are correct, then aid might enhance the prospects of democracy precisely by hurting citizens, at least in the short-term. The opposite would be true of oil wealth; it hurts democracy by helping citizens.

³²See for example, Hyde and Boulding (2008) .

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Table 1: Aid, Oil, and Democratization

	<u>RE</u>		<u>ln(Crisis)</u>		<u>RE</u>		<u>ln(Crisis)</u>	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Crisis	0.712 (1.16)	-6.446+ (3.47)	-4.477 (4.22)	-0.282+ (0.17)	0.808 (1.13)	3.681* (1.54)	4.417** (1.65)	0.165* (0.08)
Crisis*Aid		2.338* (1.02)	2.143+ (1.21)	0.110* (0.05)				
Crisis*Oil						-1.934** (0.73)	-1.963* (0.88)	-0.057** (0.02)
Aid	-0.042 (0.05)	-0.096+ (0.05)	-0.096 (0.07)	-0.114* (0.06)				
Oil					-0.069** (0.02)	-0.037 (0.03)	-0.052 (0.03)	-0.036 (0.03)
Log GDP pc	0.004 (0.07)	0.009 (0.07)	0.002 (0.10)	0.014 (0.08)	0.175* (0.08)	0.195* (0.08)	0.223* (0.09)	0.198* (0.08)
Boom	-3.531+ (1.93)	-3.863* (1.93)	-4.236 (2.86)	-3.529+ (1.94)	-3.283+ (1.92)	-3.718+ (1.94)	-4.045 (2.48)	-3.014 (1.95)
Duration	-0.028** (0.01)	-0.028** (0.01)	-0.017 (0.01)	-0.028** (0.01)	-0.024** (0.01)	-0.024** (0.01)	-0.019* (0.01)	-0.024** (0.01)
Duration ²	0.000* (0.00)	0.000* (0.00)	0.000 (0.00)	0.000* (0.00)	0.000** (0.00)	0.000** (0.00)	0.000 (0.00)	0.000** (0.00)
Constant	-1.706** (0.56)	-1.610** (0.57)	-1.782* (0.83)	-1.589** (0.57)	-3.060** (0.61)	-3.209** (0.61)	-3.567** (0.70)	-3.264** (0.62)
Log likelihood	-320.9	-318.2	-312.3	-318.3	-356.0	-351.5	-348.7	-352.9

+ p<0.10; * p<0.05; ** p<0.01; Probit with errors clustered on country. $Crisis = (Growth_{t-1,t-2} * -1)$, where all negative values (meaning positive economic growth) are set to zero. Aid is the moving average of the natural log of aid per capita over the last two calendar years. Oil is the natural log of lagged oil and gas rents per capita. Calendar time polynomials (3) included in all models, but are not reported. Aid sample: 1962-2008; NxT≡3219; N≡102; regimes≡218. Oil sample: 1961-2006; NxT≡3588; N≡110; regimes≡232.

Table 2: Robustness Tests

Democracy variable	CGV (1-2)		Geddes (3-6)			
	(1)	(2)	(3)	(4)	(5)	(6)
Aid/Oil			Lag	Binary	Lag	Binary
Crisis	-5.803*	2.826+	-4.527	-0.055	3.959*	2.573
	(2.41)	(1.49)	(3.08)	(1.83)	(1.77)	(1.60)
Crisis*Aid	2.099**		2.158*	4.933*		
	(0.71)		(0.90)	(2.43)		
Crisis*Oil		-1.317**			-1.102*	-6.117*
		(0.48)			(0.49)	(2.51)
Aid	-0.108*		-0.074	-0.183		
	(0.05)		(0.05)	(0.13)		
Oil		-0.050+			-0.040	0.040
		(0.03)			(0.03)	(0.13)
Log GDP pc	0.028	0.244**	0.030	0.049	0.193*	0.091
	(0.07)	(0.08)	(0.08)	(0.08)	(0.09)	(0.07)
Boom	-4.079+	-5.180*	-3.716+	-3.623+	-4.653*	-3.420+
	(2.20)	(2.33)	(2.12)	(2.13)	(2.33)	(1.84)
Duration	-0.050+	-0.054*	-0.030**	-0.031**	-0.024**	-0.023**
	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Duration ²	0.002	0.002	0.000*	0.000*	0.000*	0.000*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	-2.105**	-3.699**	-2.095**	-2.307**	-3.244**	-2.525**
	(0.49)	(0.58)	(0.64)	(0.72)	(0.79)	(0.55)
Log likelihood	-326.6	-342.2	-298.4	-290.9	-309.3	-361.7
NxT	3531	3841	3096	2943	2975	3620
N	109	115	101	101	108	110
T	1962-2008	1961-2006	1964-2008	1966-2008	1966-2008	1961-2008
Regimes			205	198	197	228

+ p<0.10; * p<0.05; ** p<0.01; Probit with errors clustered on country. Lagged aid is the moving average of the natural log of aid per capita over t-3, t-4. Lagged oil is over t-3, t-4, t-5, t-6. Binary aid divides sample at median level of aid per capita. Binary oil marks whether country had more than \$10 per capita in oil rents in any of the past 10 years. Calendar time polynomials (3) included in all models, but are not reported.

Table 3: Further tests of Aid, Crisis, and Democratization

Democracy variable	<u>Geddes (1-2)</u>		<u>CGV (3-4)</u>		<u>Geddes (5-8)</u>			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Period	PCW	CW	PCW	CW				
Aid					Grants	Loans	Multi-	Bi-
Crisis	-15.151*	-2.949	-12.085*	-2.056	-0.246	-0.116	-0.090	-0.263*
	(7.67)	(3.80)	(5.77)	(2.64)	(0.16)	(0.11)	(0.14)	(0.13)
Crisis*Aid	4.799*	1.164	3.960*	0.976	0.095*	0.087+	0.062	0.109**
	(2.34)	(1.12)	(1.70)	(0.70)	(0.05)	(0.04)	(0.06)	(0.04)
Aid	-0.019	-0.175*	-0.079	-0.111*	-0.113*	-0.090	-0.097+	-0.100+
	(0.07)	(0.07)	(0.07)	(0.05)	(0.06)	(0.06)	(0.06)	(0.05)
Log GDP pc	-0.180	0.195*	-0.143	0.192*	0.016	0.026	0.007	0.028
	(0.12)	(0.10)	(0.10)	(0.08)	(0.08)	(0.07)	(0.08)	(0.08)
Boom	-5.135+	-3.279	-2.416	-5.690+	-4.658*	-4.330+	-4.511*	-4.604*
	(2.88)	(2.16)	(2.43)	(3.01)	(2.31)	(2.24)	(2.24)	(2.29)
Duration	-0.029*	0.033	-0.008	-0.058	-0.023**	-0.023**	-0.023**	-0.023**
	(0.01)	(0.04)	(0.04)	(0.04)	(0.01)	(0.01)	(0.01)	(0.01)
Duration ²	0.000*	-0.002	0.000	0.002	0.000	0.000	0.000	0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	0.014	-3.084**	-0.488	-2.847**	-1.591**	-1.886**	-1.744**	-1.584**
	(0.92)	(0.77)	(0.79)	(0.63)	(0.60)	(0.59)	(0.61)	(0.60)
Log likelihood	-168.7	-135.4	-158.3	-164.0	-305.5	-305.6	-305.9	-304.6
NxT	1191	2028	1366	2165	3113	3115	3093	3081
N	89	88	94	95	100	101	101	101
Years	1990-	1962-	1990-	1962-	1962-	1962-	1962-	1962-
Covered	2008	1989	2008	1989	2008	2008	2008	2008
Regimes	116	173			209	210	210	208

+ p<0.10; * p<0.05; ** p<0.01; Probit with errors clustered on country. $Crisis = (Growth_{t-1,t-2} * -1)$, where all negative values (meaning positive economic growth) are set to zero. $\ln(1 + 100 * Crisis)$ used in models 5-8. Aid is the moving average of the natural log of aid per capita over the last two calendar years. Calendar time polynomials (3) included in models 5-8, but are not reported.

Table 4: Short-term effect of Crisis on Spending and Calorie consumption

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Government spending</i>						
Crisis	-0.0367** (0.005)	-0.0469** (0.006)	-0.0366** (0.005)	-0.0112 (0.011)	-0.0266* (0.013)	0.0079 (0.019)
Crisis*Oil		0.0047** (0.001)				
Crisis*Aid				-0.0081** (0.003)	-0.0018 (0.004)	-0.0160** (0.005)
Oil	-0.0038 (0.008)	-0.0037 (0.008)				
Aid			-0.0053 (0.007)	0.0001 (0.007)	-0.0059 (0.010)	0.0078 (0.012)
R ²	0.18	0.18	0.17	0.17	0.20	0.31
NxT	2517	2517	2397	2397	1417	980
N	98	98	92	92	78	81
T	1962-2006	1962-2006	1962-2007	1962-2007	1962-1989	1990-2007
	(7)	(8)	(9)	(10)	(11)	(12)
<i>Calorie consumption</i>						
Crisis	-0.0054** (0.001)	-0.0066** (0.002)	-0.0050** (0.001)	-0.0010 (0.002)	-0.0007 (0.003)	0.0001 (0.004)
Crisis*Oil		0.0006* (0.000)				
Crisis*Aid				-0.0013+ (0.001)	-0.0012 (0.001)	-0.0016 (0.001)
Oil	0.0007 (0.002)	0.0007 (0.002)				
Aid			0.0023 (0.002)	0.0031+ (0.002)	0.0046* (0.002)	-0.0018 (0.003)
R ²	0.11	0.12	0.11	0.12	0.14	0.30
NxT	2328	2328	2184	2184	1598	842
N	93	93	86	86	73	76
T	1962-2005	1961-2007	1962-2005	1962-2005	1962-1989	1990-2005

+ p<0.10; * p<0.05; ** p<0.01; Dependent variables: $\Delta \ln(GovtSpending)$ and $\Delta \ln(CalorieConsumption)$; OLS. This table only reports coefficients for Δ variables. Country-fixed effects, calendar time trend polynomials (3), lagged variables, constant, and control variables (Conflict Intensity and log Boom) included in all models but not reported. Full results reported in D-1 and D-2. *Crisis* is $\ln((Crisis * 100) + 1)$ in all columns.

Appendix A: Measuring authoritarian stability

Many studies that examine the relationship between non-tax resources – such as foreign aid and oil – and regime stability rely on data derived from the Polity index. For example, Ross’s (2001) original study uses a levels variable derived from the full Polity scale, combining autocracy and democracy scores.³³ Smith (2004) and Morrison (2009) use the Polity *durable* variable to mark regime stability. This variable takes a value of 1 when the Polity scale changes by at least three points (in either direction) from one year to the next; it does not distinguish the direction of regime change. Wright (2009) employs both a binary indicator of democratic transition as well as an indicator for a positive 3-point change in the Polity combined score. Djankov, Montalvo and Reynal-Querol (2008) use a change in the democracy variable from the Polity index, and Knack (2004) uses a 25-year change in the Freedom House score for a cross-section of countries. Ulfelder (2007) measures democracy by taking cut-points along two dimensions of the Polity scale, executive recruitment and political participation, while Haber and Menaldo (2011) use both changes in the combined Polity score and a binary indicator of democratic transition from Cheibub and Vreeland (2010).

Data on authoritarian regime change differ from measures derived from the Polity score in three ways. First, regime failure data mark transitions between distinct authoritarian regimes – defined as a change in the group of elite who rule – even when degree of ‘democraticness’ in the country does not change and the Polity score remains the same. Second, the Polity score sometimes marks institutional change within the tenure of an authoritarian regime as an episode of democratization. Third, movement along the combined Polity scale can conflate transitions to democracy and transitions to subsequent authoritarian regimes.³⁴

The Polity score measures levels of institutional checks on the executive (Gleditsch and Ward, 1997). In some cases the Polity score does not mark transitions from one authoritarian regime to another when both regimes have a similar level of institutional checks on the executive. For example, the end of Mobutu’s regime in the former Zaire (DRC) in 1997 marks no change in the Polity score (both 1996 and 1997 are coded 0) and is thus not captured in variables measuring changes in the Polity score. Similarly, in 1974 the dominant party in Niger lost power when a coup led by the military chief of staff imprisoned President Diouri (Higgott and Fuglestad, 1975). The military ruled until 1993. The Polity score does not change during this entire period, remaining at -7 until the early 1990s. When Iraq’s Saddam Hussein forced Baathist President Hassan al-Bakr to resign 1979, the Polity scale measures a two-point shift which does not meet the 3-point threshold for ‘regime change’. The transitions data from Geddes, Wright and Frantz (2012) mark the departure of Mobutu and the coup against Diouri as transitions between dictatorships.³⁵ In both the Geddes and CGV data, Iraq 1979 is marked as a regime change within the spell of non-democratic rule. These examples illustrate that the Polity score, while a useful measure of changes

³³Ross (2009) uses a binary variable for democratization from Cheibub & Gandhi (2004).

³⁴As Herb (1999, 11) points out, oil wealth does not necessarily reduce the likelihood of transitions to subsequent authoritarian regimes. In unreported results, I use transition to subsequent dictatorship (instead of transitions to democracy) as the dependent variable and find that oil (either by itself or conditional on crisis) is not correlated with the likelihood of these types of transitions.

³⁵Because the present paper examines transitions to democracy, it treats transitions to a subsequent dictatorship as right-censored observations.

in institutional checks on the executive, does not capture some transitions between authoritarian regimes. If we think that *regime change* or *authoritarian instability* entails a change in the group of elites that rule a country, then the Polity score and measures derived from it, leave out some transitions within the spell of non-democratic rule.

Second, an increase in the Polity score during the tenure of an authoritarian regime, which might reflect the legalization of opposition parties or the relative absence of violence during an authoritarian election, is sometimes marked as a regime change and even an episode of democratization. For example, Mobutu legalized parties and held a national conference in 1992 (Schatzberg, 1997) – an event coded as an 8-point increase in the Polity score, and thus coded as a regime change for measures marking 3-point Polity change. Mobutu was forced from power five years after the national conference, never having opened up power to the opposition (Prunier, 2009). In 1997, the Iranian regime selected a moderate (Khatami) from a slate of candidates approved by the Guardian Council in a relatively peaceful election. The Polity score increases from -6 to 3, and the Polity democracy score from 0 to 4, meaning this event is coded as a regime change and a democratization episode using Polity. While shifts in the Polity score may reflect changes in the institutional checks on the executive, Zaire 1992 and Iran 1997 do not constitute regime change, insofar as the main group of elite remained in power. To put these numbers in perspective, Chile’s democratic transition in 1989 is coded as an increase in the Polity scale from -1 to 8. Using a change variable from the Polity scale thus treats Chile 1989, Zaire 1992, and Iran 1997 as roughly equivalent episodes of democratization, though only the first of these is a transition to democracy.

Third, the Polity *durable* variable marks both positive and negative movements along the Polity scale and thus provides little purchase on questions of democratization. Even if one were to separate the positive changes in the Polity scale from the negative changes, the *durable* variable still marks some transitions to a subsequent authoritarian regime as a an episode of democratization. The 1979 Iranian Revolution is coded as a 10-point increase in the combined Polity scale, roughly the same size increase as the 1989 democratization in Chile (+9). Thus a variable that looks at changes in the Polity combined scale or the direction change using the *durable* variable would mark both Chile 1989 and Iran 1979 as equivalent episodes of democratization, when the latter might more accurately be characterized as a transition between two separate authoritarian regimes.

Finally, the Polity *durable* variable may not be particularly useful for assessing regime stability – at least when we define a *regime* as a group of elite who rule and select the leader – because it contains both Type I and Type II errors. For example, it can mark institutional change within the duration of an autocratic regime as the equivalent of regime failure if the institutional change increases or decreases the Polity score by 3 points (Type I), while failing to capture some transitions between groups of non-democratic elites where the Polity scale does not change (Type II). As noted, Polity *durable* marks Zaire 1992 as authoritarian instability when in fact the National Sovereign Conference was part of a strategy to stay in power amidst the new international context of the post-Cold War period (Type I). Polity *durable* then codes Zaire 1997 as authoritarian stability when in fact a transition between two authoritarian regimes took place (Type II).³⁶ While the

³⁶Other examples where Polity *durable* marks a 3-point change in the Polity scale during the lifetime of a non-democratic regime as regime change/failure include: Brazil 1974; Egypt 2005; Jordan 1989; Mexico 1977 and 1988;

two binary indicators of democratic transition used in this paper differ slightly on the exact dates of democratic transition, they do not include these institutional changes as democratic transitions unless a new group of leaders gains access to power as a result.

Data on transitions to democracy

The CGV and Geddes data differ slightly in the universe of cases they cover and the exact date for coding a transition to democracy. The Geddes data marks transitions when power transfers from one group to another, while the CGV data date transitions from the change in the institutional rules under which a transition in power takes place. For example, because the electoral rules in Kenya that led to incumbent turnover in the 2002 were in place in 1998, the CGV data code 1998 as the transition to democracy. The Geddes data code the transition in 2002.

While there are slight differences in the universes and exact dates of democratic transitions, the main distinction between the CGV and Geddes data on democratic transitions occur in the coding of regime *duration*. In the CGV data, a *regime* and its duration is defined by how long the country has been a non-democracy and the identity of the leader (civilian, monarchy, military). Thus the regime duration variable identifies how long the country has been ruled by a non-democratic regime with a leader of a particular type. The Geddes data attempt to code regimes and their duration by the group of elite who have access to power and can select the leader. When a country remains non-democratic with the same type of leader, the CGV data count this as one regime spell. In the Geddes data, there can be successive authoritarian regimes with the same type of leader but different groups of elites who rule.

For example, the military coup in Algeria in 1992 is coded as a transition between successive dictatorships in the Geddes data while Benjedid's time in power and the post-coup military rule (Zeroual) are grouped together as one regime in the CGV data because both leaders were one-time members of the military. The regime duration variable in the Geddes data therefore starts over, counting from 1 in 1993, while the CGV duration variable continues counting from 1966, which is the year when the first leader of the FLN regime who was from the military was selected (Boumediene).³⁷ When the Algerian regime selects a non-military leader, Bouteflika, in 2000, the CGV code a regime change (from military to civilian), while the Geddes data groups together the post-1992 military leader (Zeroual) and Bouteflika as one regime because both leaders were selected by the same group of elite.

In binary dependent variable models with controls for duration dependence there will be differences between these two data sets because the definition of a *regime* and hence its duration differ. This has implications not only for how we estimate and control for duration dependence in these models but also for non-proportional hazards (Box-Steffensmeier, Reiter and Zorn, 2003; Carter and Signorino, 2010).

Senegal 1978; Tanzania 1995; Uganda 1993 and 2005; and Zimbabwe 1999. Most of these examples are cases of institutional liberalization within a non-democratic regime, but do not constitute a fundamental change in the group of elite who rule and select the leader.

³⁷The Geddes data code Ben Alla, Boumediene, and Benjedid as part of the same FLN regime. The CGV data code Ben Alla as a separate regime because he was a civilian leader of the FLN.

Appendix B: Heterogeneity in the resource curse literature

One of Haber and Menaldo's (2011) critiques of the resource curse literature is that it fails to account for how resource wealth affects democracy over time. One way to do this, as they point out, is to control for all cross-country differences with country-fixed effects and then pool countries together to find an average within-country effect. They employ error-correction models with country-fixed effects when analyzing the Polity data, and estimate a conditional logit model when analyzing a binary dependent variable measuring transition to democracy (actually a Markov transition model). Because they analyze a long time series, the potential bias resulting from including fixed effects in a binary dependent variable model is small: Katz (2001) shows that for time-series data with $T > 16$ conditional logit is both unbiased and consistent.

However, using conditional logit to examine transitions to democracy forces the model to drop countries that never democratize, or in the Markov transition model, countries that never change states. This could potentially induce selection bias into the model (Beck and Katz, 2001; Ross, 2009). For example, oil-rich monarchies or countries such as China and Vietnam never experience a transition to democracy (up to 2010), and are thus excluded from a conditional logit.³⁸ One approach to address this issue might be to use a two-step Heckman model that accounts for sample selection by including a selection correction parameter in the second stage equation for democratic transition. However, in Monte Carlo simulations, Freedman and Sekhon (2010) find that the two-step solution for a binary dependent variable may induce more problems than it solves.

To assess how conditional logit affects the estimate of *Oil* in a model of democratic transitions, I examine whether reducing the sample to only those countries that (eventually) democratize introduces bias, using the CGV data on democratic transitions. The first model in Table B-1 examines oil in the full sample ($N=3841$); the second models selection into the group of countries that eventually experience a transition to democracy in the sample period. This model also includes a variable indicating whether a country was *Never Colonized* and whether it experienced a prior autocratic transition *Prior*. In a Heckman framework we might think of these as excluded variables, assumed to only affect the likelihood of transition through selection into the group of countries that eventually democratize. The third model uses only those observations in countries that eventually democratize, the restricted sample ($N=1455$), to examine whether *Never Colonized* and *Prior* predict democratization. The fourth model examines oil in the restricted sample; and the fifth estimates a conditional logit on the restricted sample.

The coefficient for *Oil* in the full sample (column 1) is negative and statistically significant, suggesting that oil reduces the likelihood of a democratic transition. This is consistent with Ross (2009) and other similar studies. The model in the second column shows that oil wealth also reduces the probability of being in the group of countries that experience democracy. Unsurprisingly, the distribution of oil in countries that democratize and those that do not is quite different, suggesting the potential for selection bias. The model in the second column also shows that *Never Colonized* and *Prior* are very good predictors of being in the sample of countries that democratize at some

³⁸The issue of no-variance across time in oil-rich monarchies also crops up in OLS models with the combined Polity score as a dependent variable. Because the Polity scale is necessarily truncated, some oil-rich monarchies do not experience any change over time in these models as well.

point. The model of democratic transition in the third column shows that these two variables are not very good at predicting democratization, conditional on covariates, in the restricted sample.

The model in the fourth column omits these two variables for the restricted sample and estimates a logit without unit effects. The *Oil* coefficient is smaller and not statistically different from zero. This result suggests that among the group of countries that democratize, oil wealth is not associated with a decreased likelihood of democratization. This result is consistent with Haber and Menaldo's (2011) analysis of a Markov transition model with conditional logit, though their positive results for oil wealth are statistically significant. Finally, introducing unit effects in column 5 substantially increases the standard errors for *Oil* and reverses the sign on *Oil*; it is now negative and marginally statistically significant.

One interpretation of these results is that oil wealth reduces the likelihood of being in the group of countries that democratize at some point (column 2), but have little effect on when countries democratize (column 4). This interpretation is consistent with Colgan's (2011) finding that oil prices are not correlated with democracy and with Smith's (2004) analysis that booms and busts have little effect on the Polity *durable* variable. That oil wealth may not explain when a country democratizes would also be consistent with Haber and Menaldo's (2011) analysis, though they examine a much longer time series. These results also indicate that introducing unit effects via conditional logit, as Haber and Menaldo do in the Appendix to their paper, actually makes the *Oil* coefficient more negative. This result suggests that the positive effect for *Oil* in a conditional logit model in Haber and Menaldo's analysis may stem from restricting the sample to only countries that eventually democratize and not from the inclusion of unit effects. That said, these results are not directly comparable to Haber and Menaldo's because they examine a much longer time series.

Another approach to unit heterogeneity in the oil curse literature is to think about this issue from a split-population perspective (Poirier, 1980; Meng and Schmidt, 1985; Svolik, 2008; Xiang, 2010). We might think that one group of countries is unlikely to ever democratize – that is, they have a very low (close to zero) latent probability of transitioning to a democracy – while a second group has a positive latent probability of democratizing. When we estimate a typical binary dependent variable, we cannot separate out the observed zeros in the dependent variable that indicate ‘never democratize’ (the first group of countries) and observed zeros that indicate a positive latent probability (the second group). Split population models can address this issue by simultaneously and separately estimating the probability of never democratizing and probability of democratizing at some point.

As the results in the first three columns of Table B-1 suggest, oil rents decrease the probability of democratic transition in a sample of all observations, but may not affect this probability in a sample of countries that eventually democratize. The conditional logit forces the model to separate the two groups, assuming that the probability of democratizing in countries that have yet to experience a transition is zero (with certainty). The split-population model is an attempt to estimate the latent probability of never democratizing and the latent probability of not yet democratizing (relative to democratizing).

To do this, I estimate a bivariate probit with partial observability similar to Xiang (2010). To

Table B-1: Heterogeneity and the oil curse

Model	logit	logit	logit	logit	conditional logit	conditional logit
DV	Dem (1)	Restricted Sample (2)	Dem (3)	Dem (4)	Dem (5)	Dem (6)
Oil	-0.184** (0.06)	-0.158+ (0.09)	-0.091 (0.07)	0.051 (0.07)	-0.365+ (0.21)	-0.243 (0.22)
Log GDP pc	0.531** (0.20)	-0.018 (0.30)	0.655** (0.19)			
ln(Crisis)	0.157 (0.19)	-0.275* (0.13)	0.368+ (0.19)	0.341+ (0.19)	0.483* (0.21)	0.648** (0.23)
ln(Boom)	-0.330 (0.21)	-0.206 (0.13)	-0.248 (0.24)	-0.137 (0.25)	-0.349 (0.28)	-0.421 (0.28)
Prior		1.983** (0.63)	0.377+ (0.23)			
No colony		1.281* (0.55)	-0.421 (0.33)			
ln(Crisis)*Oil						-0.158+ (0.09)
Duration	-0.114+ (0.06)	-0.106** (0.03)	0.007 (0.08)	0.004 (0.08)	0.125 (0.11)	0.124 (0.11)
Duration ²	0.004 (0.00)	0.006** (0.00)	-0.002 (0.00)	-0.002 (0.00)	-0.007 (0.01)	-0.007 (0.01)
Duration ³	-0.000 (0.00)	-0.000** (0.00)	0.000 (0.00)	0.000 (0.00)	0.000 (0.00)	0.000 (0.00)
Constant	-7.437** (1.42)	0.295 (2.09)	-8.023** (1.29)	-3.485** (0.68)		
Log likelihood	-346.2	-1945.6	-260.3	-267.2	-167.7	-166.1
NxT	3841	3841	1455	1455	1455	1455

+ p<0.10; * p<0.05; ** p<0.01. Dependent variable is a binary measure of transition to democracy from Cheibub, Gandhi, and Vreeland (2010). Calendar time polynomials (3) included in all specifications but not reported.

use Xiang's terminology, the relevance equation is an attempt to predict the likelihood of being in the first group – countries that are never at risk of democratizing. This equation includes *Oil*, *ln(GDPpc)*, *Never Colonized* and *Prior*. The second equation is the transition outcome equation which includes the following variables: *Oil*, *ln(GDPpc)*, *Post-Cold War*, regime *Duration*, *Crisis*, *Boom*, *Neighbor Democracy*, and regime *Duration*. I include *Never Colonized* and *Prior* in the relevance equation under the assumption they predict the likelihood of never democratizing but not the likelihood of when a country democratizes. The restricted sample probit model in column 3 of Table B-2 is an attempt to test this exclusion restriction.

Table B-2: Split population and the oil curse

	(1)	(2)	(3)	(4)
<u>Outcome</u>				
Oil	0.132 (0.36)	0.079+ (0.04)	0.148 (0.35)	0.049 (0.04)
Crisis	0.233 (0.76)	2.780* (1.38)	8.119** (3.14)	3.609 (2.88)
Crisis*Oil		-1.213* (0.54)		-1.812+ (1.10)
Log GDP pc	0.053 (0.44)	0.173 (0.11)	0.325 (0.31)	0.218 (0.18)
Post-1989	0.161 (0.19)	0.210* (0.09)	0.858** (0.28)	0.517+ (0.28)
Neighbor democracy	0.491 (0.67)	0.599** (0.23)	0.427 (0.63)	0.344 (0.35)
Boom	-4.149 (3.67)	-4.665* (1.90)	-5.994+ (3.49)	-6.188** (2.14)
Duration	-0.002 (0.00)	-0.002 (0.00)	-0.004 (0.01)	-0.000 (0.00)
Constant	-2.190 (3.77)	-3.235** (0.81)	-4.218 (2.91)	-3.477** (1.32)
<u>Relevance</u>				
Oil	-0.216 (0.32)	-0.143* (0.06)	-0.154 (0.14)	-0.088 (0.06)
Log GDP pc	-0.033 (0.74)	-0.270 (0.19)	-0.243 (0.44)	-0.538 (0.44)
Never Colonized	0.096 (0.25)	0.189 (0.13)	0.245 (0.24)	0.259 (0.29)
Prior	0.690 (1.85)	4.187** (0.43)	1.313 (1.35)	5.627** (2.13)
Constant	2.111 (5.85)	3.965** (1.42)	0.572 (2.75)	4.883* (2.02)
ρ	-0.988 (0.041)	-0.985 (0.020)	0.143 (1.11)	-0.952 (1.34)
Log likelihood	-316.7	-312.8	-323.6	-320.4
Observations	3841	3841	3588	3588

+ p<0.10; * p<0.05; ** p<0.01. Dependent variable in columns 1 and 2 is a binary measure of transition to democracy from Cheibub, Gandhi, and Vreeland (2010). In the last two columns the dependent variable is transition to democracy from the Geddes, Wright, and Frantz (2012) data.

The model in column 1 is a bivariate probit with partial observation without the interaction between *Oil* and *Crisis* in the outcome equation. The model in column 2 includes this interaction term. In column 2, the coefficient for *Oil* in the relevance equation is negative and larger in absolute size than the *Oil* coefficient in the full sample probit in column 1 of Table B-1. The *Oil* coefficient in the outcome equation is positive but not different from zero. This result suggests that oil reduces the likelihood of relevance but has a small positive *direct* effect on the probability of democratic transition. This result is consistent with the results in Table B-1, which suggested that *Oil* decreases the likelihood of transition in the full sample but not the restricted sample of countries that democratize at some point. The positive coefficient for *Oil* in the outcome equation (B-2.1) is also consistent with Haber and Menaldo's finding that oil wealth increases the likelihood of transition in their conditional fixed effects models. In column B-2.2, the main result for the interaction between *Oil* and *Crisis* persists, again suggesting that oil wealth blunts the democratizing effect of economic crises. The models in columns 3 and 4 are similar to those in columns 1 and 2, except they use the Geddes data on authoritarian regime transitions. The results are similar.

The bivariate probit models are sensitive to model specification. For example, the models do not converge when I include other control variables, such as *Duration* in the relevance equation or higher order polynomials for *Duration* in the outcome equation. This should not come as a surprise given the strong assumptions about bivariate normality in the errors and the related caution from Freedman and Sekhon (2010). Note also that the estimates of the correlation between the errors in the outcome and relevance equations are not consistent across model specifications that only differ by one variable. The exact model specification cannot be rigorously tested against alternatives and these results should be interpreted with extreme caution.

The results in this section suggest that oil rents affect selection into the group of countries that are at risk of democratizing. First we found that restricting the sample to only those countries that transition at some point decreases the *Oil* coefficient such that it is not statistically different from zero. Including fixed effects in the binary transition model of the resource curse moves the estimate of the effect of *Oil* closer to the full sample estimate. The result from the split-population models in B-2.1 and B-2.3 suggests that the negative effect of oil occurs in the relevance equation; this finding is consistent with the interpretation that oil rents reduce the probability that a country is ever at risk of democratizing. This evidence points towards the possibility that oil rents by themselves do not affect variation in the likelihood of democratization over time within countries. This conclusion, however, does not rule out the possibility that oil rents influence democratization through an indirect channel. Indeed, the main result for the purposes of this paper remains in the split-population models: oil rents condition the effect of economic crisis on the probability of democratic transition even after accounting for the possibility that the direct effect of oil rents is potentially positive in the outcome equation.

Appendix C: Stockpiled oil

This Appendix reports the results of the main specifications using a measure of stockpiled oil: the (natural log of the) cumulative value in constant dollars of oil rents in particular regime up to the year of observation (*Stock*). *Regime* is taken from Geddes et al. (2011). This means that the cumulative total starts from zero for the first year of every new regime (in power on January of the observation year), for example 1980 for Iran. As noted earlier this variable is highly correlated with current rents, in part because of the log. I also calculate a variable measuring change in rents from the previous year, and take the log of the absolute value, with negative values transformed back into negative value after the log transformation.

The first model in Table C-1 includes *Stock* but no interaction; the second includes the interaction between $\ln(\text{Crisis})$ and *Stock*. Columns 3 and 4 do the same for *Change*. Column 5 includes both sets of interaction terms. In models without an interaction term *Stock* has a negative and significant coefficient, while the *Change* coefficient is positive but small and not different from zero. The interaction models show that $\text{Stock} \times \text{Crisis}$ is negative and significant, as expected, but $\text{Change} \times \text{Crisis}$ is not. These results are consistent with those reported in the main text, which should not be surprising given the high correlation between *Stock* and *Oil*.

Table C-3: Stock of oil rents

	(1)	(2)	(3)	(4)	(5)
$\ln(\text{Crisis})$	0.097 (0.09)	0.186+ (0.10)	0.077 (0.09)	0.077 (0.09)	0.181+ (0.10)
$\ln(\text{Crisis}) \times \text{Stock}$		-0.033+ (0.02)			-0.033+ (0.02)
$\ln(\text{Crisis}) \times \Delta \text{Oil}$				-0.007 (0.12)	-0.081 (0.15)
ΔOil			0.043 (0.14)	0.047 (0.16)	0.057 (0.16)
<i>Stock</i>	-0.050* (0.02)	-0.029 (0.02)			-0.027 (0.02)
Log GDP pc	0.171+ (0.09)	0.185* (0.09)	0.030 (0.08)	0.030 (0.08)	0.148 (0.09)
$\ln(\text{Boom})$	-0.073 (0.09)	-0.082 (0.09)	-0.088 (0.10)	-0.088 (0.10)	-0.098 (0.09)
Duration	-0.015 (0.01)	-0.015 (0.01)	-0.014 (0.01)	-0.014 (0.01)	-0.014 (0.01)
Duration ²	0.000 (0.00)	0.000 (0.00)	0.000 (0.00)	0.000 (0.00)	0.000 (0.00)
Constant	-3.262** (0.70)	-3.362** (0.70)	-2.274** (0.63)	-2.274** (0.63)	-3.066** (0.69)
ρ	-1.698** (0.58)	-1.779** (0.60)	-1.820** (0.70)	-1.819** (0.70)	-2.098** (0.79)
Log likelihood	-354.0	-352.2	-342.0	-342.0	-337.5
Observations	3588	3588	3467	3467	3467

+ $p < 0.10$; * $p < 0.05$; ** $p < 0.01$; Random effects probit. Calendar time polynomials (3) included in all models, but are not reported.

Appendix D: Government spending and calorie consumption

The analysis of government spending uses data on government size from the Penn World Tables (PWT) because this data set has more extensive coverage of non-democracies than World Bank data. Past research on spending, however, uses data from the World Bank (WDI), so I check the results using this data as well. All variables (spending, oil, *Crisis*, *Boom*) are logged as a first attempt to mitigate the influence of outliers. In the ECMs, *Crisis* and *Boom* are differenced and lagged by one year – not the two-year moving average used in the analysis of transitions to democracy. The ECM's are estimated with country- and calendar year time trend polynomials (3); the standard errors are White heteroskedastic-consistent when tests suggest the presence of heteroskedasticity in the error variance. In all specifications I use the sample of dictatorships from Geddes et al. (2011), though the main results for spending are robust to using the CGV sample of non-democracies. The analysis of the calories data excludes Kuwait 1990-1993 due to the presence of large outliers in this data during the Iraqi invasion. Table D-1 and D-2 show the full results for the models in Table 4 of the main text.

Table D-3 reports the *Crisis* coefficient and interaction terms for a series of robustness tests for both the spending and calories models. The first five columns of the upper panel report results for crisis, oil, and spending. The first column includes the shock years (1973-74 and 1979-80); the second excludes these observations and is the main result reported in the manuscript. The third column excludes shock years and removes outliers, while the fourth column excludes shock years and influential observations. Finally, the fifth column assesses the model using WDI data on economic variables, including government spending (log constant dollars). In all these specifications, the main result persists.

The last five columns of the upper panel report results for crisis, aid, and spending – with the same set of robustness tests. Again the main result is found in all these specifications. The coefficient for *Crisis***Aid* in the last column is statistically significant at the 0.15 level.

The bottom panel of D-3 repeats these robustness tests for calorie consumption. In all tests for oil, the main result remains. For the aid tests, the main result is weaker once we exclude outliers and influential observations. However, in split sample analysis by time period, the main result is found for the post-1989 period with these same outliers and influential observations excluded. The result remains when the estimation omits country-fixed effects as well as when robust regression is employed (not reported).

Table D-1: Government spending

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Differenced variables</i>						
Crisis	-0.0367** (0.005)	-0.0469** (0.006)	-0.0366** (0.005)	-0.0112 (0.011)	-0.0227+ (0.013)	0.0079 (0.019)
Crisis*Oil		0.0047** (0.001)				
Crisis*Aid				-0.0081** (0.003)	-0.0018 (0.004)	-0.0160** (0.005)
Oil	-0.0038 (0.008)	-0.0037 (0.008)				
Aid			-0.0053 (0.007)	0.0001 (0.007)	-0.0059 (0.010)	0.0078 (0.012)
Conflict intensity	0.0149+ (0.009)	0.0158+ (0.009)	0.0181* (0.009)	0.0171+ (0.009)	0.0335** (0.012)	0.0282+ (0.014)
Boom	0.0112* (0.005)	0.0118* (0.005)	0.0127** (0.005)	0.0131** (0.005)	0.0077 (0.006)	0.0221** (0.008)
<i>Lagged variables</i>						
Crisis	-0.0344** (0.007)	-0.0468** (0.008)	-0.0335** (0.007)	0.0055 (0.014)	-0.0102 (0.018)	0.0217 (0.025)
Crisis*Oil		0.0052** (0.002)				
Crisis*Aid				-0.0127** (0.004)	-0.0065 (0.005)	-0.0200** (0.007)
Oil	0.0189** (0.004)	0.0163** (0.004)				
Aid			-0.0098+ (0.005)	-0.0017 (0.006)	-0.0059 (0.008)	0.0029 (0.014)
Conflict intensity	0.0051 (0.008)	0.0065 (0.008)	0.0023 (0.008)	0.0016 (0.008)	0.0264* (0.011)	0.0062 (0.015)
Boom	0.0199** (0.006)	0.0207** (0.006)	0.0233** (0.007)	0.0234** (0.006)	0.0080 (0.008)	0.0445** (0.011)
Spending	-0.1340** (0.010)	-0.1358** (0.010)	-0.1189** (0.010)	-0.1188** (0.010)	-0.1598** (0.014)	-0.2958** (0.024)
Constant	3.6592** (0.267)	3.7094** (0.267)	3.2582** (0.267)	3.2315** (0.267)	4.4405** (0.383)	11.7538** (2.558)
R ²	0.18	0.18	0.17	0.17	0.20	0.31
NxT	2517	2517	2397	2397	1417	980
N	98	98	92	92	78	81
T	1962-2006	1962-2006	1962-2007	1962-2007	1962-1989	1990-2007

+ p<0.10; * p<0.05; ** p<0.01; Dependent variable: $\Delta \ln(\text{GovtSpending})$; OLS with country-fixed effects and calendar year time trend polynomials (3) in all models (not reported). *Crisis* is $\ln((\text{Crisis} * 100) + 1)$ in all columns.

Table D-2: Calorie consumption

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Differenced variables</i>						
Crisis	-0.0054** (0.001)	-0.0066** (0.002)	-0.0050** (0.001)	-0.0010 (0.002)	-0.0001 (0.003)	0.0001 (0.004)
Crisis*Oil		0.0006* (0.000)				
Crisis*Aid				-0.0013+ (0.001)	-0.0012 (0.001)	-0.0016 (0.001)
Oil	0.0007 (0.002)	0.0007 (0.002)				
Aid			0.0023 (0.002)	0.0031+ (0.002)	0.0050* (0.002)	-0.0018 (0.003)
Conflict intensity	-0.0036 (0.002)	-0.0035 (0.002)	-0.0039+ (0.002)	-0.0038 (0.002)	-0.0026 (0.003)	-0.0060+ (0.003)
Boom	0.0010 (0.001)	0.0010 (0.001)	0.0010 (0.001)	0.0009 (0.001)	0.0021 (0.001)	-0.0013 (0.002)
<i>Lagged variables</i>						
Crisis	-0.0061** (0.002)	-0.0069** (0.002)	-0.0053** (0.002)	-0.0051 (0.003)	-0.0015 (0.004)	-0.0038 (0.006)
Crisis*Oil		0.0004 (0.000)				
Crisis*Aid				-0.0001 (0.001)	-0.0005 (0.001)	-0.0009 (0.002)
Oil	0.0015 (0.001)	0.0012 (0.001)				
Aid			0.0007 (0.001)	0.0007 (0.001)	0.0014 (0.002)	-0.0048 (0.003)
Conflict intensity	-0.0027 (0.002)	-0.0026 (0.002)	-0.0033+ (0.002)	-0.0031+ (0.002)	-0.0018 (0.003)	-0.0069* (0.003)
Boom	0.0002 (0.001)	0.0002 (0.001)	0.0003 (0.001)	0.0002 (0.002)	0.0003 (0.002)	0.0002 (0.003)
Calories	-0.0903** (0.009)	-0.0909** (0.009)	-0.0860** (0.009)	-0.0860** (0.009)	-0.1045** (0.014)	-0.2517** (0.034)
Constant	0.7124** (0.074)	0.7179** (0.073)	0.6780** (0.074)	0.6777** (0.074)	0.8282** (0.114)	2.5329** (0.753)
R ²	0.11	0.12	0.11	0.12	0.14	0.30
NxT	2328	2328	2184	2184	1598	842
N	93	93	86	86	73	76
T	1962-2005	1961-2007	1962-2005	1962-2005	1962-1989	1990-2005

+ p<0.10; * p<0.05; ** p<0.01; Dependent variables: $\Delta \ln(\text{Calories})$ OLS with country-fixed effects in all models (not reported). *Crisis* is $\ln((\text{Crisis} * 100) + 1)$ in all columns.

Table D-3: Robustness for spending and calories

Shock years removed	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Outliers removed	No	No	Yes	No	No	No	No	Yes	No	No
Influence removed	No	No	No	Yes	No	No	No	No	Yes	No
Economic data	PWT	PWT	PWT	PWT	WDI	PWT	PWT	PWT	PWT	WDI
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<i>Spending</i>										
Crisis	-0.04469** (0.0052)	-0.04688** (0.0057)	-0.02476** (0.0038)	-0.04699** (0.0057)	-0.04964** (0.0097)	-0.01614 (0.0100)	-0.01122 (0.0109)	-0.00457 (0.0072)	-0.01068 (0.0109)	-0.02086 (0.0144)
Crisis*Oil	0.00413** (0.0011)	0.00467** (0.0012)	0.00199* (0.0008)	0.00468** (0.0012)	0.00387* (0.0018)					
Crisis*Aid						-0.00622* (0.0028)	-0.00813** (0.0031)	-0.00511* (0.0020)	-0.00844** (0.0031)	-0.00667 (0.0047)
R ²	0.18	0.18	0.22	0.18	0.20	0.17	0.17	0.21	0.17	0.20
Obs	2825	2517	2454	2509	2361	2673	2397	2333	2389	2280
<i>Calorie consumption</i>										
Crisis	-0.00666** (0.0014)	-0.00664** (0.0015)	-0.00663** (0.0015)	-0.00437** (0.0010)	-0.00959** (0.0015)	-0.00101 (0.0023)	-0.00102 (0.0023)	-0.00247 (0.0022)	-0.00145 (0.0023)	-0.00229 (0.0027)
Crisis*Oil	0.00052* (0.0003)	0.00062* (0.0003)	0.00060* (0.0003)	0.00044* (0.0002)	0.00081** (0.0003)					
Crisis*Aid						-0.00135* (0.0007)	-0.00128+ (0.0007)	-0.00074 (0.0006)	-0.00112 (0.0007)	-0.00164* (0.0008)
R ²	0.12	0.12	0.12	0.16	0.14	0.11	0.12	0.12	0.11	0.13
Obs	2616	2328	2319	2201	2414	2440	2184	2172	2177	2288

+ p<0.10; * p<0.05; ** p<0.01. Control variables, constant, lagged variables, calendar year time trend polynomials, and country-fixed effects not reported. Models in columns (2) and (7) are the base models in the main text and in D-1 and D-2. See text for details.

Appendix E: Additional robustness tests

Table E-1: Robustness tests for Geddes data

	Maddison data (1-6)						WDI data (7-8)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Crisis	-6.598+	-5.879	-3.787	3.350*	3.209*	4.453**	-0.082+	0.035+
	(3.49)	(3.60)	(3.47)	(1.57)	(1.62)	(1.71)	(0.05)	(0.02)
Crisis*Aid	2.375*	2.282*	1.981+				0.029*	
	(1.04)	(1.07)	(1.04)				(0.01)	
Crisis*Oil				-1.828*	-1.871*	-1.609*		-0.013*
				(0.72)	(0.83)	(0.79)		(0.01)
Aid	-0.086	-0.091	0.020				-0.183**	
	(0.05)	(0.06)	(0.06)				(0.05)	
Oil				-0.032	-0.025	-0.042		-0.037
				(0.03)	(0.03)	(0.03)		(0.03)
Boom	-3.737+	-2.967	-5.626*	-3.406+	-2.982	-5.030*	-0.047*	-0.036+
	(1.98)	(1.94)	(2.43)	(1.97)	(1.92)	(2.27)	(0.02)	(0.02)
Neighbor dem	0.911**			0.764**				
	(0.27)			(0.25)				
Log GDP pc	-0.080	-0.130	0.021	0.102	0.009	0.174*	-0.041	0.122*
	(0.09)	(0.09)	(0.09)	(0.09)	(0.10)	(0.08)	(0.05)	(0.06)
Africa		-0.029			-0.061			
		(0.19)			(0.17)			
Latin America		0.744**			0.599**			
		(0.19)			(0.19)			
Europe		0.122			0.513*			
		(0.44)			(0.22)			
Party			-0.512			-0.332		
			(0.38)			(0.37)		
Military			0.851**			0.879**		
			(0.32)			(0.32)		
Monarchy			0.284			0.467		
			(0.41)			(0.42)		
Duration	-0.025**	-0.021*	0.013	-0.023**	-0.021**	0.020	-0.025**	-0.021*
	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Duration ²	0.000+	0.000	-0.000	0.000**	0.000*	-0.000	0.000+	0.000*
Constant	-1.344*	-0.862	-2.060**	-2.843**	-2.093**	-3.327**	-1.006*	-2.423**
	(0.61)	(0.72)	(0.78)	(0.65)	(0.70)	(0.71)	(0.47)	(0.51)
Log likelihood	-312.8	-304.8	-276.9	-347.2	-340.6	-311.2	-301.4	-325.2
Observations	3219	3219	3219	3558	3558	3558	2676	2779

+ p<0.10; * p<0.05; ** p<0.01; Probit with errors clustered on country. $Crisis = (Growth_{t-1,t-2} * -1)$, where all negative values (meaning positive economic growth) are set to zero. Aid is the moving average of the natural log of aid per capita over the last two calendar years. Calendar time polynomials (3) included in all models, but are not reported. Interaction between regime type categories and duration polynomials included but not reported in models 4 and 6.

Table E-2: Robustness tests for CGV data

	(1)	<u>RE</u> (2)	(3)	(4)	(5)	<u>RE</u> (6)	(7)	(8)
Crisis	0.447 (1.17)	-3.472 (4.00)	-5.051* (2.37)	-4.246+ (2.46)	0.575 (1.13)	3.652* (1.62)	2.467 (1.56)	3.090* (1.44)
Crisis*Aid		1.668 (1.17)	1.940** (0.73)	1.768* (0.71)				
Crisis*Oil						-1.528+ (0.81)	-1.295* (0.51)	-1.184* (0.49)
Aid	-0.060 (0.04)	-0.085 (0.07)	-0.070 (0.05)	-0.091* (0.05)				
Oil					-0.077** (0.03)	-0.087* (0.04)	-0.044+ (0.03)	-0.045 (0.03)
Log GDP pc	0.024 (0.07)	0.021 (0.10)	-0.042 (0.08)	0.090 (0.08)	0.233** (0.08)	0.329** (0.11)	0.129 (0.10)	0.285** (0.09)
Boom	-3.909+ (2.19)	-5.367+ (2.88)	-4.276+ (2.36)	-4.561* (2.32)	-4.963* (2.33)	-6.691* (2.88)	-4.790* (2.42)	-5.561* (2.49)
Neighbor Dem.			1.031** (0.35)				1.049** (0.34)	
Military				0.375** (0.11)				0.382** (0.10)
Monarchy				-0.405 (0.41)				-0.623+ (0.37)
Duration	-0.050+ (0.03)	-0.018 (0.03)	-0.038 (0.03)	-0.044 (0.03)	-0.057* (0.03)	-0.035 (0.03)	-0.047+ (0.03)	-0.051* (0.03)
Duration ²	0.002 (0.00)	0.001 (0.00)	0.002 (0.00)	0.002 (0.00)	0.002 (0.00)	0.001 (0.00)	0.001 (0.00)	0.002 (0.00)
Duration ²	-0.000 (0.00)							
Constant	-2.177** (0.49)	-2.562** (0.87)	-1.768** (0.54)	-2.665** (0.58)	-3.587** (0.58)	-4.663** (0.87)	-3.317** (0.62)	-4.097** (0.61)
Log likelihood	-328.9	-317.3	-319.8	-317.5	-345.3	-335.5	-334.4	-331.4
Observations	3531	3531	3531	3531	3841	3841	3841	3841

+ p<0.10; * p<0.05; ** p<0.01; Probit with errors clustered on country. $Crisis = (Growth_{t-1,t-2} * -1)$, where all negative values (meaning positive economic growth) are set to zero. Aid is the moving average of the natural log of aid per capita over the last two calendar years. Calendar time polynomials (3) included in all models, but are not reported.

Table E-3: Further transformations of aid

Aid	t-3, t-4	t-5,t-6	t-3/t-6	Binary	Sqrt, not log
	(1)	(2)	(3)	(4)	(5)
Crisis	-4.527 (3.08)	-2.405 (2.60)	-2.951 (2.69)	-0.055 (1.83)	-3.385+ (1.98)
Aid	-0.074 (0.05)	-0.053 (0.04)	-0.071 (0.05)	-0.183 (0.13)	-0.039+ (0.02)
Crisis*Aid	2.158* (0.90)	1.687* (0.81)	1.841* (0.82)	4.933* (2.43)	0.797* (0.31)
Log GDP pc	0.030 (0.08)	0.040 (0.08)	0.043 (0.08)	0.049 (0.08)	0.010 (0.08)
Boom	-3.716+ (2.12)	-3.070 (2.01)	-3.375 (2.10)	-3.623+ (2.13)	-3.730+ (1.94)
Duration	-0.030** (0.01)	-0.031** (0.01)	-0.032** (0.01)	-0.031** (0.01)	-0.028** (0.01)
Duration ²	0.000* (0.00)	0.000* (0.00)	0.000* (0.00)	0.000* (0.00)	0.000* (0.00)
Constant	-2.095** (0.64)	-2.162** (0.71)	-2.141** (0.70)	-2.307** (0.72)	-1.705** (0.57)
Log likelihood	-298.4	-295.4	-290.8	-290.9	-318.5
Observations	3096	2952	2943	2943	3219

+ p<0.10; * p<0.05; ** p<0.01; Probit with errors clustered on country. $Crisis = (Growth_{t-1,t-2} * -1)$, where all negative values (meaning positive economic growth) are set to zero. Calendar time polynomials (3) included in all models, but are not reported.

Table E-4: Further transformations of oil

Oil	Binary	t-3, t-4	Sqrt	Log	Log
	(1)	(2)	(3)	(4)	(5)
Crisis	2.573 (1.60)	3.959* (1.77)	2.503+ (1.50)	-5.348+ (3.15)	3.708* (1.60)
Oil	0.040 (0.13)	-0.040 (0.03)	-0.015** (0.01)	-0.080* (0.03)	-0.044 (0.03)
Crisis*Oil	-6.117* (2.51)	-1.102* (0.49)	-0.470 (0.30)		-1.938* (0.79)
Aid				-0.137* (0.06)	-0.095+ (0.06)
Crisis*Aid				2.130* (0.95)	
Log GDP pc	0.091 (0.07)	0.193* (0.09)	0.213** (0.08)	0.132 (0.09)	0.143+ (0.09)
Boom	-3.420+ (1.84)	-4.653* (2.33)	-3.962+ (2.06)	-4.217* (2.13)	-4.497* (2.14)
Duration	-0.023** (0.01)	-0.024** (0.01)	-0.025** (0.01)	-0.027** (0.01)	-0.026** (0.01)
Duration ²	0.000* (0.00)	0.000* (0.00)	0.000** (0.00)	0.000* (0.00)	0.000+ (0.00)
Constant	-2.525** (0.55)	-3.244** (0.79)	-3.347** (0.61)	-2.288** (0.64)	-2.468** (0.64)
Log likelihood	-361.7	-309.2	-348.9	-303.2	-301.4
Observations	3620	2975	3588	3162	3162

+ $p < 0.10$; * $p < 0.05$; ** $p < 0.01$; Probit with errors clustered on country. $Crisis = (Growth_{t-1,t-2} * -1)$, where all negative values (meaning positive economic growth) are set to zero. Calendar time polynomials (3) included in all models, but are not reported.

Table E-5: Further types of aid

Aid	Bi-lateral grant	Bi-lateral loan	Multi-lateral grant	Multi-lateral loan
	(1)	(2)	(3)	(4)
ln(Crisis)	-0.243+ (0.14)	0.035 (0.09)	-0.087 (0.14)	-0.060 (0.10)
ln(Crisis)*Aid	0.108* (0.04)	0.014 (0.05)	0.075 (0.06)	0.091+ (0.05)
Aid	-0.087 (0.05)	-0.057 (0.06)	-0.160* (0.08)	-0.048 (0.06)
Log GDP pc	0.037 (0.08)	0.030 (0.08)	-0.012 (0.08)	0.049 (0.08)
ln(Boom)	-4.614* (2.29)	-4.159+ (2.27)	-4.348+ (2.34)	-4.305+ (2.21)
Duration	-0.024** (0.01)	-0.023* (0.01)	-0.023* (0.01)	-0.024** (0.01)
Duration ²	0.000 (0.00)	0.000 (0.00)	0.000 (0.00)	0.000 (0.00)
Constant	-1.706** (0.62)	-1.867** (0.60)	-1.581** (0.61)	-2.092** (0.61)
Log likelihood	-304.8	-306.6	-300.6	-305.4
Observations	3080	3081	3092	3094

+ p<0.10; * p<0.05; ** p<0.01; Probit with errors clustered on country.
Crisis = $\ln(1 + 100 * Crisis)$ Calendar time polynomials (3) included
in all models, but are not reported.